THE CAMBRIDGE
HISTORY OF

# SCIENCE

VOLUME 6

# THE MODERN BIOLOGICAL AND EARTH SCIENCES

EDITED BY
PETER J. BOWLER
JOHN V. PICKSTONE

THE CAMBRIDGE HISTORY OF SCIENCE

VOLUME 6

*The Modern Biological and Earth Sciences*

This volume in the highly respected Cambridge History of Science is devoted to the history of the life and earth sciences since 1800. It provides comprehensive and authoritative surveys of historical thinking on major developments in these areas of science, on the social and cultural milieus in which the knowledge was generated, and on the wider impact of the major theoretical and practical innovations. The chapters were written by acknowledged experts who provide concise accounts of the latest historical thinking coupled with guides to the most important recent literature. In addition to histories of traditional sciences, the volume covers the emergence of newer disciplines such as genetics, biochemistry, and geophysics. The interaction of scientific techniques with their practical applications in areas such as medicine is a major focus of the book, as is its coverage of controversial areas such as science and religion as well as environmentalism.

Peter J. Bowler is Professor of the History of Science at Queen's University in Belfast. He was president of the British Society for the History of Science from 2004 to 2006 and is a member of the Royal Irish Academy and a Fellow of the British Academy and the American Association for the Advancement of Science. He is the author of numerous books, including *Charles Darwin: The Man and His Influence*, published by Cambridge in 1996.

John V. Pickstone is Wellcome Research Professor at Manchester University, where he founded the Centre for the History of Science, Technology and Medicine and directed it until 2002. He has published numerous books and articles, including *New Ways of Knowing: A New History of Science, Technology and Medicine* (2000) and *Surgeons, Manufacturers and Patients: A Transatlantic History of the Total Hip Replacement* (2007), coauthored with Julie Anderson and Francis Neary.

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David C. Lindberg is Hilldale Professor Emeritus of the History of Science and past director of the Institute for Research in the Humanities at the University of Wisconsin–Madison. He has written or edited a dozen books on topics in the history of medieval and early modern science, including *The Beginnings of Western Science* (1992). He and Ronald L. Numbers have previously coedited *God and Nature: Historical Essays on the Encounter between Christianity and Science* (1986) and *When Science and Christianity Meet* (2003). A Fellow of the American Academy of Arts and Sciences, he has been a recipient of the Sarton Medal of the History of Science Society, of which he is also past president (1994–5).

Ronald L. Numbers is Hilldale Professor of the History of Science and Medicine at the University of Wisconsin–Madison, where he has taught since 1974. A specialist in the history of science and medicine in the United States, he has written or edited more than two dozen books, including *The Creationists* (1992, 2006), *Science and Christianity in Pulpit and Pew* (2007), and the forthcoming *Science and the Americans*. A Fellow of the American Academy of Arts and Sciences and a former editor of *Isis*, the flagship journal of the history of science, he has served as the president of the American Society of Church History (1999–2000), the History of Science Society (2000–1), and the International Union of History and Philosophy of Science/Division of History of Science and Technology (2005–9).

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## *The Modern Biological and Earth Sciences*

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PETER J. BOWLER

JOHN V. PICKSTONE

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# NOTES ON CONTRIBUTORS

PASCAL ACOT undertook research on the history of scientific ecology at the CNRS (Centre National de la Recherche Scientifique) in France. In 1998, he directed the writing of a collective book, *The European Origins of Scientific Ecology* (2 vols. plus CD-ROM). He also wrote (with collaborators) the eleventh volume of the *Biosphera* encyclopedia, translated in the United States as *The Concept of Biosphere*. His most recent book is *Histoire du Climat*.

DAVID E. ALLEN is a research associate of the Wellcome Trust Centre for the History of Medicine at University College London and a scientific associate of London's Natural History Museum. He holds a doctorate in the history and philosophy of science from Cambridge University and was a research administrator before retirement.

OLGA AMSTERDAMSKA teaches Social Studies of Science and Medicine at the University of Amsterdam. Her research focuses on the development of the biomedical sciences, history of epidemiology, and the interactions between the laboratory, the clinic, and public health in twentieth-century medicine. She is the former editor of *Science, Technology, and Human Values* and one of the editors of the *Handbook of Science and Technology Studies* (2007).

KEITH R. BENSON is a historian of biology with a special interest in the history of biology in North America, the history of the marine sciences, the history of developmental biology, and biology and society. He is Professor of History at the University of British Columbia. He is coeditor of *The Development of American Biology* and *The American Expansion of Biology*, editor of the recent translation of Jacques Roger's classic book *The Life Sciences in Eighteenth-Century France*, and coeditor (with Fritz Rehbock) of *The Pacific and Beyond*, a multiauthored history of oceanography. He is currently treasurer of the International Society of the History, Philosophy, and Social Studies of Biology (ISHPSSB) and editor-in-chief of *History and Philosophy of the Life Sciences*.



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STEPHEN BOCKING is Professor of the History of Science and Environmental History at Trent University in Peterborough, Ontario. His recent books include *Nature's Experts: Science, Politics, and the Environment* (2004); *Biodiversity in Canada: Ecology, Ideas, and Action* (2000); and *Ecologists and Environmental Politics: A History of Contemporary Ecology* (1997).

PETER J. BOWLER is Professor of the History of Science at Queen's University in Belfast. He was president of the British Society for the History of Science from 2004 to 2006 and is a member of the Royal Irish Academy and a Fellow of the British Academy and the American Association for the Advancement of Science. He is the author of numerous books, including *Charles Darwin: The Man and His Influence*, published by Cambridge in 1996.

ROBERT BUD is Principal Curator of Medicine at the Science Museum, London. He led the museum's major online projects, *Ingenious* and *Making the Modern World*, and he is involved with its current medical site, launching in 2009. He also holds the honorary positions of Associated Scholar, Department of History and Philosophy of Science, Cambridge; Honorary Senior Research Fellow, Department of Science and Technology Studies, University College London; and Honorary Research Fellow, Department of History, Classics and Archaeology, Birkbeck College. His books include *The Uses of Life: A History of Biotechnology* (1994) and *Penicillin: Triumph and Tragedy* (2007).

RICHARD M. BURIAN completed a PhD in philosophy at the University of Pittsburgh and works on the interactions among development, evolution, and genetics from Darwin forward. A former head of the Philosophy Department and director of the STS Program at Virginia Polytechnic Institute and State University and a past president of the International Society of the History, Philosophy, and Social Studies of Biology, he recently published *Epistemological Essays on Development, Genetics, and Evolution: Selected Essays* (2005).

EUGENE CITTADINO has taught the history of science and medicine, environmental history, and science and technology studies at Harvard University, Brandeis University, the University of California, the University of Wisconsin, and New York University. His main research interests are in the history and social relations of the life sciences, particularly ecology, botany, and evolutionary biology.

MARIO A. DI GREGORIO is Professor of the History of Science at the University of L'Aquila, Italy, and Visiting Professor at the University of Cape Town, South Africa. He was formerly a Research Fellow at Darwin College and Affiliated Lecturer, Faculty of History, at the University of Cambridge, and Visiting Professor at the University of California, Los Angeles. He is the author of *T. H. Huxley's Place in Natural Science* (1984), *Charles Darwin's Marginalia* (with N. W. Gill) (1990), and *From Here to Eternity: Ernst Haeckel and Scientific Faith* (2005). He is also an opera singer (bass-baritone) and actor.

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HENRY FRANKEL is Professor of Philosophy at the University of Missouri at Kansas City. He became interested in the controversy of continental drift because of the philosophical issues surrounding theory choice, and he is now equally interested in purely historical aspects of the controversy. With support of the National Science Foundation (USA); the National Endowment for the Humanities (USA); the American Philosophical Society; the Linda Hall Library, Kansas City, Missouri; the University of Missouri Research Board; and his home institution, he is completing a three-volume work, *The Controversy over Continental Drift*, for Cambridge University Press.

JEAN-PAUL GAUDILLIÈRE is historian of science and medicine and senior researcher at INSERM (Institut National de la Santé et de la Recherche Médicale). He has worked on the transformation of biological and medical research in the twentieth century and is currently writing a history of biological therapies. He has published *Inventer la biomédecine* (Inventing Medicine) (2002; English translation forthcoming) and *La médecine et les sciences: XiXème–Xxème siècles* (Medicine and the Sciences: Nineteenth and Twentieth Centuries) (2006). He recently edited the special issue of *Studies in History and Philosophy of the Biological and Biomedical Sciences* on drug trajectories (2005) and a special issue of *History of Technology* on "How Drugs Became Patentable" (June 2008).

MOTT T. GREENE is a historian of earth sciences and Director of the Program in Science, Technology and Society at the University of Puget Sound. He is the author of *Geology in the Nineteenth Century* (1982) and former editor of the journal *Earth Sciences History*.

ANNE HARRINGTON is Professor and Chair of the Department of the History of Science at Harvard University and Visiting Professor for Medical History at the London School of Economics, where she coedits a new journal called *Biosocieties*. For six years, she codirected Harvard's Mind, Brain, and Behavior Initiative (www.mbb.harvard.edu). She is the author of *Medicine, Mind and the Double Brain* (1987); *Reenchanted Science* (1997); and *The Cure Within: A History of Mind–Body Medicine*. Her edited collections include *The Placebo Effect* (1997), *Visions of Compassion* (2000), and *The Dalai Lama at MIT* (2006). She is currently working on a new synthetic history of psychiatry and on the meanings of new interest in literature narrating what it "feels like" to live inside a broken or disordered brain.

JONATHAN HARWOOD is Professor of the History of Science and Technology at the Centre for History of Science, Technology and Medicine at the University of Manchester. His interests include the history of biology from 1870 to 1945 (especially genetics), the social history of the German professoriate, and the history of the agricultural sciences. His most recent book is *Technology's Dilemma: Agricultural Colleges between Science and Practice in Germany, 1860–1934* (2005), and he is currently writing a book on the rise and fall of "peasant-friendly" plant breeding.

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JONATHAN HODGE has written historically on Buffon and Lamarck; Fisher and Wright; and Lyell, Darwin, and Wallace; as well as philosophically on natural selection theory. He is now coediting a second edition of *The Cambridge Companion to Darwin* and writing monographs on Lyell and on Charles Darwin's early years.

NICK HOPWOOD is Senior Lecturer in the Department of History and Philosophy of Science at the University of Cambridge, where he teaches history of modern medicine and biology and is researching the visual culture of embryology. A former developmental biologist, he is the author of *Embryos in Wax: Models from the Ziegler Studio* (2002) and coeditor of *Models: The Third Dimension of Science* (2004).

MARK JACKSON is Professor of the History of Medicine and Director of the Centre for Medical History at the University of Exeter. After qualifying initially in medicine in 1985, he has pursued research on the social history of infanticide, the history of feeblemindedness, and the history of allergic diseases. He is currently working on the history of stress, with a particular focus on Hans Selye (1907–1982). His books include *New-Born Child Murder: Women, Illegitimacy and the Courts in Eighteenth-Century England* (1996); *The Borderland of Imbecility: Medicine, Society and the Fabrication of the Feeble Mind in Late Victorian and Edwardian England* (2000); and *Allergy: The History of a Modern Malady* (2006).

RICHARD L. KREMER is Associate Professor of History at Dartmouth College. He currently studies university laboratories, experimental practice, and scientific instruments and their makers. His published works include *Study, Measure, Experiment: Stories of Scientific Instruments at Dartmouth College* (2005); *Letters of Hermann von Helmholtz to His Wife* (1990); and numerous articles on nineteenth-century German universities.

SUSAN C. LAWRENCE is Associate Professor of History at the University of Nebraska at Lincoln. Her book *Charitable Knowledge: Hospital Pupils and Practitioners in Eighteenth-Century London* was published in 1996. She is currently working on a book on the history of human dissection in Anglo-American medical education from the eighteenth century to the present.

SUSAN E. LEDERER is the Robert Turrell Professor of Medical History and Bioethics and the Chair of the Department of Medical History and Bioethics at the University of Wisconsin School of Medicine and Public Health. A historian of American medicine and medical ethics, she is the author of *Subjected to Science: Human Experimentation in America before the Second World War* (1995) and served as a member of President Clinton's Advisory Committee on Human Radiation Experiments. Her most recent book is *Flesh and Blood: A Cultural History of Transplantation and Transfusion in Twentieth-Century America* (2008).

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PAUL LUCIER is a historian of the earth sciences and the environment. He is the author of several articles and a book, *Scientists and Swindlers: Consulting on Coal and Oil in America, 1820–1890* (2008). He is currently working on a history of gold and silver mining in the American West.

ROY MACLEOD is Professor Emeritus of History at the University of Sydney. He was educated at Harvard University and the University of Cambridge and has written extensively on the social and political history of science, medicine, and technology. He was the founding coeditor of *Social Studies of Science* and is currently editor of *Minerva*.

RUSSELL C. MAULITZ is a professor at Drexel University College of Medicine and Managing Medical Information Scientist at CHI Systems, Inc., both in Philadelphia. At Drexel, he teaches medical informatics and, through its Division of Medical Humanities, gives occasional medical history lectures. His publications concern modern clinical medicine and pathology in the United States and Western Europe. *Morbid Appearances*, his monograph on nineteenth-century pathology, was reissued in paperback in 2002.

JAMES MOORE is a historian of science at the Open University. He has taught at Cambridge, Harvard, Notre Dame, and McMaster universities, and his books include *The Post-Darwinian Controversies* (1979), *The Darwin Legend* (1994), and (with Adrian Desmond) *Darwin* (1991). Moore is working on a biography of Alfred Russel Wallace.

DAVID OLDROYD is an honorary visiting professor in the School of History and Philosophy of Science at the University of New South Wales in Sydney, from which he retired from his chair in 1996. His main interests are (of late) in the area of the history of geology, in which he has authored several books, including *Thinking about the Earth* (translated into German, Turkish, and Chinese); *Earth, Water, Ice and Fire: Two Hundred Years of Geological Research in the English Lake District; The Iconography of the Lisbon Earthquake* (with J. Kozak); and *Earth Cycles: A Historical Approach.* He has served as secretary-general of the International Commission on the History of Geological Sciences for eight years and has received awards for his geohistorical work from the Geological Society of London and the Geological Society of America.

JOHN V. PICKSTONE has worked at Manchester University since 1974, and in 1986 he founded the Wellcome Unit and the Centre for the History of Science, Technology and Medicine. Since 2002, he has been the Wellcome Research Professor. His early research was on the history of physiology, medicine in northwest England, and medical innovations. His recent books include *Ways of Knowing: A New History of Science, Technology and Medicine* (2000); *Companion to Medicine in the Twentieth Century* (edited with Roger Cooter, 2002); and *Surgeons, Manufacturers and Patients: A Transatlantic*

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*History of the Total Hip Replacement* (with Julie Anderson and Francis Neary, 2007).

RONALD RAINGER is Professor of History at Texas Tech University, where he teaches the history of science and technology. For the past several years he has worked on the history of oceanography, but recently he has returned to his earlier research on the history of paleontology. He is currently working on a project on paleontology in America.

JEFFREY C. SCHANK is an associate professor in the Department of Psychology at the University of California, Davis. He has a PhD from the University of Chicago and did postdoctoral research on animal behavior at Indiana University.

THOMAS SÖDERQVIST is Professor in History of Medicine and Director of Medical Museion, University of Copenhagen. His publications on the history of twentieth-century life sciences include *The Ecologists* (1986) and *The Historiography of Contemporary Science, Technology and Medicine* (as coeditor, 2006), and his works on scientific biography include *Science as Autobiography* (2003) and *The History and Poetics of Scientific Biography* (as editor, 2007). His present research interest is the interface between the historiography of science and the material culture of recent biomedicine.

CRAIG STILLWELL teaches science and technology studies at Southern Oregon University. His research includes the history of biology and medicine, with an emphasis on immunology.

JOHN P. SWANN received his PhD in the history of science and in pharmacy from the University of Wisconsin. Before assuming his present position as FDA Historian in 1989, he was a postdoctoral Fellow at the Smithsonian Institution and held a research post at the University of Texas Medical Branch. His publications have focused on the history of drugs, biomedical research, the pharmaceutical industry, and regulatory history. He is currently at work on a book on the history of diet pills and obesity.

CHARLES TWARDY has a PhD in history and philosophy of science (and cognitive science) from Indiana University. He has worked on causal and probabilistic reasoning as a postdoctoral researcher at Monash University and at two small companies. He has published on causation, teaching critical thinking, algorithmic compressibility, and Mayan astronomy.

MARY P. WINSOR studied at Harvard and Yale universities and worked summers at Woods Hole and the Museum of Comparative Zoology. She joined the faculty of the University of Toronto in 1969 and is now Professor Emeritus. She is the author of *Starfish, Jellyfish, and the Order of Life* and *Reading the Shape of Nature*.

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MICHAEL WORBOYS is Director of the Centre for the History of Science, Technology and Medicine and the Wellcome Unit for the History of Medicine at the University of Manchester. He has worked on the history of British colonial science, tropical medicine, and germ theories of disease in the period 1860–1920. He also has a long-standing interest in the history of infectious disease, including work on tuberculosis, gonorrhea, and the control of smallpox in India. His ongoing work includes projects on rabies in Britain (with Neil Pemberton), fungal diseases in the twentieth century (with Aya Homei), and the history of bacteriological laboratories in Britain from 1890 to 1920.

DORIS T. ZALLEN holds a PhD from Harvard University and is Professor of Science and Technology Studies at Virginia Polytechnic Institute and State University. A former laboratory scientist, she now studies the social, ethical, and policy issues associated with advances in genetic medicine. She is the author of *Does It Run in the Family? A Consumer's Guide to DNA Testing for Genetic Disorders* (1997).

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# GENERAL EDITORS' PREFACE

The idea for *The Cambridge History of Science* originated with Alex Holzman, former editor for the history of science at Cambridge University Press. In 1993, he invited us to submit a proposal for a multivolume history of science that would join the distinguished series of Cambridge histories, launched nearly a century ago with the publication of Lord Acton's fourteen-volume *Cambridge Modern History* (1902–12). Convinced of the need for a comprehensive history of science and believing that the time was auspicious, we accepted the invitation.

Although reflections on the development of what we call "science" date back to antiquity, the history of science did not emerge as a distinctive field of scholarship until well into the twentieth century. In 1912, the Belgian scientist-historian George Sarton (1884–1956), who contributed more than any other single person to the institutionalization of the history of science, began publishing *Isis*, an international review devoted to the history of science and its cultural influences. Twelve years later, he helped to create the History of Science Society, which by the end of the century had attracted some 4,000 individual and institutional members. In 1941, the University of Wisconsin established a department of the history of science, the first of dozens of such programs to appear worldwide.

Since the days of Sarton, historians of science have produced a small library of monographs and essays, but they have generally shied away from writing and editing broad surveys. Sarton himself, inspired in part by the Cambridge histories, planned to produce an eight-volume *History of Science*, but he completed only the first two installments (1952, 1959), which ended with the birth of Christianity. His mammoth three-volume *Introduction to the History of Science* (1927–48), more a reference work than a narrative history, never got beyond the Middle Ages. The closest predecessor to the *Cambridge History of Science* is the three-volume (four-book) *Histoire Générale des Sciences* (1957–64), edited by René Taton, which appeared in an English translation under the title *General History of the Sciences* (1963–4). Edited just before the late-century



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boom in the history of science, the Taton set quickly became dated. During the 1990s, Roy Porter began editing the very useful Fontana History of Science (published in the United States as the Norton History of Science), with volumes devoted to a single discipline and written by a single author.

The *Cambridge History of Science* comprises eight volumes, the first four arranged chronologically from antiquity through the eighteenth century and the latter four organized thematically and covering the nineteenth and twentieth centuries. Eminent scholars from Europe and North America, who together form the editorial board for the series, edit the respective volumes:

Volume 1: *Ancient Science*, edited by Alexander Jones, University of Toronto, and Liba Chaia Taub, University of Cambridge
Volume 2: *Medieval Science*, edited by David C. Lindberg and Michael H. Shank, University of Wisconsin–Madison
Volume 3: *Early Modern Science*, edited by Katharine Park, Harvard University, and Lorraine Daston, Max Planck Institute for the History of Science, Berlin
Volume 4: *Eighteenth-Century Science*, edited by Roy Porter, late of Wellcome Trust Centre for the History of Medicine at University College London
Volume 5: *The Modern Physical and Mathematical Sciences*, edited by Mary Jo Nye, Oregon State University
Volume 6: *The Modern Biological and Earth Sciences*, edited by Peter J. Bowler, Queen's University of Belfast, and John V. Pickstone, University of Manchester
Volume 7: *The Modern Social Sciences*, edited by Theodore M. Porter, University of California, Los Angeles, and Dorothy Ross, Johns Hopkins University
Volume 8: *Modern Science in National and International Context*, edited by David N. Livingstone, Queen's University of Belfast, and Ronald L. Numbers, University of Wisconsin–Madison

Our collective goal is to provide an authoritative, up-to-date account of science – from the earliest literate societies in Mesopotamia and Egypt to the end of the twentieth century – that even nonspecialist readers will find engaging. Written by leading experts from every inhabited continent, the essays in *The Cambridge History of Science* explore the systematic investigation of nature and society, whatever it was called. (The term "science" did not acquire its present meaning until early in the nineteenth century.) Reflecting the ever-expanding range of approaches and topics in the history of science, the contributing authors explore non-Western as well as Western science, applied as well as pure science, popular as well as elite science, scientific practice as well as scientific theory, cultural context as well as intellectual content, and the dissemination and reception as well as the production of scientific knowledge. George Sarton would scarcely recognize this collaborative effort as the history of science, but we hope we have realized his vision.

David C. Lindberg
Ronald L. Numbers

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# I

# INTRODUCTION

*Peter J. Bowler and John V. Pickstone*

Preparation of this volume has been a daunting task for both editors and authors. We have had to create a workable framework through which to present an overview of the development of a diverse range of sciences through a period of major conceptual, methodological, and institutional changes. Equally problematic has been the need to ensure that the presentation takes note of both the enduring traditions within the history of science and the major historiographical initiatives of the last few decades. We have tried to ensure adequate treatment of both the sciences themselves and historians' concerns about how they should be studied. Some sacrifices have had to be made to create a viable list of topics. The result is, we hope, representative, but it is by no means encyclopedic. Topics that might have been expected were dropped either because there was not enough space to cover them adequately or, in a few cases, because the editors could not find authors willing to synthesize vast ranges of information and insights in the space that could be allowed. We are particularly conscious that agriculture and related sciences are barely present and that some areas of the environmental sciences could not be covered, including oceanography and meteorology.[1] Delays have been inevitable in the production of so complex a text, and although some efforts have been made to update the references in the chapters, we and the authors are conscious of the fact that what we are presenting will not always reflect the very latest developments and publications.

We have sought to achieve a balance between the earth and the life sciences, the traditions of natural history and the biomedical sciences, the "old" and "new" sciences, and between the development of particular sciences and more general perspectives and techniques. We have also tried to alert the reader to new developments in the historiography of science and to current interests

[1] See Peter Bowler, *The Fontana/Norton History of the Environmental Sciences* (London: Fontana; New York: Norton, 1992). For useful notes on the agricultural sciences, see Harwood, Chapter 6, this volume.



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in the relationship between the history of science and broader social and cultural history. This introduction seeks to provide an outline of these issues for the reader who needs a first introduction to the history of the life and earth sciences in the modern period.

The history of science has come a long way since the editors first came into the field. Scientists have often worried about initiatives that explore the social dimension of how scientific knowledge is created, fearing that the search for social context ends up treating science as no more objective than any other belief or value system. Some historians worry that strongly relativist approaches may alienate the history of science from one of its natural constituencies – the scientists themselves. At the same time, however, virtually all professional historians of science have found it necessary to distance themselves from the kind of history that is often done by the scientists who take a passing interest in the development of their field. Such history is invariably done by hindsight, using modern interests to determine the value of past science, often thereby doing violence to what the historian sees as crucial within the very different cultural and social contexts of past eras. We need a balance between the need to contextualize science, so that we can see it as a human activity, and the scientists' feeling that – whatever the human dimension – there is something special about scientific knowledge even if it cannot be regarded simply as facts about nature.

By the 1960s, the history of science had emerged as a recognized academic discipline with a central core of interests and techniques. At this time, it was still widely assumed that the study of how science develops should be concerned principally with the scientific theory. The history of science was routinely linked with the philosophy of science – the study of the scientific method and the epistemological problems generated by the search for objective knowledge of nature. No doubt the generation of scientific knowledge had philosophical, religious, and practical implications, but these were of interest to a rather different group of "externalist" historians who concerned themselves with the engagement between science and the outside world. Few "internalists" would have conceded that the external factors played a role in shaping the *knowledge* that was generated.

At the same time, no internalist historian would have pretended that science was merely the steady accumulation of factual information as implied by the old method of induction. Indeed, much attention was already focused on areas where science seemed to have advanced by new theories that required the reinterpretation of all existing knowledge in the field. In this sense, the history of science was part of the history of ideas, and the creation of major new theories was seen as integral to the emergence of new worldviews that had transformed Western culture. Concepts such as heliocentric astronomy, evolution theory, or the germ theory of disease were accepted as a defining feature of the modern world. But such conceptual revolutions were still seen as being initiated by puzzles or opportunities created by the accumulation of factual observations. The search for a better way of describing the world

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in objective terms was still paramount, and the broader implications of the resulting theoretical revolutions were still seen as a secondary phenomenon. There was a one-way flow of influence between theoretical innovation within science and the wider domains of Western science and culture. Everyone simply had to adjust themselves to the new ideas generated by scientific progress.

This model of the history of science, often associated with the philosophy of science promoted by Sir Karl Popper, was broadly acceptable to the scientists themselves because it preserved the claim that new initiatives could be explained simply as attempts to gain better descriptions of the natural world. But already by 1962 Thomas S. Kuhn's *Structure of Scientific Revolutions* had challenged that consensus by arguing that the scientific community had to be understood in sociological terms. Social pressures helped maintain scientific conformity, and most research was done within paradigms that predetermined the projects that were relevant and the innovations that were acceptable. Radical new insights were resisted, even when old theories were visibly failing to account for new observations – the anomalies were swept under the carpet until a crisis was reached, and only then did a scientific revolution become possible. Here was a radical, and at the time highly controversial, challenge to the objectivity of science. It was also a challenge that encouraged internalist historians to take an interest in the workings of scientific communities. And it soon became clear that innovations in scientific theory did not necessarily originate within the field concerned; some spread from related fields or were prompted by new instruments or by new arrangements for professional education or practice. To get a rounded view of the production of knowledge, historians had to understand the social and economic features of the period – its institutions as well as its ideas.

From this point onward, the history of science became steadily more sociological, more interested in what scientists actually do than in what the armchair philosophers say they ought to be doing. Attention has increasingly switched from the theories themselves to the professional groupings that define the way science is actually done. Historians now pay much greater heed to the emergence, maintenance, and transformation of research schools and disciplines.

Historians' growing interest in the practice of science has led to a spread of interests away from the classic theoretical revolutions. Where theoretical revolutions did not map directly onto the emergence of new disciplines, the new approach has tended to deflect attention away from theoretical innovations as the main punctuation marks in the development of science. For example, though the Darwinian revolution of the 1860s undoubtedly had major effects on how scientists thought within established areas of natural history and the life sciences, evolutionary biology became established as a recognizable branch of the field only much later, in the mid-twentieth century, and then only with much difficulty. We should not assume any simple mapping of ideas and structures, and still less that evolution was

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a major determinant in all biological sciences. Much of late nineteenth-century biology can be profitably studied in terms of the changing patterns of work within established areas such as morphology or physiology, and this is obviously true for medicine, where the impact of Darwinism was minimal except via eugenics.

And yet, seen from another perspective, Darwinism retains its importance – as transforming or threatening common understandings of the world. Through studies of evolutionary theory or through analyzing the ways in which individuals and communities see disease or epidemics, we can investigate the interplay of technical knowledge and more general, shared cosmologies. Was man a unique creation? Was disease a punishment? Or are we to reconcile ourselves to a world where the emergence of humans or the occurrence of epidemics have natural causes rather than meanings? We no longer take for granted that the flow of influence is one-way only, from scientific insights to broader social and cultural developments. The fact that science is embedded not only within its own social structures but also within society as a whole is now seen as shaping the way in which scientific innovations are made.

Scientists have religious beliefs and philosophical opinions; they may in addition have political views, both consciously expressed and reflecting the less tangible influence of broader ideologies embedded within the societies within which they live. They also have practical concerns, both about their professional positions and the ways their work can be exploited in medicine and technology. Historians now routinely expect to find that these factors influence scientists' choice of research projects and the kinds of theories they are inclined to support or develop. Without necessarily wanting to go down the route of radical social constructivism, few historians would deny that accounts of brain functions in the early nineteenth century were related to social class or that Darwin's theory shows the influence of the individualistic social philosophy within which he was raised. Indeed, the best modern historiography seeks to integrate the ideological contexts with the detailed, technical work.

A further spin-off from this willingness to concede the effect of the local professional environment has been the recognition among historians that our own perception of the past is shaped by our viewpoint in the present. To some extent, English-speaking historians have defined the great scientific revolutions of the past in terms of concerns and values still current in their own national scientific consciousness. The amount of attention focused on Charles Darwin by historians of evolutionism, for instance, reflects English-speaking scientists' greater commitment to the genetical theory of natural selection as the defining feature of their field. Darwin's impact would be seen in a very different light by French or German historians of science seeking to describe the role played by evolutionism in their own countries. They are much more likely to focus on museums and universities – rather than natural

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history, field geology, and exploration – and more likely to see cell theory and morphology as the main business of nineteenth-century "biology." As a consequence, they are also more likely to stress the links between biology and medicine.

The intense focus on the impact of Darwinism among Anglo-American historians of biology also has "knock-on" effects in other areas. The decision to treat the debate over Charles Lyell's uniformitarianism as a defining feature in the emergence of scientific geology is almost certainly a product of the sense that his methodology marked an important step on the way to Darwinism. But continental geologists paid much less attention to Lyell and would thus dismiss this debate as a sideshow. Most of the chapters in this volume have been written by historians trained within the Anglo-American community. Yet because the chapter titled "Geology" has been written by a specialist in the development of continental European geology, the impact of Lyell has been played down in accordance with that tradition.

Readers should also be aware that much of the recent writing on biomedical sciences comes from historians who are interested in medicine and its practice, as well as in the sciences. They tend to stress the ways that "scientific practices" are related to diagnosis, and they have to be aware of the complex, ever-changing social and institutional environments in which most medical experts have worked. As a result, the chronologies of the history of medicine tend to be different from those of the history of science.

Histories of the physical sciences have tended to focus on the scientific revolution of the seventeenth century, and some historians of biology tried to follow them by stressing mechanistic biology, quantification, or the experiments of William Harvey. Other historians of biology focus on Darwinism, or evolution more generally, in the belief that this defining concept made biology scientific. But historians of medicine have usually focused on the establishment of clinical medicine in the hospitals of post-Revolutionary Paris, seeing there not just a new concept of disease as tissue lesion but an associated set of practices through which the "gaze" of the clinical examination (and autopsy) displaced the patients' narrative in defining the nature of the illness. Some historians would see the focus shifting later to laboratories, where medical scientists created new tests and new forms of experimentation, so that by the end of the nineteenth century, physiology and bacteriology increasingly defined the understandings to which clinicians aspired.

But, in general, we do well to see such methodological shifts not as replacements but as displacements by which new concerns and procedures are added to the repertoire, often through arguments about their importance compared to the longer-standing (and persistent) practices. Thus patients' narratives and clinical examinations remain important in most areas of medicine, and in some (e.g., psychoanalysis), they remain central. So, too, in the development of the biological sciences, taxonomy and natural histories of particular localities remain important, even when most biologists may be more concerned

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with analyzing bodies into patterns of cells, proteins, or DNA or with experimenting on physiological or biochemical systems.[2]

So perhaps medicine can teach historians of science to be rather less "linear" and rather more pluralist in their accounts of scientific work. Certainly we can see how a concern with scientific and medical work within institutions has provided a sociohistoric framework in which we can map the development of biomedical theories and practices over the nineteenth and twentieth centuries. It is a framework that connects and compares the leading and imperial nations of the West, especially through their educational policies and economic activity. It seems worth sketching that framework in the hope that it will serve to connect and ground the chapters that follow in this volume.[3]

Few historians would now try to understand the zoology of Georges Cuvier and Jean-Baptiste Lamarck, or the medical science of Xavier Bichat and François Magendie, without reference to the new or reformed institutions created by the government of France after the Revolution. These provided financial support and institutional power for intellectuals who saw themselves as reformers of their subjects and as creators of textbooks, journals, and definitive collections. That the prestige institutions of early nineteenth-century France were state museums, hospitals, and professional schools – rather than universities – helped create a tradition of elite technocrats close to government and a long-standing opposition between state-supported intellectuals and the Catholic Church. Those early nineteenth-century institutions were the context for major developments in analytical zoology, botany, stratigraphy, and general anatomy, and of various applications of chemistry to plants, animals, and humans. That was also the context *outside* of which Claude Bernard and Louis Pasteur found ways of developing their experimental laboratories in the latter half of the century. In the twentieth century, and especially since the 1960s, prestigious French research has mostly been supported by institutes with direct state support rather than through the universities.

German science, by contrast, was shaped beginning in the 1820s by new or reformed universities that enjoyed considerable autonomy and competed for staff and students through the promotion of "research." Recent evidence that the motives of German states were often economic as well as educational and cultural should not hide the long-standing global importance of this new idea of a university – as a community of researchers bent on developing their "disciplines," with students who themselves were potential researchers. Here was a machine for the multiplication of knowledge that bears comparison with the reproductive capacities of modern capitalism. And it was in

[2] For this way of looking at the sciences, see John V. Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine* (Manchester: Manchester University Press, 2000; Chicago: University of Chicago Press, 2001).

[3] See also the chapters herein on institutions, especially universities, and see the national histories of science in Volume 7 of this series.

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Germany, beginning about 1860, that systematic linkages were made between university scientists (especially chemists), industrial companies looking for new products, and governments keen to promote (late) industrialization. By the 1890s, Germany led the world in organic chemistry, dyestuffs, and new pharmaceuticals and was a major player in the new electrical industries. German science, like much of German culture, set the standard for "civilized nations." Germany was the fatherland of cell theory and medical bacteriology, agricultural chemistry and forestry, morphology and embryology, and the application of experiment within the biological and medical sciences. Experimental physiology, for all its French roots, had been largely developed in German universities; there, too, it spread to plant physiology and to clinical science. In 1890, a science-minded British doctor would try to spend time in a German laboratory (though a cautious patient might prefer the bedside empiricism celebrated by the Harley Street elite).

German university science was imitated with more or less success in the capitals of Northern and Eastern Europe and in the better state and private universities of the United States after the Civil War. But in the United States and especially in Britain, Germanic imports coexisted with more traditional forms of higher education aimed at the gentry and would-be clergy, and with scientific communities in which gifted amateurs were prominent. Wealthy amateurs continued to play a significant role in the scientific elite through the last decades of the nineteenth century, and in some areas of natural history there was significant liaison between the elite and a host of amateur collectors. Although Scottish medical education was university-based, most medical education in England and the United States was based on charity hospitals or proprietary medical schools run by clinicians. Proprietary medical schools were especially prominent in the United States until after the Great War.

In Britain, the older model of scientific education coexisted with a tradition of scientific exploration and surveying appropriate to a great imperial power. In North America, too, the opening up of the American West generated a cultural imperative in which surveying was central to the scientific enterprise. The early nineteenth century saw the foundation of numerous geological surveys, and although these did important scientific work, the intention of the governments that funded them was always utilitarian – they wanted to know what mineral wealth was there to be exploited. Field stations and botanical gardens were founded both in Europe and in colonized territories, again with a view toward understanding how the animals and plants of the various continents could be exploited commercially. Local institutions might also test the potentiality for imported species to be grown commercially in a new environment. The great natural history museums founded in many European and American cities were certainly part of the process by which natural history became professionalized, but they were also "cathedrals of science" that symbolized the West's dominance over the countries whose animals, plants, and fossils were displayed there.

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Starting about 1850, and especially from the 1870s, university and medical reform, plus the founding of new kinds of institutions, allowed the upgrading of "academic science" – often using German models adapted to local conditions. In the United States, "German" research schools coexisted with programs of professional education that sought to instill the principles of practice, including those of engineering and the other "applied sciences," which in Germany were left to the polytechnics. In Britain, the research ideal was variously taken up for chemistry, physics, and physiology, especially in the universities of Glasgow, London, and Manchester. In Cambridge, research flourished in physiology and physics – alongside natural history and the peculiarly strong mathematical tradition. But not until the 1890s did "research" become central to the development of all the major universities. Oxford attained scientific eminence in the early twentieth century, often by importing established professors from the provinces.

By the opening of the twentieth century, France, Britain, and the United States were "catching up" in the biomedical sciences, which were also developing in Japan as it "Westernized." Like most other sciences, biomedicine was favored by a new stress on economic development as nations competed for trade and empire. The imperial connection was particularly important for the biological and agricultural sciences because in the 1890s science began to be seen as a key to the success of empires. "Tropical medicine" would make the colonies safe for Europeans and might improve the health of native workers; scientific agriculture would make for profitable crops and husbandry. Humans, too, might be better bred, multiplying the strong and reducing the reproduction of the weak; in the early 1900s, genetics as a new science was closely tied to eugenics as social prescription. In all such fields, including child rearing, reliance on tradition now seemed inadequate for social progress; science held the key to better practice, and its messages were to be spread through schools, clinics, and popular lectures.

At much the same time, and again across all the leading nations, bacteriology promised the conquest of infectious diseases at home, and new state and charity institutes were established for medical research. These were loosely linked with universities, whose medical schools were becoming more scientific as the professions and governments, especially in the United Kingdom and the United States, pursued a university-based model of medical education. The generation before the Great War was formative for the institutions and disciplines of biology and medicine, both in "applied areas" and in the "pure sciences" dominated by experimental physiology, then seen as a model of scientific medicine and as a bridge between the medical and science faculties.

The interwar years were difficult for the European nations damaged by defeat or victory. Although the war had increased state investment in research, and though that effort continued, the pace of educational expansion seems to have slowed in France, Germany, and the United Kingdom. The American

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economy was stronger, and new subjects such as biochemistry and genetics were institutionalized there partly because American universities were more open and "applied" in their structures. The German hegemony was gone; some American researchers still went there, but they also came to Britain, and the Anglo-American scientific community became more important.

At the same time, the decline of infectious disease in the West and the emergence of chronic causes of mortality, especially cancer, gave new focus to medical research and charity. By the 1930s, the world's leading pharmaceutical companies all had laboratories for research and product development (not just for quality control). Infectious disease in the tropics remained important for the British and French empires, and the Rockefeller Foundation funded American studies – for the southern states as well as for countries in which the United States had a growing economic interest. The Rockefeller Foundation also emerged as a major player in fundamental science, supporting a program in what became molecular biology.

Since 1940, the world of biomedical sciences has been transformed by the two forms of investment that had emerged strongly by the end of the nineteenth century – from governments and from industry. The third quarter of the twentieth century was dominated by state investment as Western and Soviet bloc governments poured huge resources into war-related research, space programs, medical services, agricultural intensification, and overseas development. In the earth and environmental sciences, these investments created new opportunities for scientists and led to the transformation of some disciplines. Opportunities to study the deep-sea bed, generated by the concern for submarine warfare, boosted the prestige of geophysics at the expense of traditional geology and made possible the emergence of the theory of plate tectonics and continental drift. Space exploration offered new methods of monitoring the earth's surface. Almost all countries saw a substantial increase in university-level science and in technical manpower, often financed directly or indirectly by military and industrial resources.

Similar developments took place in those areas of the life sciences that could be associated with medicine. Heart disease, and especially cancer, became objects of investment and prestige comparable to the space race, and researchers presented themselves as "biomedical" to capitalize both on the intellectual prestige of science and the intended benefits of medicine. The pharmaceutical firms expanded their product ranges to include the new antibiotics and new kinds of molecules acting on the nervous and cardiovascular systems; traditional remedies were marginalized, especially in the hospitals, which now dominated health care.

In the decades after World War II, biological sciences in universities were reconfigured, partly in response to the successful analyses of DNA, RNA, proteins, and the relations between them – all made possible by sophisticated analytical methods, including isotopes, x-ray crystallography, and the creative use of specific enzymes. After the Cambridge discoveries of Watson

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and Crick in 1953, the genetic code came to define a "molecular" biology – pulling together the various life sciences at a level below cells and genes. The old configurations of disciplines based on botany and zoology (in the science faculties) and the medical sciences (as taught to medical students) variously gave way to a vertical division between ecological sciences concerned with environments and biomedical sciences, which focused on subcellular structures and happenings in man or any other organism. That is simplistic – some new configurations, such as neurosciences, were system based, spanning from coelenterates to cerebral dysfunctions in man – but one way or another, the disciplinary structures of the early twentieth century gave way to new formations whose inhabitants were sufficiently numerous and confident to rival the prestige of the physical scientists and the relevance of the clinicians. The biomedical sciences were the new frontier and the motor of change in medical practice; the environmental sciences, on a much smaller scale, held the key to a newly emergent challenge – environmental damage and species loss on a global scale.

This restructuring of biology and medicine gained force in the last quarter of the century as molecular biology and the new genetics moved from analytical acumen to experimental syntheses and came to be linked more closely with the large pharmaceutical and agricultural companies that, partly through repeated mergers, had come to shape medical and agricultural practices worldwide. These companies invested in genetic engineering – directly, by buying up the small companies founded by academics, or through supporting university research.

One should not, of course, forget the large quantity of university research that continues to be funded by research councils and others according to the disciplinary priorities of academics, or indeed the massive "development" work that is characteristic of the industries and of relatively little interest to academics. But nor can one ignore the extension of the "technoscientific" interplays across much of the biomedical research scene. The ties of research to commerce were further enhanced, in various countries, by the privatization of the laboratories and agricultural stations once paid for by the state and by the tendency of governments to see science as a direct part of the infrastructure of national industries rather than a form of cultural investment.

That these general patterns of development can be described across nations, especially for the twentieth century, should not, however, hide the continuing importance of local and national differences. Although fully comparative histories are rare, many sociohistoric studies are enhanced by partial or implicit contrasts between locations. As we have hinted, one important consequence of focusing on the practice of science has been recognition of the local variations in how fields are organized and defined. For example, neither the conceptual revolution nor the disciplinary specialization that led to the creation of genetics in Britain and especially the United States worked out the same way in France and Germany. Nor could one fully account for patterns

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of cancer research and treatment without noting the marked national differences in the professional uses of radium in the early twentieth century. And, for all the international movements around molecular biology, the success of the postwar Cambridge program owes much to a peculiarly British drive by which some physicists and chemists were encouraged to address the problems of life and a few biologists were welcomed into a famous physics laboratory with a strong specialization in x-ray crystallography. Whatever the rhetoric, science has never been an internationally homogeneous body of information because the scientific community itself reflects national styles of thought and social organization.

To chart these developments, we have divided the chapters in this volume into a number of categories by subject matter rather than by historiographical approach. Some deal with traditional areas of interest to historians of science, others with newly emerging categories characteristic of the professionalized science of the twentieth century. A number of chapters deal with individual disciplines, but against this background we have a chapter reminding us of the continued involvement of the amateur in many areas of natural history. Traditional areas of study within natural history included botany and zoology, but we chart the increased specialization of modern science by showing how these broadly based areas became fragmented into ecology, genetics, and other specialties, often through the definition of new objects of study previously obscured by the search for a comprehensive explanation. In the biomedical sciences, of course, there was much less room for the amateur from the start, and the involvement of the medical profession shaped opportunities for the emergence of scientific disciplines and professions.

As we have noted already, another way of tracing the practices of science is to look at the institutions within which the research is done and the external bodies that make use of the information produced. So we have included chapters on institutions such as museums and hospitals and also the increasingly important locus of the university. The strong link with practical applications is illustrated through chapters on geological industries and various branches of medicine. Our survey has not lost sight of the external relations of science, such as the interaction with religion and the involvement of the biological sciences in the attempt to understand human nature. Newer areas of external concern such as environmentalism and the ethics of human experimentation are also included.

For the most part, authors have been "given their head" and allowed to approach their topic in whatever way seemed natural to them. Given the immensely difficult job of summarizing both historical information and changing historical interpretations in less than ten thousand words, we are hugely grateful for their efforts (and their patience). Some have chosen to develop their account from the primary (scientific) literature in their field, whereas others have focused exclusively on the secondary literature in which the historical issues have been debated. Obviously, a starting point in the

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primary literature is essential in those areas where comparatively little historical analysis has been done.

As this volume shows, by comment or by omission, there are still large areas of science that remain neglected by historians, sometimes quite important ones, so perhaps this volume will guide younger researchers to these unworked areas. But its scope may also encourage them to try to answer big questions about the development of the sciences, in all their variety across time and space. Many papers on the history and sociology of science now seem to assume that science is one or is differentiated only by places of work such as the museum or laboratory. But the chapters that follow give a much richer picture – of multiple dynamic interactions between changing conceptual structures, technical possibilities, and social formations. Getting a grip on these interactions remains a major challenge for historians and an important way for all of us to understand our present.

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*Part I*

# WORKERS AND PLACES

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# 2

# AMATEURS AND PROFESSIONALS

*David E. Allen*

Science in the nineteenth century underwent major transformations. The immense growth of knowledge encouraged subdivision into increasingly narrow and self-contained areas of specialization. Science changed from an area of learning in which it was exceptional for people to be paid to pursue it into one in which large numbers were receiving instruction in schools and universities with the expectation of making their living from it. Science turned into a substantial profession, but the process of professionalization was not automatic. In most developed countries, there were conditions inimical to it, and when the change eventually took place, it did so comparatively abruptly and generated considerable tension. This compression has been a boon to historians, for it provides them with a clearly marked stratum dividing the preexisting world of science from the very different one that emerged shortly afterward.

## THE PREPROFESSIONAL ERA

Until the 1880s, it is unhelpful and misleading to employ the categories "amateur" and "professional." Whereas "amateur" has come to acquire a derogatory overtone, especially in the United States,[1] it was the "professional" who was despised in the early nineteenth century. A professional was someone who received money to do something that others did for pleasure, and to put one's labor up for hire placed one in the position of a servant. This aristocratic prejudice had trickled down into the upper middle class and restricted the

[1] Sally Gregory Kohlstedt, "The Nineteenth-Century Amateur Tradition: The Case of the Boston Society of Natural History," in *Science and Its Public: The Changing Relationship*, ed. Gerald Holton and William A. Blanpied (Dordrecht: Reidel, 1976), pp. 173–90; Elizabeth B. Keeney, *The Botanizers: Amateur Scientists in Nineteenth-Century America* (Chapel Hill: University of North Carolina Press, 1992), p. 3.



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range of occupations members of that class could follow.[2] Only four were acceptable: the armed forces, the church, and the more respectable branches of the law and medicine.

It was the social respectability of physicians that created the first paid positions in the life or earth sciences. There were professorships of botany in the medical schools, and since the sixteenth century botany had achieved autonomy as a discipline and gained chairs of its own. In the eighteenth century, Carl Linnaeus (1707–1778) and a few others were able to make a living in chairs of botany. Medicine was subsequently able to provide niches, especially in museums of anatomy, for zoologists and paleontologists, too.

The rise of industrialism produced a second vocational outlet for specialists: first mineralogists and later, as knowledge of stratigraphy developed, earth scientists of a broader kind. From as early as 1766, in France it was possible for a select few to subsist on fees earned as freelance consultants in geology. There were also government bodies, such as the Board of Ordnance in Britain and the Boundary Survey in Ireland, whose interests extended sufficiently into geological territory for individuals on their staffs to have fieldwork accepted as part of their official duties. From the 1820s onward, undisguised employment on state-sponsored geological surveys became available – some of these beginning as short-term projects but increasingly becoming effectively permanent.[3] By the middle of the nineteenth century, these surveys held the largest bodies of people outside the universities and national museums who were paid to undertake research in the natural history sciences. They could even serve as Trojan horses for the employment of other kinds of naturalists by governments: In 1872, the Geological Survey of Canada had "and natural history" added to its title and recruited John Macoun as its botanist.[4] Even in a country without a tradition of patronage, such as the United States, a substitute was available from rich philanthropists such as William Maclure (1765–1840). His munificence financed the Academy of Natural Sciences of Philadelphia during the twenty-three years of his presidency and sustained the entomologist and conchologist Thomas Say (1787–1834) and the ichthyologist Charles-Alexandre LeSueur (1778–1840).[5]

The drawback of these protoprofessional positions was that the pay was not enough to live on for anyone aspiring to middle-class status. In France,

[2] Morris Berman, "'Hegemony' and the Amateur Tradition in British Science," *Journal of Social History*, 8 (1974), 30–50.

[3] See Paul Lucier, Chapter 7, this volume.

[4] Carl Berger, *Science, God, and Nature in Victorian Canada* (Toronto: University of Toronto Press, 1983), p. 16.

[5] Thomas Peter Bennett, "The History of the Academy of Natural Sciences of Philadelphia," in *Contributions to the History of North American Natural History*, ed. Alwyne Wheeler (London: Society for the Bibliography of Natural History, 1983), pp. 1–14; Charlotte M. Porter, *The Eagle's Nest: Natural History and American Ideas, 1812–1842* (Tuscaloosa: University of Alabama Press, 1986), pp. 5, 57.

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Austria, and especially Germany, in which there were long-established traditions of patronage by the state, as well as in the United States, where emphasis on the practical potential of science early on brought funding by government, this drawback was much less of a problem than in Britain. There, would-be professionals had to contend not only with the state's reluctance to support learning,[6] an attitude buttressed by the doctrine of laissez-faire but also with the miserably small salaries conceded when it departed from its normal aloofness. There was an assumption that such posts would attract those with private means, but some were taken out of desperation by people whose expectation of financial security had been dashed by a collapse in the family fortunes. Such was the fate that overtook the geologist Henry Thomas De la Beche (1796–1855), the zoologist William Swainson (1789–1855), and the pioneer of marine biology Edward Forbes (1815–1854). For these *rentiers manqués*, as they have been termed,[7] the struggle to reconcile their social position with their reduced means was hard. They had to seek more than one source of livelihood, often at a severe cost in research time and health. Nevertheless, science in Britain was enriched by this trickle of social refugees, a benefit only possible, ironically, in a world still free from certification barriers. Posts in government service were filled by competitive examination only after 1855 in Britain; until then, scientists had been appointed as much on the strength of recommendations from the politically influential as from those competent to pronounce on their achievement. The nearest thing to a paper qualification for a post in the life sciences was a medical degree and the nearest thing to postgraduate training was a journey to little-known parts of the world as the naturalist attached to a voyage or expedition, perhaps as a surgeon on a naval vessel or (as in Charles Darwin's case) as gentleman-companion to its captain. The shortage of more concrete yardsticks made election to the more prestigious scientific societies all the more coveted.

The drawbacks to being employed in public or private institutions devoted to learning were more than just financial. Despite lavishly funding expeditions to distant parts of the globe, governments were reluctant to pay for the study of what those expeditions brought back. Some valuable collections lay in museums unpacked for as long as several decades.[8] Simply catching up with curatorial arrears, let alone dealing with routine administration and inquiries from outsiders, left little or no time for carrying out research. The only real advantage that holders of such posts enjoyed over the general run of amateurs was permanent access to a large reference collection, but many

[6] J. B. Morrell, "Individualism and the Structure of British Science in 1830," *Historical Studies in the Physical Sciences*, 3 (1971), 183–204.

[7] D. E. Allen, "The Early Professionals in British Natural History," in *From Linnaeus to Darwin: Commentaries on the History of Biology and Geology*, ed. Alwyne Wheeler and James H. Price (London: Society for the History of Natural History, 1985), pp. 1–12.

[8] Paul Lawrence Farber, *The Emergence of Ornithology as a Scientific Discipline: 1760–1850* (Dordrecht: Reidel, 1982), p. 149; Ray Desmond, *The India Museum, 1801–1879* (London: Her Majesty's Stationery Office, 1982), pp. 63–4.

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wealthy scientists possessed such collections of their own and plenty of time in which to put them to use.

## CATEGORIZING THE AMATEURS

Except in the geological surveys and the universities of the German states, researchers able to earn a living from the life or earth sciences were too thinly scattered to permit much sense of a professional community to emerge. If they worked in a major city, they could meet their counterparts in the learned societies that had been increasing in number since late in the previous century. But otherwise their only opportunities of mingling with others who shared their interests were the annual gatherings of the Gesellschaft Deutscher Naturforscher und Artze, the British Association for the Advancement of Science, the Congrès Scientifique de France, and the American Association of Geologists and Naturalists. Started respectively in 1822, 1831, 1833, and 1840 (the last evolved into the American Association for the Advancement of Science in 1848),[9] these bodies drew their respective countries' scientists en masse to a different city each year. In the informal appendages to which these meetings gave rise, such as the Red Lions Club in Britain, professionals found common cause and sometimes vented their grievances.

So small was the community of science professionals in the pre-1880 era, and so slight the difference in outlook between that community and everyone else involved in scholarly pursuits, that the category of "professional" can hardly be of much use for historical analysis. Rather, it is within the amateurs that historians of science are increasingly coming to recognize categories that can more usefully be distinguished. The amateurs comprised various sets of people with differing levels of knowledge and degrees of commitment. The most elaborate of several classifications so far proposed to this end is a threefold one put forward by Nathan Reingold:[10]

- "Researchers," the people at the cutting edge, with a devotion to research yielding appreciable accomplishment and usually but not invariably in fully scientific occupations;

[9] Sally Gregory Kohlstedt, "Savants and Professionals: The American Association for the Advancement of Science, 1848–1860," in *The Pursuit of Knowledge in the Early American Republic*, ed. Alexandra Oleson and Sanborn C. Brown (Baltimore: Johns Hopkins University Press, 1976), pp. 209–325.

[10] Nathan Reingold, "Definitions and Speculations: The Professionalization of Science in America in the Nineteenth Century," in Oleson and Brown, *The Pursuit of Knowledge in the Early American Republic*, pp. 33–69. See also Robert H. Kargon, *Science in Victorian Manchester: Enterprise and Expertise* (Manchester: Manchester University Press, 1977). On the continued role of gentlemen-amateurs even within the influential "X club," see Adrian Desmond, "Redefining the X Axis: 'Professionals,' 'Amateurs' and the Making of Mid-Victorian Biology," *Journal of the History of Biology*, 34 (2001), 3–50.

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- "Practitioners," those largely employed in science-related occupations using their scientific training but not necessarily publishing;
- "Cultivators," those applying their knowledge in some kind of scientific activity but not remunerated and quite often concerned with their own self-education rather than the increase of knowledge.

In this context, Neal Gillespie's definition of "working naturalists" also merits repeating: "those who, for the most part, published in recognised scientific formats; whose purpose in writing about nature was not primarily philosophical, ideological, or literary; and who . . . developed a sense of professionalism that excluded the closet naturalist as well as the mere popularizer."[11] These are clearly the same people Roy Porter has distinguished as "career" geologists: "a self-sustaining, self-validating knowledge elite, guardians of expertise in their fields of intellectual endeavor."[12]

Such categories offer means of countering the tendency for "amateur" to be used as no more than a synonym of "nonprofessional." It also needs to be borne in mind that contemporaries would not necessarily have seen Reingold's trio as constituting a hierarchy. Although the expertise of the "researchers" would have been deferred to, it would not have saved them from being snubbed by "cultivators" who pulled social rank on them. Scientific knowledge had not yet acquired sufficient complexity to prevent those in all three categories from reading the same publications or attending the same lectures, and all but the grander societies catered to them without distinction. That is not to say that some stratification and segmentation did not exist. Class and (often more bitter) sectarian divisions were conducive to mixing socially only with those with whom one felt comfortable. In some manufacturing districts of Britain, a special type of society came into being to meet the constricted circumstances in which artisans strove to convert a tradition of identifying medicinal herbs into a thoroughgoing Linnaean botany.[13]

The layering of the scientific community furthered the proliferation of local societies that was such a feature of the mid-nineteenth century in several European countries. Britain and France witnessed the peak of that proliferation in the 1870s,[14] after which faster transportation made bodies

[11] Neal C. Gillespie, "Preparing for Darwin: Conchology and Natural Theology in Anglo-American Natural History," *Studies in the History of Biology*, 7 (1984), 93–145.

[12] Roy Porter, "Gentlemen and Geology: The Emergence of a Scientific Career, 1660–1920," *Historical Journal*, 21 (1978), 809–36. See also Martin J. S. Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985).

[13] Anne Secord, "Science in the Pub: Artisan Botanists in Early Nineteenth Century Lancashire," *History of Science*, 32 (1979), 269–315; Anne Secord, "Artisan Botany," in *Cultures of Natural History*, ed. N. Jardine, J. A. Secord, and E. C. Spary (Cambridge: Cambridge University Press, 1996), pp. 378–93.

[14] [J. Britten], "Local Scientific Societies," *Nature*, 9 (1873), 38–40; Yves Laissus, "Les Societes Savantes et l'Avancement des Sciences Naturelles: Les Musees d'Histoire Naturelle," in *Actes du Congres National des Societes Savantes* (Paris: Bibliotheque Nationale, 1976), pp. 41–67, see p. 47; Philip

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with a national coverage more attractive. In a late-settled country such as Canada, however, local societies devoted to natural history were at first the only learned organizations available.[15] The development of such societies was retarded by a tendency to adopt the conventional model of the classical academy. The bodies thus produced were socially exclusive and functionally inflexible because of the high costs of owning a building and employing staff to organize meetings and take care of a library and collections. The inappropriateness of this model for field natural history was exposed in Britain in 1831 when a new type of body emerged, the field club, inspired by the practice in the medical schools of taking classes out into the countryside to familiarize them with herbs in their natural state.[16] Making such outings the central activity and dispensing with the millstone of a headquarters, this alternative model demonstrated that it was still possible to function reputably through fieldwork and published reports alone.[17]

The field club was an ideal framework for the collective pursuit of natural history in the more thinly populated areas. It could meet in places convenient for those who were otherwise isolated while enabling all parts of the local "territory" to receive attention. It also brought in the medical practitioners and ministers of religion anchored in rural comunities. Many of those who manned these two professions were university educated, some of them fully a match in intellectual caliber to those employed as scientific specialists. The Rev. Miles Berkeley (1803–1889), for example, combined running a parish with a stupendous research output and a world reputation as a mycologist.

A medical career had long been the most obvious destination for anyone interested in animals or plants. In Britain, legislation in 1815 aimed at stamping out quacks had the side effect of making a working knowledge of herbs almost a precondition of a license to engage in general practice.[18] Field classes for medical students multiplied in response, and a wave of recruits to recreational botany was secured in the process.

Ministers of religion based in rural parishes tended to enjoy a greater margin of leisure than their medical counterparts. Protestantism is customarily thought of as more conducive to the study of nature, but enough *abbés* rose to prominence as naturalists in pre–twentieth-century France to suggest that the Roman Catholic Church was by no means inimical to the study of nature. The established church in England, thanks to its policy of

Lowe, "The British Association and the Provincial Public," in *The Parliament of Science: The British Association for the Advancement of Science, 1831–1981*, ed. Roy MacLeod and Peter Collins (Northwood: Science Reviews, 1981), pp. 118–44, see p. 132.

15 Berger, *Science, God, and Nature in Victorian Canada*, p. 12.

16 D. E. Allen, "Walking the Swards: Medical Education and the Rise and Spread of the Botanical Field Class," *Archives of Natural History*, 27 (2000), 335–67.

17 D. E. Allen, "The Natural History Society in Britain through the Years," *Archives of Natural History*, 14 (1987), 243–59.

18 S. W. F. Holloway, "The Apothecaries' Act, 1815: A Reinterpretation," *Medical History*, 10 (1966), 107–29, 221–36.

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filling its benefices with university graduates, created the circumstances most productive of clergymen-naturalists. As a result, through much of the nineteenth century, the life and earth sciences were able to look to the churches for their nonprofessional leadership. Although increasingly less in evidence in the following century, it is a tradition still not yet entirely extinct and overdue for detailed historical study.

A surprising feature brought to light in studies of the Manchester Literary and Philosophical Society and the Botanical Society of London is the high proportion of the members related by blood or marriage, perhaps because family members were the easiest to recruit when a society sought to increase its size.[19] Exceptional though these cases may have been, it does seem that the naturalist community was impressively close-knit. In an age when nepotism still operated in the filling of paid positions, those networks could give rise to dynasties of professionals, of which the de Jussieus in France and the Hookers in Britain are the outstanding examples. As the former unity of science broke up and an increasing army of specialist societies emerged in the larger cities, there were some members who long retained a loyalty to two or more societies and even held office simultaneously in each.[20]

## THE CULTURE OF COLLECTING

The world of natural history was held together by the commitment of everyone in it to the same set of activities and attitudes. While the prevailing modes of study were collecting, describing, listing, or mapping, no division could emerge between those who were paid and those who were not. The necessary techniques were simple to learn and the implements, with one exception, inexpensive. The exception was the microscope, but when the cost of microscopes came down in the 1830s, anyone content with merely observing and describing had access to many fields of study. Works of identification were coming down in price and were no longer published in Latin. The life and earth sciences in the era before the 1880s were open to every literate person. Rich naturalists threw open their houses to allow fellow enthusiasts free run of their libraries and collections.[21] This helped to make up for the exclusiveness of many societies before the spread of public libraries and municipal museums in the second half of the nineteenth century.

[19] Arnold Thackray, "Natural Knowledge in Cultural Context: The Manchester Model," *American Historical Review*, 791 (1974), 672–709; D. E. Allen, *The Botanists: A History of the Botanical Society of the British Isles through 150 Years* (Winchester: St. Paul's Bibliographies, 1986), pp. 44–5.

[20] D. E. Allen, "The Biological Societies of London, 1870–1914: Their Interrelations and Their Responses to Change," *Linnean*, 4 (1988), 23–38.

[21] H. T. Stainton, "At Home," *Entomologists' Weekly Intelligencer*, 5 (1859), 73–4; A. S. Kennard, "Fifty and One Years of the Geologists' Association," *Proceedings of the Geologists' Association*, 58 (1948), 271–93.

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Collecting may even have retarded the development of a more scientific natural history. It was fun, it was only too easy, and it provided a purpose for travelers with time on their hands. The more remote one's destination, the greater the chances of making finds that were important scientifically. The study of marine algology, for example, was advanced by the efforts of well-to-do women in seaside towns who found a valued role for themselves by patrolling their local beaches for unfamiliar seaweeds.[22] Geologists enlisted the help of quarrymen, whose on-the-spot alertness was crucial to many an important fossil discovery. One at least, the Scotsman Hugh Miller (1802–1856), used his knowledge of fossils as a route to influence and fortune as a popularizer of the subject.

Naturalists collected because it was the time-honored route to take, and one could not record if one could not distinguish what one discovered and ideally put a name to it. The amassing of specimens enjoyed high respectability among savants. In the early nineteenth century, thanks to natural theology, it also acquired a moral sanction. Many who had risen to wealth from industry found the possession of a large natural history collection a convenient way of laying a claim to rank, and if they lacked the time or inclination to put a collection together, they could buy one at auction, ready-made. Alternatively, they could subscribe to a commercial collecting agency or to one of the exchange clubs that sprang up, especially in botany. Of these, the Unio Itineraria was the trend-setting model, founded around 1826 by two botanists in Germany, a country that lacked overseas possessions so that its naturalists had to resort to a self-help substitute in order to acquire specimens from distant areas.[23] A body called the Esslinger Reisgesellschaft allowed participants to subscribe for shares in expeditions, in return for which they would receive a proportion of whatever was brought back. This permanent syndicate enriched collections in Germany and in other parts of Europe as well.

By the mid-nineteenth century, the numbers of collectors and museums were such that a naturalist could reasonably count on supporting himself from the proceeds of what he could manage to send back, especially from the tropics, to specialist dealers in natural history material. Alfred Russel Wallace (1823–1899), Henry Walter Bates (1825–1892), and Richard Spruce (1817–1893) were three of the best known to adopt this precarious way of making a living, initially in all three cases in the Amazonian jungle, and in the process earned outstanding reputations as scientists. Most other professional collectors have at least been sure of their funding in advance, including the

[22] Ann B. Shteir, *Cultivating Women, Cultivating Science* (Baltimore: Johns Hopkins University Press, 1996), pp. 183–91; D. E. Allen, "Tastes and Crazes," in Jordine, Secord, and Spary, *Cultures of Natural History*, pp. 394–407, see p. 400.

[23] Sophie Ducker, "History of Australian Phycology: Early German Collectors and Botanists," in *History in the Service of Systematics*, ed. Alwyne Wheeler and James H. Price (London: Society for the Bibliography of Natural History, 1981), pp. 43–51.

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no less resourceful group of plant hunters who combed the region east of the Himalayas for horticultural novelties on behalf of private growers and commercial nurseries.[24]

Although collecting was the dominant activity in the era of preprofessional science, there were a few enthusiasts who undertook a more active study of nature. Some experimented with crossing plants or captured birdsong in musical notation. J. F. M. Dovaston studied the phenomenon of territory in birds through watching their behavior on his estate and even did some rudimentary marking of individual birds and distinguishing of territory boundaries.[25] Those who made major contributions typically belonged to some research subcommunity, perhaps sitting at the center of a web of postal informants, like Charles Darwin or the chief exponent of Humboldtian botany in Britain, Hewett Cottrell Watson (1804–1881). Some worked among the professionals while retaining amateur status, including the plant taxonomist George Bentham (1800–1884), who spent much of his life at the Royal Botanic Gardens at Kew in an entirely voluntary capacity.[26]

## ACADEMICIZATION

Buoyed by its faintly aristocratic aura, the world of natural history entered the last quarter of the nineteenth century confident in what it was doing and with no expectation of altering its ways – although its members were having to revise their convictions drastically to accommodate evolutionary theory. Even those employed as professionals were content to continue as systematists, conscious of the magnitude of the task and expecting to carry on along essentially the same lines.

In fact, the life sciences were about to be polarized by the emergence of the academic discipline of biology. It is significant that a parallel cleavage did not take place in geology, which, even when substantially professionalized, retained links with its amateur following. This was primarily because of the strong emphasis geology continued to place on fieldwork after it developed into an academic discipline.[27] In Britain, the staff of the state-supported Geological Survey necessarily spent much of each year out in the open air. Although it resembled the major botanic gardens in this field orientation

[24] Alice M. Coats, *The Quest for Plants: A History of the Horticultural Explorers* (London: Studio Vista, 1969), pp. 87–141.

[25] D. E. Allen, "J. F. M. Dovaston, an Overlooked Pioneer of Field Ornithology," *Journal of the Society for the Bibliography of Natural History*, 4 (1967), 277–83.

[26] B. Daydon Jackson, "The Late George Bentham, F. R. S.," *Journal of Botany*, 22 (1884), 353–6.

[27] J. G. O'Connor and A. J. Meadows, "Specialization and Professionalization in British Geology," *Social Studies of Science*, 6 (1976), 77–89; Porter, "Gentlemen and Geology"; Ronald Rainger, "The Contribution of the Morphological Tradition: American Palaeontology, 1880–1910," *Journal of the History of Biology*, 14 (1981), 129–58.

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and in being located outside academia, the Survey also functioned as an influential research school.

It was the rise of quite novel laboratory-based disciplines within the universities, rather than any widespread disillusion with systematics on the part of the existing community, that caused the latter to be displaced from its dominance of the life sciences. Improved and still-cheaper microscopes were one major factor in the transformation (though it could not have been the sole one, for the collectors might have made microscopy their own). Another was the increasing numbers of university teachers and researchers competing to open up new fields of study. Now that it was feasible instrumentally to investigate the more arcane processes of nature, descriptive work came to seem banal and unprogressive by comparison. The advent of Darwinism only tipped the balance further by calling into being additional subdisciplines, such as embryology, to reconstruct the continuities of organic development.

Laboratories, however, were expensive to provide, requiring costly apparatus, the recruitment of technical assistants, and extra space. Resistance to the new disciplines was often as much for financial reasons as it was on intellectual grounds. And it was partly because the universities in the German states were better supported that they were able to obtain a lead over their counterparts in other countries in fostering the exploration of these new areas of knowledge. For several decades already, Germany had been looked up to by academics elsewhere as the structural ideal as well as the pacemaker; now it came so well to the fore in the new trends in the life sciences as to make a postgraduate spell in one of its university laboratories virtually obligatory for aspiring teachers and researchers in other countries.

One of those countries, though, the United States, was committed so early to the practical applications of science that it needed the impulse from German biology far less to achieve a thoroughgoing professionalization. A marked rise in the teaching of science, especially botany, took place in U.S. secondary schools in the 1830s.[28] By 1870, it was common for science professors to constitute the majority of teaching faculty in the country's colleges.[29] The United States had missed the stage of the gentleman-naturalist, and its community of collectors contained a high proportion of recent immigrants from Europe, in particular Germany, who needed paid occupations to sustain them.[30] The country's late urbanization also delayed the proliferation of local scientific societies, or indeed the acquisition of such societies in any significant numbers, until after the Civil War.[31] America's social fluidity and

[28] Keeney, *Botanizers*, pp. 54–7.

[29] Stanley M. Guralnick, "The American Scientist in Higher Education, 1820–1910," in *The Sciences in the American Context: New Perspectives*, ed. Nathan Reingold (Washington, D.C.: Smithsonian Institution Press, 1979), pp. 99–141.

[30] Melville H. Hatch, "Entomology in Search of a Soul," *Annals of the Entomological Society of America*, 47 (1954), 377–87, at p. 379.

[31] Reingold, "Definitions and Speculations," p. 34; Ralph Bates, *Scientific Societies in the United States*, 2nd ed. (Cambridge, Mass.: MIT Press, 1995).

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mobility generated many self-styled experts and made certification a particularly pressing priority. Economic needs in both the United States and Canada promoted the development of a market-oriented agriculture, which faced problems in combating insect infestations of crops grown on previously untilled land. This produced a flurry of posts for applied entomologists, with the result that entomology became rapidly professionalized and progressed at a faster rate than in Europe.[32] The Entomological Society of Canada, conceived on its founding in 1863 as merely a link for scattered collectors, soon had its journal subsidized by the government in return for supplying annual reports to the minister of agriculture.[33] Even in the unlikely field of ornithology, the U.S. Congress was persuaded of its applied potential and created a Division of Economic Ornithology alongside an earlier-established entomological sister within the U.S. Department of Agriculture in 1885.[34] But the dislocation caused by the Civil War undermined America's chance for a clear lead over the other competitors in the race to achieve a fuller-scale professionalization of science. Not until the 1870s did the transformation of colleges into institutions of research and graduate training on the German pattern begin to take place, and in the end all the main competitors of Germany breasted the tape together.

In Britain and France, it took at least a decade for the full proportions and the fundamental character of the change to become widely apparent. Only those close to the academic scene would have been likely to recognize the signals that heralded it. These often took the form of an outburst in the literature by one of the leading exponents of the up-and-coming disciplines, such as that by the French physiologist Claude Bernard in 1867 decrying the lack of laboratories and denigrating fieldwork.[35] In Britain, what was later seen as a landmark event was the promotion of the Natural Sciences Tripos at Cambridge to an honors degree in its own right in 1861. But it was not until 1872 that the "new biology" (as its protagonists challengingly proclaimed it) achieved its first real institutional conquest in Britain when the Natural History Department of London's School of Mines acquired space for a teaching laboratory and became free at last to start training its many students in the novel approach.[36]

Despite the conviction that what was being promoted was a radically different creed, there was a time lag in relabeling. Just as the London department

[32] W. Conner Sorensen, *Brethren of the Net: American Entomology, 1840–1880* (Tuscaloosa: University of Alabama Press, 1995).

[33] Berger, *Science, God, and Nature in Victorian Canada*, p. 6.

[34] Mark V. Barrow, *A Passion for Birds: American Ornithology after Audubon* (Princeton, N.J.: Princeton University Press, 1997), p. 60.

[35] Robert Fox, "The *Savant* Confronts His Peers: Scientific Societies in France, 1815–1914," in *The Organization of Science and Technology in France, 1808–1914*, ed. Robert Fox and George Weisz (Cambridge: Cambridge University Press, 1980), pp. 241–82, see p. 258.

[36] J. Reynolds Green, *A History of Botany in the United Kingdom from the Earliest Times to the End of the 19th Century* (London: Dent, 1914), pp. 531–2.

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continued to be one of "natural history," the first association in the United States to reflect the new academic trend persisted in calling itself the American Society of Naturalists. Yet well before that, in 1876, Johns Hopkins University, self-consciously pioneering a recasting of higher education, had led the way in establishing a department of "biology" – with a physiologist and a morphologist as its sole faculty members.[37] Without institutional conservatism to overcome, it took noticeably less long for the paradigm switch to be reflected in the literature. As the output of research papers from the newly emergent disciplines rose to a flood, it began by pouring into existing journals with old-style titles such as the *Botanical Gazette* in the United States. But new journals soon appeared whose orientation was anything but ambiguous: first France's *Archives de Zoologie expérimentale et générale* in 1876 and Britain's *Journal of Physiology* two years after that. Soon after, the same dual pattern was in evidence in the societies, too. In some cases, existing societies were invaded and transformed, and in others the new specialties gave birth to bodies in their own specialized image, some open only to those who had published original research.[38] Specialist societies were the product not merely of the intellectual fissiparousness of academic biology but also of the tensions that arose when biologists colonized bodies that taxonomists and collectors had dominated. This only exacerbated an awkwardness occurring already as the scientific content of natural history itself became sharply more technical. Even in ornithology, a study in which academic biology continued to have little presence, the less scientifically inclined were starting to jib at seeing their subscriptions used for bringing out journals that were increasingly above their heads.[39] In entomology, the situation was to become particularly tense, for that area had a much higher proportion of diehard collectors and also experienced an invasion of applied researchers employed in posts outside the universities. In response, amateur entomologists increasingly chose to congregate in separate societies. That was not a viable alternative, however, in the less populous countries, for the devotees of any minority interest need to exist in considerable numbers to sustain the cost of publishing a periodical. In those countries, a workable modus vivendi was sometimes achieved by partitioning a society into semiautonomous sections, as in the Koninklijke Nederlandse Botanische Vereniging.[40]

[37] Keith R. Benson and C. Edward Quinn, "The American Society of Zoologists, 1889–1989: A Century of Interpreting the Biological Sciences," *American Zoologist*, 30 (1990), 353–96; Jane Maienschein, *Transforming Traditions in American Biology, 1880–1915* (Baltimore: Johns Hopkins University Press, 1991).

[38] Toby A. Appel, "Organizing Biology: The American Society of Naturalists and Its Affiliated Societies," in *The American Development of Biology*, ed. Ronald Rainger, Keith R. Benson, and Jane Maienschein (Philadelphia: University of Pennsylvania Press, 1988), pp. 87–120.

[39] Barrow, *Passion for Birds*, p. 57.

[40] P. Smit, "Van Floristiek tot Moleculaire Biologie: 125 Jaren Koninklijke Nederlandse Botanische Vereniging," *Jaarboek van de KNBV over het jaar 1970* (Amsterdam: Koninklijke Nederlandse Botanische Vereniging, 1971), pp. 117–55; Patricia Faase, *Between Seasons and Science* (Amsterdam: SPB Academic, 1995), pp. 29–41.

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By the 1880s, exponents of the laboratory disciplines were firmly on the ascent across both continents, and the adherents to systematics and the like were increasingly being made to feel outmoded. Within universities there was much bitterness where long-entrenched professors, loyal to the old approach, refused to release rooms for laboratory space or allocate departmental funds to the purchase of equipment.[41] Ironically, though, it was convenient for the biologists to have the old approach persist, for the very fact that it was identified with amateurism allowed them to emphasize their distance from it and so underline their status as a new breed of professionals. For that reason, not all who embraced the new approach considered it sufficient to ignore the world of systematics, a few even going so far as to pour scorn on it publicly. Foremost in that activity were some whose careers had begun in the other world and who now sought to cover their intellectual tracks.[42] An additional reason for such hostility may simply have been incomprehension by those who adopted the more experimental approach derived from physiology.[43]

## ATTEMPTED ADAPTATIONS

There has been an uncritical assumption by some historians, as Paul Farber has pointed out, that the developments just described represent simply the growing up of the life sciences. In the words of another exposer of this fallacy, it was assumed that natural history was gradually transformed into biology by "an intellectual ascent . . . to a higher sort of science involving experiments and explanations."[44] Such assumptions ignore the awkward fact that, far from disappearing or being transmuted, the preexisting approach survived and, after undergoing a substantial redefinition, emerged as vigorous as ever. Despite the contempt to which it was subjected, the natural history tradition proved very resilient. Located largely outside the universities, it was impervious to concepts and techniques that preoccupied academic biologists. The biologists spoke an alien language and had ways of working that were effectively precluded for those without access to a laboratory and the requisite training.

That is not to say that the professionals who continued to practice systematics, and at least some of the more scientifically inclined amateurs, were

[41] F. O. Bower, *Sixty Years of Botany in Britain (1875–1935): Impressions of an Eye-Witness* (London: Macmillan, 1938), p. 102.

[42] R. A. Baker and R. A. Bayliss, "The Amateur and Professional Scientist: A Comment on Louis C. Miall (1842–1921)," *Naturalist*, 110 (1985), 141–5.

[43] Paul L. Farber, "The Transformation of Natural History in the Nineteenth Century," *Journal of the History of Biology*, 15 (1982), 145–52; Eugene Cittadino, "Ecology and the Professionalization of Botany in America, 1890–1905," *Studies in the History of Biology*, 4 (1980), 171–98.

[44] Lynn K. Nyhart, "Natural History and the 'New' Biology," in Jordine, Secord, and Spary, *Cultures of Natural History*, pp. 426–43, see p. 426; Farber, *Emergence of Ornithology as a Scientific Discipline*, pp. 123–9.

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not perplexed and sometimes demoralized by the sudden and drastic change that had overtaken them – it was impossible to ignore the loud trumpetings of the biologists. The natural history community in any case contained already a sprinkling of dissidents who looked for something different. These dissidents felt that collecting was too often just an end in itself, while the compiling of local records seemed to be virtually played out. In a typical mood of fin de siècle disillusionment, one even moaned, with absurd exaggeration, that "every nook has been explored zoologically and botanically, and the stations of every rare species of plant or animal exactly recorded."[45] To those who shared that bleak view, it seemed high time to be switching to some alternative approach.

Two candidates commended themselves to these dissenters. One was a simplified version of the new biology that concentrated on developmental processes. Given the deceptively similar name of "nature study," this originated in the United States, where traditional natural history was less deeply rooted. It crossed the Atlantic, only to become identified too closely with primary education and see its hopes dashed.[46] The other candidate was ecology – in the original, narrow meaning of that word, not the synonym for wider environmentalism it has now become.[47] As that discipline emerged, it was largely a matter of mapping types of vegetation and discriminating plant communities; as such, it seemed merely an extra wing of natural history and recruited some able amateur taxonomists. In continental Europe, this approach evolved into nothing more alien than the parallel classificatory system of phytosociology. When that proved hard to apply in the fluid conditions of the Atlantic edges, British ecologists opted for the American emphasis on vegetation development and succession, but with a slant of their own toward understanding the underlying physiological mechanisms, a shift that excluded the amateur following. Contrary to their expectation, though, ecologists failed to capture plant geography from the taxonomists: The relationship between environment and community proved too complex to be put on a physiological basis.[48] In the end, both of these substitutes thus turned out to be culs-de-sac. In any event, though, the field museum tradition fulfilled too basic a function, and its routines had such a perpetual appeal, that it was unlikely to have been abandoned on any major scale. Although it had lost its central position in science, it had much more inherent vitality than its critics suspected.

[45] D. E. Allen, *The Naturalist in Britain: A Social History* (London: Allen Lane, 1976), p. 192.

[46] E. L. Palmer, "Fifty Years of Nature Study and the American Nature Study Society," *Nature Magazine*, 50 (1957), 473–80; E. W. Jenkins, "Science, Sentimentalism or Social Control? The Nature Study Movement in England and Wales, 1899–1914," *History of Education*, 10 (1981), 33–43.

[47] See Pascal Acot, Chapter 24, this volume.

[48] Joel B. Hagen, "Evolutionists and Taxonomists: Divergent Traditions in Twentieth-Century Plant Geography," *Journal of the History of Biology*, 19 (1986), 197–214.

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Disorienting though the irruption of biology was for the natural history community, it was not nearly as divisive as an issue that surfaced within the community's own ranks in the same period. This was a reaction against collecting on ethical grounds. A conscience about the depredations of collecting and its apparent cruelty had emerged in the 1830s, but the social prestige of field sports and the mass production of guns had combined to smother those early murmurings. The prevailing attitude eventually changed because of two horrifically destructive fashions: first the extraordinary fern craze in Britain and then the international demand for the plumage of birds for millinery.[49] The second of these, more commercial and provocative of deeper emotions, gave rise to what came to be known in the United States as the "Feather Fight" and called into being a series of protest groups on both sides of the Atlantic that gave rise to the Society for the Protection of Birds in Britain and the National Association of Audubon Societies in the United States in 1891 and 1905, respectively.[50] Particularly notable was the prominent part women played in those groups.

The initial pieces of legislation achieved by this outbreak of protectionist campaigning proved hard to enforce, and some of the American measures were even repealed. The struggle was consequently drawn out. Several other developments, however, coincided to boost the fortunes of protectionism: a fashion for feeding wild birds, the simplification of photography, the production of compact, "streamlined" handbooks, and the general availability of more powerful field glasses.[51] By 1900, watching birds instead of shooting them was fast becoming the accepted approach in ornithology in northwest Europe and North America. The more scientific, however, were deeply distrustful of sight records and were won over only in the 1920s, when the inculcation of a drill in noting field characters succeeded in raising the general standard sufficiently. This was the contribution preeminently of Ludlow Griscom in the United States and the Rev. F. R. C. Jourdain in Britain. By contrast, it took half a century longer for a similar degree of constraint to become general among botanists, and the difficulty of identifying most kinds of insects without capturing, if not killing, them kept entomology immune from the anticollecting fervor.

[49] D. E. Allen, *The Victorian Fern Craze: A History of Pteridomania* (London: Hutchinson, 1969); D.E. Allen, "Changing Attitudes to Nature Conservation: The Botanical Perspective," *Biological Journal of the Linnean Society*, 32 (1987), 203–12; Robin W. Doughty, *Feather Fashions and Bird Preservation* (Berkeley: University of California Press, 1975).

[50] William Dutcher, "History of the Audubon Movement," *Bird-Lore*, 7 (1905), 45–57; F. E. Lemon, "The Story of the R. S. P. B.," *Bird Notes and News*, 20 (1943), 67–8, 84–7, 100–2, 116–18; T. Gilbert Pearson, "Fifty Years of Bird Protection," in *Fifty Years' Progress of American Ornithology, 1883–1933*, ed. Frank M. Chapman and T. S. Palmer (Lancaster, Pa.: American Ornithologists' Union, 1933), pp. 199–213; Frank Graham, Jr., *The Audubon Ark: A History of the National Audubon Society* (New York: Knopf, 1990).

[51] Allen, *Naturalist in Britain*, pp. 230–5.

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## INTERNAL SALVATION

Meanwhile natural history had been discovering some scientifically fruitful alternatives to collecting. The origin of one of these also lay in the 1830s, when Britain's two national botanical societies both instituted the exchanging of herbarium specimens as a membership attraction. The networks that arose from this were used by the plant geographer H. C. Watson as a means of building up a more precise picture of the range of each species of vascular plant accredited to the wild flora of England, Wales, and Scotland. The high cost of printing maps led Watson to adopt a system of indicating distributions numerically. Dividing the country into successively smaller units as the mounting quantity of records made that feasible, he published in 1873–4 a compendium documenting the evidence for the occurrence of each species in any of 112 "vice-counties" (as he termed his ultimate unit).[52] Watson's method was subsequently copied for working out the distribution in Britain of breeding birds and of land and freshwater molluscs. More informative dot maps had meanwhile been introduced in Germany by a professor at the University of Giessen, Hermann Hoffmann, who in publishing a series of such maps for the flora of Upper Hesse in the 1860s produced the first ever for Europe as a whole.[53] Dot mapping became well established in Scandinavia by 1900, culminating fifty years later in Erik Hultén's *Atlas över Karlvaxterna i Norden* (Atlas of the Distribution of the Vascular Plants of Northwestern Europe). Inspired by that and by a major Dutch cooperative project in 1930–5 under the auspices of the Instituut voor het Vegetatie-Onderzoek van Nederland, the Botanical Society of the British Isles pioneered the use of automatic data processing in 1954–62 to produce an *Atlas of the British Flora* – and a supplementary one of the more "critical" taxa in 1968.[54] The product of a *levée en masse* of an army of amateurs working under academic direction, this inspired a string of national distribution atlases of numerous zoological and botanical orders produced by similar cooperative networks. After 1964, the main administrative burden was borne by Britain's eventual equivalent of the U.S. Biological Survey, the government-funded Nature Conservancy.

Proceeding in parallel with this succession of mapping initiatives have been similarly large-scale cooperative ventures in other types of work related to the study of birds. These have been the more impressive for having been achieved in a field long ignored by academic biology. The near coincidence on both sides of the Atlantic of several of the stages through which this line

[52] J. E. Dandy, *Watsonian Vice-Counties of Great Britain* (London: Ray Society, 1969).

[53] S. M. Walters, "Distribution Maps of Plants – An Historical Survey," in *Progress in the Study of the British Flora*, ed. J. E. Lousley (London: Botanical Society of the British Isles, 1951), pp. 89–95.

[54] A. W. Kloos, "The Study of Plant Distribution in Holland," in *The Study of the Distribution of British Plants*, ed. J. E. Lousley (Oxford: Botanical Society of the British Isles, 1951), pp. 64–7; Faase, *Between Seasons and Science*, pp. 58–62; Allen, *Botanists*, pp. 153–8.

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of work passed is striking and suggests a degree of cross-national contact that has yet to be revealed by historical study.

As early as 1843, the Académie Royale des Sciences of Brussels, as part of a program of studying various kinds of periodic phenomena, instigated by its secretary, the statistician Adolphe Quetelet (1796–1874), began sponsoring the collection of data on certain seasonal bird migrations. Other European countries, most notably Russia and Sweden, followed the Belgian lead. From 1875 onward, intensive mass attacks on the mystery of migration were mounted in Germany, Austria-Hungary, Britain, and North America, in the last two of which the help of lighthouse keepers was extensively enlisted.[55] These surveys were ambitious: In the United States, under the dynamic Clinton Hart Merriam (1855–1942), a national chain of observers raised by a circular mailed to eight hundred newspapers operated under thirteen district supervisors.[56] But for the most part they produced merely further sets of incomplete and unreliable timetables. What was really needed was systematic observing at certain favorable spots and, better still, a means of getting the birds to reveal their movements themselves. Around 1900, inspired by the work of Heinrich Gätke (1814–ca. 1890) on the German islet of Heligoland, regularly manned bird observatories began to be established, first on the Baltic and then around the North Sea and elsewhere. Coinciding with this, a fall in the price of aluminum permitted the use of leg rings of the requisite lightness, a solution that came from Denmark. Major bird-banding schemes followed in 1909 almost simultaneously in the United States, Britain, and France.[57]

Having experienced the stimulus and realized the advantages of "network research," ornithologists' ambitions rose further. Thanks to the wide readerships secured by Frank Michler Chapman (1864–1945) through his journal *Bird-Lore* in the United States and by Harry Forbes Witherby (1873–1943) through his *British Birds*, population counts gradually built up strong followings from 1900 onward. In the United States, the work was taken over in 1914 by the U.S. Biological Survey but soon languished after the early death of Wells Woodbridge Cooke (1858–1916), the staff member who had propelled it.[58] In Britain, however, national censuses of individual bird species were attracting over a thousand volunteer enumerators by 1931 and bringing the realization that in "mass observation" the amateur community had perfected a technique with considerable research potential.[59] As state takeovers of major scientific initiatives were still rare in Britain, the decision was taken

[55] Erwin Stresemann, *Ornithology from Aristotle to the Present*, 2nd ed. (Cambridge, Mass.: Harvard University Press, 1975), p. 334.
[56] Barrow, *Passion for Birds*, pp. 230–5.
[57] Harold B. Ward, "The History of Bird Banding," *Auk*, 62 (1945), 256–65; W. Rydzewski, "A Historical Review of Bird Marking," *Dansk Ornithologisk Forening Tidsskrift*, 45 (1951), 61–95.
[58] Barrow, *Passion for Birds*, pp. 170–1.
[59] Bruce Campbell, "Co-operation in Zoological Studies," *Discovery*, 11 (1950), 328–50.

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to establish a permanent institute specializing in this type of work. Under the misleading name of the British Trust for Ornithology and after a financially perilous start in 1932, it has gone on to flourish. The amateur community had thus achieved the possibly unique feat, at least in the life and earth sciences, of independently generating a self-sustaining research enterprise.

## CONVERGENCE

In its new guise of bird-watching, ornithology – both in North America and in the northern half of Europe – gained followings of a size that its sister studies could never expect to equal and enjoyed a social respectability that they could only envy. This respectability came from the aura of field sports, which outlived its newly gunless character. From the 1930s onward, the whole of the extralaboratory community, professionals and amateurs alike, began to recover the confidence and sense of direction it had lost half a century earlier. It was more than just the spontaneous efflorescence being displayed in ornithology that was responsible for this. By then, the rather negative wave of protectionist fervor had been integrated successfully and, under the influence of academic ecology, was maturing into a more thoughtful conservation movement.[60]

Another source of reinvigoration was a convergence at last between biology and natural history. The first hints of this came around 1910, when Julian Sorell Huxley (1887–1975), a then rare instance of a biologist who was also a field naturalist, pioneered the scientific study of vertebrate behavior. In 1916, during a teaching interlude in Texas, he urged American ornithologists to direct their emerging observational networks at problems of scientific moment and thereby reduce the polarization between the worlds of the field and the laboratory. Huxley soon after returned to Oxford and helped to enthuse a group of students there to do the same.[61] At the same time, the marriage of genetics to plant taxonomy had taken hold in Scandinavia under the name of "genecology," which gradually widened into an international movement to bring experimental approaches to bear on traditional systematics. Proclaimed as the New Systematics in 1940,[62] this had a major impact on natural history before being extinguished by the swing to molecular biology in the 1960s and the near elimination of teaching and research in taxonomy in the universities.

[60] See Stephen Bocking, Chapter 32, this volume.
[61] Julian Huxley, *Memories* (London: Allen and Unwin, 1970), pp. 84–90; Julian Huxley, "Bird-Watching and Biological Science: Some Observations on the Study of Courtship," *Auk*, 35 (1916): 142–61, 256–70; J. B. Morrell, *Science at Oxford: 1914–1939* (Oxford: Clarendon Press, 1997), pp. 284–5, 299.
[62] Julian Huxley, ed., *The New Systematics* (Oxford: Clarendon Press, 1940).

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Some expected that a greater degree of convergenee would occur in the complementary direction than has proved to be the case. Hopeful pointers were seen in the voluntary enrollment by amateur naturalists in extramural university courses on genetics and physiology,[63] while the huge expansion in higher education seemed to promise a greatly increased influx of trained biologists into the ranks of those pursuing field studies. A biologically sophisticated corps d'elite largely failed to materialize, however. The more scientifically inclined have continued to adhere to nonexperimental taxonomy, recording observations and mapping distributions, and publishing on these topics in appropriate journals alongside professionals, if no longer outnumbering them.[64]

The most important change has been the increased energies now going into conservation. This has been accompanied by the advent of a body of professionals in this specialized sphere, ecologists as well as administrators, which has produced a whole area of interaction between the trained and the untrained. Yet conservation represents only a sideways thrust: It is primarily a matter of education, publicity, and fund-raising, only secondarily concerned with the advancement of scientific knowledge except insofar as that enhances understanding of how best to manage what is conserved and improve the monitoring of biodiversity. Thanks to a combination of factors, however, natural history now has a high public profile. People have greater leisure and there are more and better means of identifying what is seen. Above all, there is the good fortune that wildlife is superbly suited to the new visual media. As a result, the following for natural history, now numbering millions, gives every promise of maintaining the impetus it regained in the second half of the twentieth century. And it seems likely to do so regardless, for the most part, of that other world of experiment and laboratories.

63 Anonymous, "The Limits of the Amateur," *New Scientist*, no. 19 (1957), 7.

64 Marianrie G. Ainley, "The Contribution of the Amateur to North American Ornithology: A Historical Perspective," *Living Bird*, 18 (1979), 161–77, at p. 169.

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# 3

# DISCOVERY AND EXPLORATION

*Roy MacLeod*

In May 2003, from the Baikanur launchpad in the Central Asian deserts of Kazakhstan, British scientists fired a Russian Soyuz-Fregat rocket to launch a probe called the *Mars Express*, intended to determine whether recognizable chemical signs of life could be found in the thin atmosphere and dusty rocks of the red planet. In 1971, the Soviets had been the first to land a probe on Mars, and they were followed by the American *Viking* missions in 1976. In January 2004, the U.S. National Aeronautics and Space Administration (NASA) landed the mobile rovers *Spirit* and *Opportunity* on Mars. These represented huge and dangerous efforts. Of thirty previous missions to Mars, twenty had gone seriously wrong. In 2003, a British probe intended to explore the Martian surface, called – significantly – *Beagle-2*, failed to arrive on the surface. The European mission cost 300 million euros and the American mission ten times as much. Behind all these efforts lies the necessity of securing wide political and public support. Thus, the space missions are performed in "full view of the public." As Alan Wells, director of space research at the University of Leicester, put it, "We are breaking new ground in the public presentation of space science." His duty, in his words, is to be a professor of public relations as well as planetary science.

Today, science speaks to an international public. At the same time, it reflects national ambitions. The process by which scientific cooperation has become overwritten on a wider canvas view of international rivalry is the

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subject of this chapter. Historians speak both of science as an exploratory practice and of exploration as an objective of science. Science derives by definition from the "exploration" of the natural world. During the last three centuries, Western science, in particular, has supplied mission, means, and methods for the exploration of "inner" as well as "outer" space, enabling mankind to become, in Descartes' prose, "masters and possessors of nature." Natural knowledge has become the destroyer of myth. This has happened not only within the laboratory but also in the observation of the universe. In this story, the history of exploration rests comfortably within the history of "discovery."

In the past, the words "discovery" and "exploration" had the connotation of individual effort, referring to first sightings, landfalls, critical experiments, or "findings," or to the institutional practices by which evidence is assessed, and models are confirmed or falsified. The history of discovery is one of uniqueness, serendipity, initial encounter, and personal recognition. Exploration, on the other hand, both celebrates the moment of finding and the mission – including description, classification, and display. "Discovery," moreover, traditionally has a metropolitan referent; but in the act of exploration, the periphery becomes central, and even minor personalities become pivotal, in struggles with nature that are at times both individual and collective, heroic and pedestrian. Exploration is as inclusive as discovery is exclusive. By the act of discovery, we lay claim to possession; but by the act of exploration, we acquire the means by which we establish and trade.

The modern idea of exploration, moreover, takes a wide compassing, in practice referring as much to the efforts of the many as to the few, working not only in the indoor laboratory but in the field, on the seas, and increasingly in space, where models of the universe are tested and understandings confirmed. Within the last century, moreover, the oceans and space have become "laboratory sites," to which access is often limited to the most powerful nations on earth. These spaces have not yet been construed, as in the case of Antarctica, as "common legacies of mankind." It is in the definition of a new politics, exemplified in the Mars expeditions of 2003–4, that the deepest significance – and potential promise – of exploration for the history of science lies.

In a sense, to paraphrase Lytton Strachey, the history of modern scientific exploration can never be fully written because we know too much about it. In our modern age, abundantly familiar with a facsimile *Endeavour* and a virtual starship *Enterprise*, the history of scientific exploration can be read as a series of continuous developments representing an extension of the Enlightenment quest for universal understanding, driven by the interests of trade, commerce, and strategy. "Cataloguing the whole of creation" was not only a divinely ordained mission, in which natural history drew on the sensibilities of art, but also a persuasive project, governed by metropolitan "centres of

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calculation."[1] Such continuities persist. But alongside them have grown significantly new features involving major developments in orientation, organization, and purpose.

## LINKING UNIVERSES

Where does the modern period of scientific exploration begin? Its history during the last four hundred years unfolds within a continuous cultural space, producing features that remain present today. Among them, two are noteworthy. First, the period is aptly described as a period in which science, practiced by Europeans, sought to "remove blanks" in its cumulative record of nature, using expeditions to gain more precise information about the world and its peoples.[2] Moreover, by the end of the period, scientific exploration acquired a professional agenda. In departing from a centuries-old mixture of high resolve, commercial crusade, and unguided curiosity, European science set out to achieve specific objectives. The concept of exploration itself became "objectified." In an age of professionalization, it seemed to minimize political bearings. In the words of one author, "The entire purpose of most expeditions is to conduct fresh scientific research. This means that the expedition findings must 'add' to existing knowledge."[3] Adding to knowledge, removing speculation, became its principal raison d'être.

Since the 1970s, a generation of historians has become interested in the geopolitical constructions that grew from these objective acts and practices. Overall, it is clear that scientific expeditions embarked to solve problems left unsolved by philosophers. One such problem was the supposed existence of a northwest passage to Asia, a prospect that had exercised the minds of Europeans since the fall of Constantinople.[4] From the sixteenth century, England and France sought ways around the Straits of Magellan, the "southwest passage," possession of which gave Iberia control of the East Indies. But Europeans looked with equal zeal for a "northwest passage" over the top of the Americas and through the northern latitudes. The quest that led Henry Hudson (d. 1611) up the eponymous river in 1609 inspired

[1] See the phrase made famous by Bruno Latour, *Science in Action* (Milton Keynes: Open University Press, 1987).

[2] See Peter Whitfield, *New Found Lands: Maps in the History of Exploration* (New York: Routledge, 1998), p. 187.

[3] John Hemming, *Reference Sources for Expeditions* (London: Royal Geographical Society, 1984).

[4] See Glyn Williams, *Voyages of Delusion: The Search for the North West Passage in the Age of Reason* (London: HarperCollins, 2003). The literature has a distinguished provenance. See Samuel Eliot Morison, *The European Discovery of America: The Northern Voyages,* A.D. *500–1600* (New York: Oxford University Press, 1971); John L. Allen, "The Indrawing Sea: Imagination and Experience in the Search for the Northwest Passage, 1497–1632," in *American Beginnings: Exploration, Culture and Cartography in the Land of Norumbega*, ed. Emerson W. Baker et al. (Lincoln: University of Nebraska Press, 1994), pp. 7–36.

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navigators during the next four hundred years.[5] By the nineteenth century, however, these motives had been recast. The objective was no longer commercial but the solution of a problem, the discovery of the passage itself, which required (and frustrated) the skills of the most powerful powers on earth.

The solution of scientific problems required matching ends to means. The answer to a conceptual or geographical question awaited the arrival of an appropriate geopolitical opportunity, combined with the necessary technology and political will. Thus, James Cook's (1728–1779) three eighteenth-century voyages to the Pacific were charged with resolving geographical questions dating from the time of Ptolemy. But to confirm or deny the existence of a southern continent and to chart newly discovered lands involved making empirical observations that British naval mastery made feasible.[6] Victory over France in the Seven Years' War gave England the moment and English science the opportunity. Some of England's most notable successes were in the Pacific, but many land-based problems – for example, the determination of the source of the Nile, the course of the Niger, the cause of the Himalayas, and the unique fauna of Australia – were all made easier by the access that Britain enjoyed as an imperial power.

During the nineteenth century, changes in the definition of what constituted a "scientific problem" became increasingly clear. If, by 1800, Western science possessed a reliable set of methods and instruments and an objective rationale for exploration, then by 1900, the institutions of science and improvements in marine technology had taken command of the expedition idea and had given it fresh capability and intent. To borrow a phrase from Peter Galison, the "scientific expedition" came to command a new "trading zone" between observation and theory, in which shipboard skills complemented the laboratory bench.[7] Together with natural and university museums of science, whose interests they increasingly served, the scientific expedition became a *habitus*, a "place of knowledge."[8] The structure, organization, and eventual dissemination of that knowledge created a new space for science.[9] From the fifteenth century, the "autopic" sensibility gave European science dominion over the earth. When Western travelers brought back

[5] Robert G. Albion, "Exploration and Discovery," *Encyclopedia Americana*, International Edition (New York: Americana, 1979), vol. 10, p. 781.

[6] Alan Frost, *The Voyage of the "Endeavour": Captain Cook and the Discovery of the Pacific* (Sydney: Allen and Unwin, 1996).

[7] Peter Galison, "The Trading Zone: Coordination between Experiment and Theory in the Modern Laboratory," paper presented at International Workshop on the Place of Knowledge, Tel Aviv, May 1989.

[8] See Michel Foucault, *The Order of Things* (London: Tavistock Press, 1970), pp. xvii–xviii; Adi Ophir and Steven Shapin, "The Place of Knowledge: A Methodological Survey," *Science in Context*, 4 (1991), 3–21.

[9] For the expanding museum, see Dorinda Outram, "New Spaces in Natural History," in *Cultures of Natural History*, ed. N. Jardine, J. A. Secord, and E. C. Spary (Cambridge: Cambridge University Press, 1996), pp. 249–65.

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knowledge and specimens of plants, animals, and peoples, they were classified and cataloged at Lisbon and Cadiz or at Kew and the Jardin des Plantes, Berlin and Hamburg, Boston, and Sydney. For Victorians, however, the instrument by which the world was to be known was the expedition. By the late nineteenth century, with the rise of universities, museums, and foundations as sponsors and beneficiaries, the expedition became a major agent of Western influence, creating new disciplines, exploring new ideas, and establishing new forms of cultural appropriation.[10] Eventually, with the twentieth century came the representation of science itself as a symbolic act of perpetual exploration. In the memorable phrase of Vannevar Bush, science is humanity's "endless frontier" – knowing no boundaries or limits, with its public justification self-evident.

Nothing in the history of exploration is more conspicuous than its celebration of human achievement. The nineteenth century witnessed an incarnation of the ancient mariner. Discovery became the ambition of the scientific traveler, and the "exploration society, his vehicle."[11] The "expeditioner" became a familiar figure, repeated in a thousand portraits, photographs, and films: "Supreme enthusiasm, tempered with infinite patience, and a complete devotion to truth; the broadest possible education; keen eyes, ears and nose." So wrote the naturalist William Beebe (1877–1962), a model of the modern man,[12] who saw in "science and exploration . . . an answer for many men, uncomfortable with themselves, restless, confined by home relations and definitions, seeking an excuse to escape into the unknown."[13] With adventure came fame. The German explorer Heinrich Barth (1821–1865) spoke of the unremitting desire to be "first" – perhaps the commonest criterion of science. As a contemporary put it, "The comity of explorers has adopted the rule of the more scientific observers of nature, and holds it for law everywhere that he who first sees and first announces shall also give the name."[14] In Barth's case, laurels went to those who first penetrated "into unknown regions, never before trodden by European foot."[15] The indigenous inhabitant remained, all too often, an artifact; perhaps an opportunity, at most a distraction.

[10] See, for example, Andre Gunder Frank, "The Development of Underdevelopment," *Monthly Review*, 18 (1966), 17–31; Andre Gunder Frank, S. Amin, G. Arrighi, and I. Wallerstein, *Dynamics of Global Crisis* (London: Macmillan, 1982). For an ironic account, see Norman Simms, *My Cow Comes to Haunt Me: European Explorers, Travelers and Novelists Constructing Textual Selves and Imagining the Unthinkable in Lands and Islands beyond the Sea, from Christopher Columbus to Alexander von Humboldt* (New York: Pace University Press, 1996).

[11] Peter Raby, *Bright Paradise: Victorian Scientific Travelers* (London: Pimlico, 1996).

[12] Quoted in Victor von Hagen, *South America: The Green World of the Naturalists: Five Centuries of Natural History in South America* (London: Eyre and Spottiswoode, 1951), p. xvii.

[13] Eric Leed, *Shores of Discovery: How Expeditionaries Have Constructed the World* (New York: Basic Books, 1995), p. 12.

[14] Elisha Kent Kane, *Arctic Explorations: The Second US Grinnell Expedition in Search of Sir John Franklin* (Philadelphia: Charles and Peterson, 1856).

[15] Heinrich Barth, *Travels and Discoveries in North and Central Africa, Being a Journey Undertaken in 1849–1855* (London: Frank Cass, reprint 1965), vol. 2, p. 454, cited in Leed, *Shores of Discovery*, p. 213.

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Today, self-congratulatory eurocentrism warrants self-conscious rebuke. But there is no doubt that the process of seeing, mapping, and impressing a European identity on places otherwise "unknown to science" held a compelling fascination. This narrative was reflected in the historiography of great power rivalries and imperial conquest. The scientific expedition drew on the language of the military expedition and the heroism of the expeditionary force. For much the same reason, an active commitment to scientific exploration was, to some, the highest measure of a nation's claim to civilization. This language of the "civilizing mission" reveals as much about what it omitted as about what it claimed. With the end of the Great War, the exploration idea was transformed from a cultural undertaking to a political one, quickening the pace to complete the picture of the universe.

## SCIENCE AND THE EXPANSION OF EUROPE

Scientific exploration was not born of the nineteenth century, but in that century it came of age. Historians view the period as one of excitement for Europeans, who, having mapped their own continent, looked for new worlds to conquer. It was a period noteworthy for the "completion of details" of two continents (North and South America), the complete penetration of two others (Africa and Australasia), and the partial penetration of the sixth (Antarctica), as well as for scientific voyages "devoted largely to a study of the oceans."[16] Knowledge of Europe was no longer sufficient to explain the world. The act of exploration, never far removed from adventure, acquired a new relationship with fiction as well as fact. In 1800, much of the earth's surface remained speculative. If Africa was the Dark Continent, most Europeans knew little of Asia, or even of the Americas, and nothing at all of Antarctica. Scarcely a century later, European science was as ubiquitous as European commerce. In a short time, expeditions produced a greater understanding of geology, biology, and culture than the world had ever seen. With the next fifty years, the changing nature of exploration brought with it new combinations of private and public initiative, inspired by the formation of new disciplines, new technologies, and soaring public interest in the "conquest" of the oceans and the heavens.

With this impulse traveled assumptions dating from antiquity. Since Alexander the Great, European empires had sought to "capture" knowledge of conquered peoples and places, winds and tides, rivers and seas.[17] With

[16] Sir James Wardle and Harold E. King, "Exploration," *Chambers Encyclopedia* (1973), vol. 5, pp. 500–1.

[17] See J. H. Perry, *The Spanish Seaborne Empire* (Berkeley: University of California Press, 1990); Oskar Spate, *The Spanish Lake*, 2 vols. (Canberra: ANU Press, 1979); Carlo Cipolla, *Guns and Sails in the Early Phase of European Expansion, 1400–1700* (London: Collins, 1966); Margarette Lincoln, ed., *Science and Exploration in the Pacific: European Voyages to the Southern Oceans in the Eighteenth Century* (London: National Maritime Museum, 1998).

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knowledge of universal principles came an interest in distant nature. The governors of Solomon's House in Francis Bacon's famous utopia, The New Atlantis, entrusted its "merchants of light" to "sail into foreign countries," to trade in knowledge, and to bring it to the service of wise government.[18] By the late eighteenth century, the authors of the *Encyclopaedia* contemplated a world of relationships in which natural knowledge held a commanding place. What educated Europeans had for centuries retained in the "geography of the imagination,"[19] the essence of myth and legend, was transformed into a wish to describe the earth, the skies, and the seas, whose classification and order were governed by the eye rather than the book.[20] With knowledge of physical nature would come knowledge of social nature – of societies distant and engaging, sophisticated and primitive – their artifacts collected in the private "cabinets" of the "enlightened," wealthy, and wise. In England, the introduction of new crop plants and medicines from the New World, which once had made travelers into gardeners, now turned scholars into natural historians, just as plantation wealth transformed the English landscape.[21] The practices of the enlightened were idealized as a way of knowing, celebrated by a "republic of letters," courting the patronage of cosmopolitan taste. Their institutions served a moral economy that privileged Europe. In making knowledge European, the argument went, science would make it universal and of benefit to all.

This optimism celebrated the prospects of a class of persons devoted to travel and exploration. The period 1770–1835 has been described as the age of the "exploration narrative. This contributed to a process by which Europeans came to think of themselves as imperial centres." Indeed, ideas of empire were shaped by travel writing as travelers institutionalized ideas of racial inferiority. In 1754, Jean-Jacques Rousseau (1712–1778) complained that Europe had accumulated little objective knowledge of the world in the three centuries since it had begun colonizing and Christianizing, and organizing its trade. The reason, he suggested, was that expeditions had been dominated by four classes of men – sailors, merchants, soldiers, and missionaries. What was needed was a new class – naturalists – men eager to fill minds rather than purses.[22] Charles de Brosses (1709–1777), in 1756, similarly called on natural philosophers to serve their country by serving science first.

[18] See Francis Bacon, "The New Atlantis," in *Francis Bacon: Selections*, ed. Brian Vickers (Oxford: Oxford University Press, 1996). In the extensive literature on Bacon, see Lisa Jardine and Alan Stewart, *Hostage to Fortune* (London: Victor Gollancz, 1998); Julian Martin, *Francis Bacon, the State and the Reform of Natural Philosophy* (Cambridge: Cambridge University Press, 1992).

[19] Daniel Boorstin, *The Discoverers* (New York: Random House, 1983).

[20] Anthony Grafton, *New Worlds, Ancient Texts: The Power of Tradition and the Shock of Discovery* (Cambridge, Mass.: Harvard University Press, 1992), pp. 217–23.

[21] W. Bray, "Crop Plants and Cannibals: Early European Impressions of the New World," *Proceedings of the British Academy*, 81 (1993), 289–326, see p. 292.

[22] Leed, *Shores of Discovery*, p. 10.

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His message was as perceptive as it was prescriptive. Knowledge had always been an instrument of the state. The eighteenth century opened and closed in the belief that voyages of exploration served both commercial and military justifications. In the 1740s, John Campbell promoted an expedition to the unknown continent of Terra Australis as vital to making England "a great, wealthy, powerful and happy people."[23] And what science proposed, the state did not reject. In France and England, science was married to the navy and the army.[24] Following the Seven Years' War (1755–63), France's loss of its colonial empire in the New World transferred rivalries with England from continental Europe, India, and the Caribbean to the Pacific, Asia, and Africa. A fuller knowledge of the sea and the Orient would enable France to lay intellectual siege to the sciences of the British Empire.[25] Portugal was not slow to see the same logic, although reforms at home were not enough to secure initiatives abroad.[26]

Perhaps the first truly scientific journey in Europe was the dual French expedition of 1735 sent to Lapland and the equator to test rival Newtonian and French ideas about the sphericity of the earth.[27] But the first great age of scientific expeditions is commonly said to begin in the Pacific, with the climacteric voyages of Louis Antoine de Bougainville (1729–1811, traveled 1766–9), Jean-François de La Perouse (1741–1788, traveled 1778–85), Samuel Wallis (1728–1795, traveled 1766–8), Philip Carteret (1733–1796, traveled 1768), Captain James Cook (three expeditions, 1769–80) and his successors, George Vancouver (1757–1798, traveled 1791–5), Matthew Flinders (1774–1814, traveled 1801–3), and Antoine de Bruni d'Entrecasteaux (1739–1793, traveled 1791–3). On these voyages, naturalists, astronomers, and natural philosophers joined naval expeditions in their own right.[28] With Cook on the *Endeavour* were not only Joseph Banks (1743–1820) and his assistant Daniel Solander (1736–1782) but also the Royal Society's appointed astronomer, Charles Green.[29] Scientific draughtsmen were on British voyages long before Cook's and the presence of a natural scientist did not in itself signify scientific activity. Nor is the story limited to Britain and France. As Iris Engstrand has shown, Spain feared the

[23] See Sverker Sörlin, "Ordering the World for Europe: Science as Intelligence and Information as Seen from the Northern Periphery," *Osiris*, 15 (2000), 51–69, see p. 55.
[24] See the recent conference on "Science and the French and British Navies, 1700–1850," National Maritime Museum, London, April 30–May 3, 2001.
[25] Paul Carter, "Looking for Baudin," in *Terre Napoleon: Australia through French Eyes, 1800–1804*, ed. Susan Hunt and Paul Carter (Sydney: Historic Houses Trust, 1999), pp. 21–34.
[26] William Joel Simon, *Scientific Expeditions in the Portuguese Overseas Territories (1783–1808), and the Role of Lisbon in the Intellectual-Scientific Community of the Late Eighteenth Century* (Lisbon: Instituto Investigacao Cientifica Tropica, 1983); Daniel Banes, "The Portuguese Voyages of Discovery and the Emergence of Modern Science," *Journal of the Washington Academy of Sciences*, 78, no. 1 (1988), 47–58.
[27] Raby, *Bright Paradise*, p. 4.
[28] See Kapil Raj, "Les Grands Voyages de Découvertes," *Recherche*, no. 324 (October 1999), 80–4.
[29] Edward Duyker, *Nature's Argonaut: Daniel Solander, 1733–1782* (Melbourne: Melbourne University Press, 1999).

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impending loss of *el lago español* (the Spanish lake) and during the eighteenth and early nineteenth centuries sent survey expeditions to New Spain, tracking from the West Indies to Mexico, the Californias, and the Pacific Northwest. From the first of these (the Royal Scientific Expedition, 1785–1800) came the Botanical Garden of Mexico City, as well as much intelligence on English and French movements in the Pacific.[30]

Throughout the late eighteenth century, science and strategy were not only connected but interdependent. Cook's first voyage to the Pacific, in 1769, was formally prompted by an international agreement to obtain measurements of the transit of Venus for the purpose of calculating the astronomical unit (the distance from the earth to the sun). But it was also driven by strategic considerations, of which the first was to deny France the continent of New Holland and any other unclaimed lands (whether occupied or not) in the southern latitudes.[31] The second part of Cook's "secret instructions" held the commercial message. He was required: "Carefully to observe the Nature of the soil and the products thereof; the Beasts and Fowls that inhabit or frequent it, the fishes that are to be found . . . and in case you find any mines, minerals or valuable stones you are to bring home specimens of each, as you may be able to collect."[32]

For the community of English science, the voyage held other justifications. For Joseph Banks, as Nicholas Thomas reminds us, the experience of traveling and exploration not only furnished to the metropolitan gaze objects that were new to "science." The act itself transformed the image of its practitioners from objects of fun and Swiftian satire, mesmerized by the discovery of mere "curios," into "serious" scholars devoted to the careful cataloging of "objective knowledge."[33] The success of exploration – and its tool, the expedition – became an endorsement of the practical benefits of science.

[30] Iris H. W. Engstrand, *Spanish Scientists in the New World: The Eighteenth Century Expeditions* (Seattle: University of Washington Press, 1981); Iris H.W. Engstrand and Donald Cutter, *Quest for Empire: Spanish Settlement in the Southwest* (Golden, Colo.: Fulcrum, 1996).

[31] On the wider aspect, see Roy MacLeod, "Introduction," in *"Nature in Its Greatest Extent": Western Science in the Pacific*, ed. Roy MacLeod and Fritz Rehbock (Honolulu: University of Hawaii Press, 1988); John Gascoigne, *Science in the Service of Empire* (Cambridge: Cambridge University Press, 1998); David Miller, "Joseph Banks, Empire and Centers of Calculation in Late Hannoverian London," in *Visions of Empire: Voyages, Botany and Representations of Nature*, ed. David Miller and Peter Reill (Cambridge: Cambridge University Press, 1996), pp. 21–37; and more recently, John Gascoigne, "Exploration, Enlightenment and Enterprise: The Goals of Late Eighteenth Century Pacific Exploration," in Roy MacLeod (ed.), "Historical Perspectives in Pacific Science," *Pacific Science*, 54, no. 3 (2000), 227–39.

[32] J. C. Beaglehole, *The Exploration of the Pacific*, 3rd ed. (London: Adam and Charles Black, 1966); Richard Henry Major, *Early Voyages to Terra Australis to the Time of Captain Cook as Told in Original Documents* (Adelaide: Australian Heritage, 1963); Derek Howse, ed., *Background to Discovery: Pacific Exploration from Dampier to Cook* (Berkeley: University of California Press, 1990).

[33] Nicholas Thomas, "Licensed Curiosity: Cook's Pacific Voyages," in *The Cultures of Collecting*, ed. John Elsner and Roger Cardinal (Melbourne: Melbourne University Press, 1994), pp. 116–36. See also Nicholas Thomas, *Colonialism's Culture: Anthropology, Travel and Government* (Melbourne: Melbourne University Press, 1994).

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## UNIVERSAL KNOWLEDGE: HUMBOLDT'S COSMOS

If the expeditions of the eighteenth century brought a new sense of detail and specificity, those of the early nineteenth century brought a clearer understanding of the relationships between natural phenomena. The unity of nature acquired an appreciative exponent in Alexander von Humboldt (1769–1859), the German explorer and naturalist, whose most influential work, *Cosmos* (published in four volumes between 1845 and 1858, followed by a posthumous fifth volume in 1862), stimulated Charles Darwin and a generation of scientific travelers.[34] Revered by his countrymen – poet Johann Wolfgang von Goethe called Alexander and his brother, Wilhelm, the "sons of Zeus" – Humboldt was the greatest scientific explorer of the early nineteenth century. Consistent with the ideals of *Wissenschaft* – which became the hallmark of German science – the brothers von Humboldt shared a common purpose. Trained in Göttingen as a mining engineer, Alexander von Humboldt combined the discipline and skill of a careful observer with the unifying tenets of *Naturphilosophie*. His search for "earth knowledge for its own sake" set out to reveal a vision of earth history. Significantly, his greatest philosophical work, *Ideen zu einer Geographie der Pflanzen* (Essay on the Geography of Plants), was dedicated to Goethe.

Unlike his contemporary military surveyors, navigators, naval surgeons, and collectors, Humboldt was interested less in solving empirical problems than in determining interconnections between phenomena. His observations focused on movement, change, and distribution and succeeded in linking previously separate disciplines of geography and history, and the new "global physics,"[35] while extolling the skills of field observation, measurement, thematic mapping, and the study of human landscapes.[36] It was only by direct, personal engagement, he argued, that "we can discover the direction of chains of mountains . . . the climate of each zone, and its influence on the forms and habitats of organized beings."[37] Humboldt was a biographical bridge between the ideologues of the eighteenth century and the *Wissenschaftlers* of

[34] For Humboldt's life, see Wolfgang Hagen Hein, ed., *Alexander von Humboldt: Leben und Werke* (Frankfurt: Weisbecker, 1985); Charles W. J. Withers and David N. Livingstone, eds., *Geography and Enlightenment* (Chicago: University of Chicago Press, 1999). See Alexander von Humboldt, *Cosmos: A Sketch of the Physical Description of the Universe*, with an introduction by Nicolaas Rupke, 2 vols. (Baltimore: Johns Hopkins University Press, 1998).

[35] M. Deltelbach, "Global Physics and Aesthetic Europe: Humboldt's Physical Portrait of the Tropics," in Miller and Reill, *Visions of Empire*, pp. 258–92.

[36] Anne Godlewska, *Geography Unbound: French Geographic Science from Cassini to Humboldt* (Chicago: University of Chicago Press, 1999).

[37] Alexander von Humboldt, *Personal Narrative of Travels to the Equinoctial Regions of America* (1807), cited in Suzanne Zeller, "Nature's Gullivers and Crusoes: The Scientific Exploration of British North America, 1800–1870," in *North American Exploration*, vol 3: *A Continent Comprehended*, ed. John Logan Allen (Lincoln: University of Nebraska Press, 1997), p. 194. See also Alexander von Humboldt and Aimé Bonpland, *Personal Narrative of Travels to the Equinoctial Regions of the New Continent, during the Years 1799–1804* (London: Longman, 1818).

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the nineteenth, inspired by the naturalists Reinhold Forster (1729–1798) and his son Georg, who sailed on Cook's second voyage, and set out to do on land what Cook had performed at sea.

Years of grueling expeditions – first through Austria and Poland and then, in 1800, through the jungles and across the mountains of Central and South America – left its traces in Humboldt's work.[38] What emerged was a revelation of nature as integrated and global, with complex – but not necessarily hostile – patterns of process and change. His journals, tracing man's interactions with nature from revolutionary Latin America to the steppes of Russia, did more than inventory creation. With his French companion, the botanist Aimé Bonpland (1773–1858), he described over 8,000 species previously unknown to science and wrote thirty books, ten about the geography of places he visited. His writings – popularized in his *Ansichten der Natur* (*Aspects of Nature*) – foreshadowed the discipline of physical geography. To him can be credited a modernist, intellectual rationale for scientific exploration.[39]

Nature gave Humboldt more than mere information. *Cosmos*, written for a nonspecialist audience, displays the convictions of a man who, departing from a conservative tradition, saw slavery and injustice in the world and found it repulsive. Rather than favoring "species" nationalism and enthroning hierarchy, Humboldtian science favored a cosmopolitan literacy and a federation of mankind.

Humboldt's politics remain the subject of debate.[40] To some, his scientific position, informed by his politics, represented a radical departure from uncritical utilitarianism, fashionably coded as Baconianism, which prevailed in the English-speaking world. Perhaps his vision was a sophisticated argument for "Enlightened imperialism," as Nicolaas Rupke has recently suggested.[41] But some have found in his vision of "dramatic, extending nature" modern respect for indigenous knowledge, and the origins of environmental activism. His work in South America was widely influential in France, Germany, and the United States. In England, one of his admirers was Mary Somerville, who, like Humboldt, saw the purpose of science as embracing, rather than fragmenting, the domains of geology, botany, zoology, and astronomy. He inspired what Susan Faye Cannon has called "the accurate, measured study of widespread but interconnected real phenomena in order to find a definite law and a dynamical cause."[42]

[38] For an appreciation of his influence, see the special issue of *Quipu*, especially Luis Carlos Arboleda Aparicio, "Humboldt en la Nueva Granada: Hipsometria y territorio," *Quipu*, 13, no. 1 (2000), 53–67.

[39] Deltelbach "Global Physics and Aesthetic Europe," pp. 258–92.

[40] See Margarita Bowen, *Empiricism and Geographical Thought: From Francis Bacon to Alexander von Humboldt* (Cambridge: Cambridge University Press, 1981).

[41] Rupke, Introduction to Humboldt, *Cosmos*.

[42] Susan Faye Cannon, *Science in Culture: The Early Victorian Age* (New York: Science History Publications, 1978), p. 105.

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Humboldt's message, which mesmerized the world of science, also galvanized interest in the "periphery." What might be called the "centrality of the periphery" became a prominent trope, radiating from Humboldt and extending to the distant areas of Africa, the Middle East, Australasia, and the Pacific. Not always were expeditions successful. A mission sent by the London Bible Society to Palestine, with the goal of locating evidences in nature to endorse the "veracity" of scripture, met with ambiguous results. Darwin's experience of nature in Australia – where, as he recorded in his *Journals*, it seemed that a different Creator had been at work – showed the world to be infinitely more diverse than Europeans realized. It was this recognition, together with a continuing desire to make the unknown knowable, that stimulated the famous global scientific expeditions of the mid-nineteenth century – expeditions that ultimately adopted a Humboldtian style, with long, repeated visits, extensive publication, scholarly backing, and wide publicity.

## SCIENCE AND NATIONAL GLORY

The voyages of Cook and Bougainville became the models for national scientific expeditions in the early nineteenth century, where science and power converged. The expedition was a convenient tool of empire, a symbol of civilization, and an instrument of research.

Until the close of the Napoleonic Wars in 1815, scientific voyages had explicit military objectives. Napoleon's invasion of Egypt – accompanied by a celebrated mission of savants (itself inspired by the example of Alexander the Great) – gave science an imperial presence. The establishment of the Institut d'Egypt, based on the model of the Institut de France, was a direct play to cultural hegemony.[43] In 1800, Napoleon continued the policy of the ancien ŕegime in sending Nicolas Baudin (1754–1823), in the corvettes *Géographe* and *Naturaliste*, to the Great South Land – Flinders did not christen the continent "Australia" until 1803 – to collect specimens for the Muséum d'Histoire Naturelle and intelligence of British intentions. His ships were meticulously fitted out as floating laboratories, observatories, and conservatories, complete with plans drawn up by the Sociéte des Observations de l'Homme, under the guidance of Georges Cuvier (1769–1832). The expedition foundered in mutiny and disease, but remnants returned to Paris with two hundred stuffed birds, sixty-five quadrupeds, and forty thousand other specimens – ten times more than Cook's second voyage and enough for Josephine to create at Malmaison a menagerie of rare animals and a park of exotic shrubs.[44]

[43] See J. Christopher Herald, *Bonaparte in Egypt* (London: Hamish Hamilton, 1992).

[44] Carter, "Looking for Baudin." See also Frank Horner, "The Baudin Expedition to Australia, 1800–1804," in *Baudin in Australian Waters: The Artwork of the French Voyage of Discovery to the Southern Lands, 1800–1804*, ed. Jacquelin Bonnemaines, Elliott Forsyth, and Bernard Smith (Melbourne:

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Of course, as Marie Nöelle Bourguet has noted, "The interests of science and the interests of the empire did not [always] go . . . at the same pace."[45] But they had a fateful symmetry. As Richard Burkhardt has noted, Napoleon's defeat had profound implications for science in France, requiring the Muséum d'Histoire Naturelle and its director, Georges Cuvier, to establish relations with a new government and to restore the museum's reputation as a collector, as distinct from a confiscator, of natural history specimens from other countries. Cuvier, who considered fieldwork as tributary to theory, oversaw the museum's reinstallation of its earlier tradition of naturalist-voyagers.[46] Eventually, the museum renewed the eighteenth-century practice of using ships as floating laboratories rather than limiting them to passively collecting specimens for metropolitan cabinets.

The English were no less keen to associate science, exploration, and strategic interest from the Asiatic Society of Bengal to the austral Pacific.[47] In 1801, the Admiralty sent Lieutenant Matthew Flinders (1774–1814) on HMS *Investigator* to forestall a likely French presence on the continent claimed by Cook and called New South Wales.[48] With Flinders sailed the twenty-one-year-old naturalist Robert Brown (1773–1858) and the botanical artist Ferdinand Bauer, whose 2,000 drawings – an "extraordinary fusion of art and science" – became the most visible product of the greatest voyage of natural history ever sent to Australia.[49] That same year, Thomas Jefferson, president of the new United States of America, launched the first North American scientific expedition, under Meriwether Lewis (1774–1809) and William Clark (1770–1838), to survey and catalog the western reaches of the continent.

French and English men of science were almost by definition at war, regardless of what later historians have glossed,[50] but "enemy" naturalists often made common cause. Rarely – as when Flinders and Baudin accidentally met in Encounter Bay, off the coast of South Australia, an area known as "Terre Napoleon" – were national rivalries allowed to interrupt the smooth

Oxford University Press, 1988). See also Frank Horner, *The French Reconnaissance: Baudin in Australia, 1801–1803* (Melbourne: Melbourne University Press, 1987).

45 M.-N. Bourguet, "La Collecte du monde: Voyage et histoire naturelle (fin XVIIème siècle – début Xème siècle)," in *Le Muséum au premier siècle de son histoire*, ed. Claude Blanckaert et al. (Paris: Muséum National d'Histoire Naturelle, 1997), pp. 163–96, at p. 193. See also Maurice Crosland, "History of Science in a National Context," *British Journal for the History Science*, 10 (1977), 95–115.

46 Richard W. Burkhardt, Jr., "Naturalists' Practices and Nature's Empire: Paris and the Platypus, 1815–1833," *Pacific Science*, 55 (2001), 327–43.

47 C. A. Bayly, *Empire and Information: Intelligence Gathering and Social Communication in India, 1780–1870* (Cambridge: Cambridge University Press, 1996).

48 See Glyndwr Williams and Alan Frost, eds., *From Terra Australis to Australia* (Melbourne: Oxford University Press, 1988); William Eisler, *The Furthest Shore: Images of Terra Australis from the Middle Ages to Captain Cook* (Cambridge: Cambridge University Press, 1995).

49 Peter Watts, ed., *An Exquisite Eye: The Australian Flora and Fauna Drawings of Ferdinand Bauer, 1801–1820* (Sydney: Museum of Sydney, 1997).

50 Gavin de Beer, *The Scientists Were Never at War* (London: Nelson, 1962).

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flow of science.[51] When they did, the sin was never forgiven. Against Baudin's instructions, for example, his assistant, François Peron, turned his scientific "spy glass into the report of a spy."[52] Flinders succeeded in establishing British claims to the southern coast of Australia. But his capture and imprisonment by the French administrator on Mauritius – once the Peace of Amiens came to an end and before news that England and France were again at war could reach the Indian Ocean – was never forgotten. Only with time could historians be persuaded that scientific expeditions can always be construed as affairs of state.[53]

In the United States, the Lewis and Clark expedition suited a nation looking to expand.[54] Across the Atlantic, the end of the Napoleonic Wars brought a fresh impulse to exploration. John Barrow, writing in 1818, observed that, "No sooner did the European world begin to feel the blessings of peace, than the spirit of discovery revived. Expeditions were sent to every quarter of the globe."[55] Skilled and well-traveled military and naval officers were suddenly available for peacetime employment. Thomas Hurd (1753–1823), Hydrographer of the Admiralty, welcomed this as a means of keeping "alive the active services of many meritorious officers whose abilities would not be permitted to lie dormant, whilst they can be turned to national benefit. . . . And [he added] . . . be the means of acquiring a mass of valuable information."[56]

In France, similar conditions applied. The voyages of Dumont d'Urville (1790–1842) demonstrated the value that France, defeated in war, saw in exploration. In 1819, d'Urville sailed to the Mediterranean in the *Chevette*, surveying and compiling a florilegium (now in the Muséum d'Histoire Naturelle in Paris) and discovering the *Venus de Milo* in Melos. His observational skills led to an expedition to Western Australia and to raise the tricolor in Antarctica.

The polar regions presented a number of special challenges to "postwar" science. Perhaps it is no coincidence that Mary Shelley situated the final

[51] For the French in Australasia, see John Dunmore, *French Explorers in the Pacific – The Eighteenth Century* (Oxford: Clarendon Press, 1965); John Dunmore, *Pacific Explorer: The Life of Jean-François de La Perouse, 1741–1788* (Palmerston North: Dunmore Press, 1985); Leslie Marchant, *France Australie: A Study of French Explorations and Attempts to Found a Penal Colony and Strategic Base in South Western Australia, 1503–1826* (Perth: Artlock Books, 1982); Anne-Marie Nisbet, *French Navigators and the Discovery of Australia* (Sydney: University of New South Wales, 1985).

[52] Carter, "Looking for Baudin," p. 24.

[53] See Gascoigne, *Science in the Service of Empire.*

[54] Stephen Ambrose, *Undaunted Courage: Meriwether Lewis, Thomas Jefferson and the Opening of the American West* (New York: Simon and Schuster, 1996); James P. Ronda, *Thomas Jefferson and the Changing West: From Conquest to Conservation* (Albuquerque: University of New Mexico Press, 1997); Dayton Duncan, *Lewis and Clark: An Illustrated History* (New York: Knopf, 1997), arising from the program "Journey of the Corps of Discovery" produced by the Public Broadcasting System and American Library Association.

[55] John Barrow, *A Chronological History of Voyages into the Arctic Regions* (London: John Murray, 1818), pp. 357–8.

[56] George Peard, *Journal of Lt. George Peard of "HMS Blossom"* (Cambridge: Hakluyt Society, 1973), p. 5, cited in Leed, *Shores of Discovery*, p. 221.

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struggle of her Dr. Frankenstein in the region that destroyed the first postwar English scientific expedition.[57] Led in 1818 by Captain John Ross (1777–1856) in the *Isabella*, Lieutenant William Perry in HMS *Alexander*, Captain Buchan in HMS *Dorothea*, and Lieutenant John Franklin (1786–1847) in HMS *Trent*, this expedition was as philosophical in content as it was exploratory in nature, carrying instruments for observations "in all the departments of science, and for conducting experiments and investigations," in order that, in John Barrow's words, "in the event of the main object of the voyage being defeated either through accident or from utter impracticality, every attending might be paid to the advancement of science, and correct information obtained on every interesting subject in high northern latitudes which are rarely visited by scientific men."[58]

With Ross sailed Captain Edward Sabine (1788–1883) and Mr. Fisher, a mathematician from Cambridge.[59] Their work helped transform understanding of a globe in which Britain, as a maritime power, took a keen interest and in which expeditions from Norway and Sweden were soon to be evident.

From the 1820s onward, scientific expeditions were indispensable to colonial settlement. Metropolitan interests played on the commercial value of exploration, eagerly endorsing voyages to map and collect items of economic potential. In Britain, Sir Roderick Murchison (1792–1871), director of the Royal School of Mines, and Sir George Airy (1801–1892), Astronomer Royal at Greenwich, became instruments of the global reach of English science in Australasia and Canada, Africa, the Caribbean, and India. The observatories at Capetown and Melbourne formed part of Britain's imperial infrastructure. Surveying – with its corollary, denial of French occupation – became a recurrent subtext in British colonial policy. Suzanne Zeller sees two themes in such policy – one, inspired by Jonathan Swift's Gulliver, in which the explorer returns "home" to England to lecture to the Royal Society; the other, recalling Daniel Defoe's Robinson Crusoe, in which the explorer becomes a settler himself. In her view, both reflected the "common heritage" of natural theology, utilitarianism, and enterprise.[60]

If Zeller is correct, the tradition was not new. What was new, in part, was the far greater degree of attention paid to recording, reporting, and making public the knowledge gained, for the purpose of colonial settlement and, ultimately, representative government. Thus, administrators in Canada

[57] See Trevor H. Levere, *Science and the Canadian Arctic: A Century of Exploration, 1818–1919* (Cambridge: Cambridge University Press, 1993), especially chapter 6, "The Arctic Crusade: National Pride, International Affairs and Science."

[58] Barrow, *Chronological History of Voyages into the Arctic Regions*, p. 367.

[59] See M. J. Ross, *Polar Pioneers: John Ross and James Clark Ross* (Kingston: McGill–Queen's University Press, 1994). For first-hand accounts, see Sir Edward Sabine, "Geographical, Magnetical and Meteorological Observations during Ross's Arctic Voyage of 1818," RS (Royal Society) Archives MS 126 and 239; Sir Edward Sabine, *Remarks on the Account of the Late Voyage of Discovery to Baffin's Bay, Published by Captain J. Ross* (London: Taylor, 1819).

[60] Zeller, "Nature's Gullivers and Crusoes," p. 192 et seq.

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sent expeditions to find exploitable resources that could be taxed, while in Australia, "transplanted Britons' added to science by testing European generalizations against the "land of contrarities."[61] In 1828–30, for example, Charles Sturt (1795–1869) and Hamilton Hume (1797–1872), looking to solve the problem of prevailing droughts and curious about the contradictory course of rivers in southeastern Australia, explored and surveyed the entire Murray and Darling river systems. Followed by Major Thomas Mitchell (1792–1855) in 1831–6, their reports formed the basis of future agricultural settlement in a region thereafter justly known as "Australia Felix."[62]

To these principles of exploratory settlement were added precepts of imperial strategy. As George Basalla has shown, the "auld alliance" between science and statecraft routinely informed the Admiralty's instructions to officers commanding HM ships. In the case of HMS *Beagle* in 1835, these were twofold. First, its task was to explore the commercial navigation of the eastern seaboard of South America. The former Spanish colonies had become free from the trading monopolies of Iberia and afforded new trading opportunities for Englishmen. Second, the *Beagle* was to show the flag on the Falkland Islands, recently claimed by newly independent Argentina. Captained by a keen amateur naturalist, Captain Robert Fitzroy (1805–1865), the ship incidentally played host to the young gentleman-scholar Charles Darwin (1809–1882).

The *Beagle* gave its name to a chapter in science. But its mission was to advance Britain's "informal empire." Its voyage around South America, past the Galápagos, and across the world was determined by geopolitical rather than scientific motives.[63] Similar accounts frame the near-contemporary voyages of HMS *Erebus* and HMS *Rattlesnake* (1846–50), which took the young surgeon-naturalists (later Darwin's friends) Joseph Hooker (1817–1911) to New Zealand,[64] and Thomas Henry Huxley (1825–1895) to the eastern coast of Australia, the southern coast of New Guinea, and the Louisiade Archipelago. Their voyages must surely rank among the best-known examples of cooperation between science, the Admiralty, and the imperial impulse.

If many scientific expeditions had been imperial in motive and state financed in practice, they would have enjoyed far less public impact had they not been accompanied by expanding networks of collectors and patrons and a new thirst for private exploration and discovery.[65] From freelance

[61] F. G. Clarke, *The Land of Contrarieties: British Attitudes to the Australian Colonies, 1828–1855* (Melbourne: Melbourne University Press, 1977).

[62] Ann Mozley Moyal, *Scientists in Nineteenth-Century Australia: A Documentary History* (Sydney: Cassell, 1976). See also Roy MacLeod, ed., *The Commonwealth of Science: ANZAAS and the Scientific Enterprise in Australasia, 1888–1988* (Melbourne: Oxford University Press, 1988).

[63] George Basalla, "The Voyage of the Beagle without Darwin," *Mariner's Mirror*, 49 (1963), 42–8.

[64] See Jim Endersby, "'From Having no Herbarium': Local Knowledge vs. Metropolitan Expertise: Joseph Hooker's Australasian Correspondence with William Colenso and Ronald Gunn," *Pacific Science*, 55 (2001), 343–59.

[65] Cf. Raby, *Bright Paradise*.

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entrepreneurs to colonial administrators, an almost invisible army of "scientific travelers" came into existence – some wealthy, others not – most returning with evidence of diverse nature and peoples from exotic destinations in India, Africa, the Caribbean, and the Pacific. Sir Charles Nicholson (1808–1903), founding chancellor of the University of Sydney, was far from the first scientific traveler to transit Egypt en route to Australia, but he was one of the first to use his trips to bring antiquities to Australia. Others collected on behalf of powerful patrons – English gentry with naturalist inclinations, such as Lord Derby and the Duke of Northumberland – or else for the Royal Botanic Gardens at Kew or the Horticultural Society of London.[66] Among the travelers to the Amazon and the East Indies, Henry Walter Bates (1825–1892) and Alfred Russel Wallace (1832–1913), who virtually created the science of biogeography,[67] were only among the most visible and literate. Many who came after them brought news of new plants, animals, and peoples to whet insatiable metropolitan appetites. Their voyages, especially to the tropics, encouraged even more travel (and settlement).[68] Their writings – from Robert Louis Stevenson to Joseph Conrad – gave literary authority to discovery and life to "new spaces."

In Britain, these Victorian linkages between science, strategy, and adventure were trebly blessed by governments, scientific societies, and the reading public. In 1839, the voyage of HMS *Erebus* and HMS *Terror*, under Captain James Clark Ross (1800–1862, nephew of Captain John Ross of the *Isabella*), was promoted jointly by the Admiralty, the Royal Society, and the British Association for the Advancement of Science. Its task – to track and measure the earth's magnetic field and to reach the south magnetic pole – was of vital importance to navigation and trade.[69] The fact that France and the United States joined in the "magnetic crusade" – and were waiting for Ross in Van Dieman's Land – served both to paint a Western Christian vision of human destiny and fuel pride in its pursuit.[70]

66 Janet Browne, "Biogeography and Empire," in Jardine, Secord, and Spary, *Cultures of Natural History*, pp. 306–7.

67 Tony Rice, "Amazonia and Beyond, 1848–1862: Alfred Russel Wallace and Henry Walter Bates," in Tony Rice, *Voyages of Discovery: Three Centuries of Natural History Exploration* (London: Natural History Museum, 1999), p. 267.

68 See MacLeod and Rehbock, *"Nature in Its Greatest Extent."*

69 Captain Sir James Clark Ross, *A Voyage of Discovery and Research in the Southern and Antarctic Regions during the Years 1839–43* (London: John Murray, 1847), reprinted with foreword by Sir Raymond Priestley (London: David and Charles, 1969). See John Cawood, "The Magnetic Crusade: Science and Politics in Early Victorian Britain," *Isis*, 70 (1979), 493–518; John Cawood, "Terrestrial Magnetism and the Development of International Collaboration in the Early Nineteenth Century," *Annals of Science*, 34 (1977), 551–87.

70 Ross's expedition also benefited biology when it took winter shelter in New Zealand, giving the young Joseph Hooker an unrivaled opportunity to collect plants native to the region. "No future Botanist," he wrote to his father, William Hooker, at Kew, "will probably ever visit the countries whither I am going, and that is a great attraction," J. D. Hooker to W. J. Hooker, February 3, 1840 in *Letters to J. D. Hooker* (London: Royal Botanic Gardens, Kew), vol.II; Leonard Huxley, *Life and Letters of Joseph Dalton Hooker* (London: John Murray, 1918), vol. 1, p. 163, cited in Endersby, "'From having no Herbarium'."

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Such sentiments are not hard to find in, for example, the United States Exploring Expedition of 1838–42 led by Charles Wilkes (1798–1877), which included the young James Dwight Dana (1818–1895), soon to become America's foremost geologist. The Wilkes expedition, like that of Ross, formed part of an effort to chart the earth's magnetic field and so complete the Newtonian picture of the world.[71] On its return, its rich collections contributed to the establishment of the Smithsonian Institution as the National Museum of the United States. In the 1860s, when American initiatives were interrupted by the Civil War, Germany and the Austro-Hungarian Empire took the lead. Georg Balthasar von Neumayer enlisted the help of Alexander von Humboldt in outfitting a "magnetic" survey of the Pacific and to establish a magnetic observatory in Melbourne.

Similar motives connected science and strategy in the land-based French expeditions of the nineteenth century – to Morea (presently Peloponesia) in 1829–31 and to Algeria in 1839–42. In Mexico (1864–7), a scientific commission accompanied the unhappy Emperor Maximilian. At home and abroad, the support of scientific expeditions was a familiar feature of French colonial policy.[72] A similar theme played in Russia, with expeditions sent in the 1840s to Siberia by the Czar and the Imperial Geographical Society of St. Petersburg. Beginning in the 1870s, imperial Germany sent shipborne medical and ethnological laboratories to places of strategic interest in Asia and the Pacific.[73] By the 1890s, the "Great Game" – forever commemorated in Rudyard Kipling's Kim, trained as a chain-man, pacing the streets of the remote, walled city of Bikaneer to calculate distances for British intelligence – produced vast amounts of information about the Himalayas, Tibet, Nepal, and the northern plains of the Indian subcontinent. Russian expeditions led by Nikolai Przhevalsky (1839–1888), paralleled by British teams proceeding from India and China, produced extensive geographical and geological knowledge of the Lop Nor and Tarim basin and mapped mountain chains from northern Kashmir to western China.[74]

By the 1840s, the United States was keen to join Europe in the great missionary effort of scientific exploration.[75] From its creation in 1838, the U.S.

[71] See Henry Viola and Carolyn Margolis, eds., *Magnificent Voyages: The US Exploring Expedition, 1838–1842* (Washington, D.C.: Smithsonian Institution Press, 1985).

[72] Lewis Pyenson, *Civilizing Mission: Exact Sciences and French Overseas Expansion, 1830–1940* (Baltimore: Johns Hopkins University Press, 1993); Patrick Petitjean, "Essay Review on Science and Colonization in the French Empire," *Annals of Science*, 53 (1995), 187–92; Paolo Palladino and Michael Worboys, "Science and Imperialism," *Isis*, 84 (1993), 91–102; Lewis Pyenson, "Cultural Imperialism and Exact Sciences Revisited," *Isis*, 94 (1993), 103–8.

[73] Wolfgang Eckart, "Wissenschaft und Reisen," *Berichte zur Wissenschaftsgeschichte*, 22 (1999), 1–6.

[74] See Satpal Sangwan, "Reordering the Earth: The Emergence of Geology as Scientific Discipline in Colonial India," *Earth Sciences History*, 12 no. 2 (1993), 224–33; Robert A. Stafford, "Annexing the Landscapes of the Past: British Imperial Geology in the Nineteenth Century," in *Imperialism and the Natural World*, ed. John M. MacKenzie (Manchester: Manchester University Press, 1990), pp. 67–89.

[75] See Edward C. Carter, *Surveying the Record: North American Scientific Expeditions to 1930*, (Philadelphia: American Philosophical Society, 1999).

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Army's Corps of Topographical Engineers surveyed the American Far West and its frontiers with Mexico and Canada. Traveling through unmapped spaces, these "soldier scientists" opened the continent to science and commerce.[76] "American abundance was never better expressed," as William Goetzmann has observed, "than in the tidal wave of specimens and rocks and plants and animals that [flowed] out of the western wilderness."[77]

Overseas, an American naval expedition led by Lieutenant William Lynch (1801–1865) explored the geology of Jordan and the Dead Sea, and in the 1850s, two American naval expeditions joined in the search for Sir John Franklin who had disappeared in the Arctic while searching for the Northwest Passage in 1845. In 1855, following Commodore Matthew Perry's voyage to the Pacific and the "opening" of Japan, U.S. Navy Lieutenant Matthew Maury (1806–1873), later superintendent of the U.S. Hydrographic Office, was the first to discover evidence of underwater mountains in the Atlantic. So began the new discipline of bathymetry. It was not coincidental that, in 1858, the U.S. Navy was called on to help lay the new transatlantic cable. The tendrils of communication sustained the tentacles of empire.[78] Between 1880 and 1920, successive American expeditions to Cuba, the Philippines, Alaska, China, Korea, and Japan extended the interests of national science to what some saw as imperial ambition.[79]

## SCIENCE AND INTERNATIONALISM

If the convergence of science, strategy, and commerce appears to define the "expeditionary" century, so, too, did three variations on the theme of expeditions that were to have a lasting influence on the culture of exploration and the practice of science. First came a new form of international expedition that began in the 1870s; second were the polar voyages that came to a focus in the 1890s; and third were "university," civic, and private expeditions, which began in the 1880s and flourished through the 1920s and 1930s. All three shared a commitment to internationalism, and all three involved the mobilization of people, resources, equipment, publicity, and authority.[80] In many

[76] See William Stanton, *American Scientific Exploration, 1803–1860: Manuscripts in Four Philadelphia Libraries* (Philadelphia: American Philosophical Library, 1991).

[77] William H. Goetzmann, *Army Exploration in the American West, 1803–1863* (New Haven, Conn.: Yale University Press, 1959), p. 19; William H. Goetzmann, *Exploration and Empire: The Explorer and the Scientist in the Winning of the American West* (New York: Knopf, 1967).

[78] For the conjuncture between scientific research, technological innovation, and naval communications in this period, see Daniel Headrick, *Tools of Empire: Technology and European Imperialism in the Nineteenth Century* (New York: Oxford University Press, 1981); Daniel Headrick, *The Tentacles of Progress: Technology Transfer in the Age of Imperialism, 1850–1940* (New York: Oxford University Press, 1988).

[79] See Gary Kroll, "The Pacific Science Board in Micronesia: Science, Government and Conservation on the Postwar Pacific Frontier," *Minerva*, 40, no. 4 (2002), 1–22.

[80] See Felix Driver, *Geography Militant: Cultures of Exploration and Empire* (London: Blackwell, 2001), p. 8.

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ways, these features were not new. What *was* new was the nature of their contribution to science, their international scope, and their impact upon the "culture of exploration."

The prize for the first global expedition of the century could be claimed by the youngest democracy for the Wilkes expedition of 1838. As with England's contemporary experience of HMS *Beagle*, HMS *Rattlesnake*, and HMS *Erebus*, the American expedition was clearly identified with national interest. However, by the 1870s, a new agenda had emerged that was dedicated not merely to collecting what could be found but to the examination of particular features of global change. None of these expeditions was more general, or more significant, than the circumnavigation of HMS *Challenger* (1872–6), often said to be the first modern scientific expedition and certainly the first of many to be so called. Launched by a newly elected British government under the command of Captain Sir George Nares (1831–1865) – typically, both a naval officer and a Fellow of the Royal Society – the *Challenger* set new standards of cooperation, giving adequate space to scientists and crew and disposing of the primacy of place. Its objective was not to plant the flag but to wave it – not to claim new continents but to draw new meanings from nature.

The *Challenger*'s influence ran deep and wide. With data on currents, temperature, salinity, marine life, and the topography of the ocean floor, it brought back descriptions of underwater mountains and disproved theories that life could not exist at great depth. Dredging yielded rocks of continental origin, demonstrating the existence of an Antarctic landmass. The same deep-sea records proved useful to the laying of transatlantic cables – inevitably useful to British commerce and naval intelligence. But above all, the voyage virtually created new fields – the so-called *Challenger* disciplines – in marine geophysics, marine biology, oceanography, and geophysics.[81]

These new disciplines took decades to mature. Far more quickly came other developments. For perhaps the first time, the physical sciences, which had long held the upper hand in framing theories of the earth and its composition, were "challenged" by the biological sciences, with their emphasis on global biodiversity. Moreover, the *Challenger* marked a turning point in according the global expedition a standing place as an academic "institution"

[81] The *Challenger* has a voluminous literature. For a valuable introduction, see Margaret Deacon, *Scientists and the Sea, 1650–1900: A Study of Marine Science* (London: Ashgate, 1971; 2nd ed., 1991). Voyager narratives repay rereading (as they amply repaid their publishers). See, for example, Lord George Campbell, *Log Letters from "The Challenger"* (London: Macmillan, 1876); H. N. Moseley, *Notes by a Naturalist on the "Challenger"* (London: Macmillan, 1879). See also P. F. Rehbock, ed., *At Sea with the Scientifics: The Challenger Letters of Joseph Matkin* (Honolulu: University of Hawaii Press, 1992). For the "*Challenger* disciplines," see Helen Rozwakowski, "Small World: Forging a Scientific Maritime Culture for Oceanography," *Isis*, 87 (1996), 409–19; Tony Rice, "Fathoming the Deep, 1872–1876: The *Challenger* Expedition," in Rice, *Voyages of Discovery*, pp. 290–6. For its lasting impact on science, see Bernard L. Gordon, "Textbooks in the Wake of the *Challenger*," *Proceedings of the Royal Society of Edinburgh Section B*, 72 (1972), 297–303.

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alongside the land-locked observatory, academy, and museum. In some cases, the expedition thereafter became the natural "field extension" of such homotopias.[82] Thereafter, they were increasingly "managed" and in the hands of modernizing universities found a new rationale. Such was the case with the study of ancient civilizations, from the Near East to the Far North, from which university and national museums became important beneficiaries.[83]

These new interests were, in large part, prompted by the study of Darwinian theory in relation to human evolution and development, which, when questioned by the discoveries of remote regions, challenged comfortable Enlightenment dualities between the civilized and the savage. In 1888, for example, the University of Pennsylvania began a custom that many American universities followed in sponsoring expeditions to South America.[84] In 1898, W. H. R. Rivers (1864–1922) led an expedition to the Torres Strait,[85] bringing Cambridge many items now in the university's Archaeological and Anthropological Museum. Other expeditions were sponsored by museums throughout Europe. In the tropical Pacific, the German South Sea expedition of 1908–10, under Georg Thilenius (1868–1937), was sponsored by the Ethnological Museum of Hamburg. Eight scientists studied thirty-four islands, mostly in Micronesia, and published eleven volumes between 1914 and 1938.[86]

The last two decades of the nineteenth century and the first of the twentieth saw a revival of interest in scientific internationalism. On the one hand, national prestige was measured by scientific status; on the other hand, the achievements of science gave an acceptable face to adventurism. The reinvention of the Olympic Games in 1896 inspired Alfred Nobel, and although "Scientific Exploration" was not a Nobel category, the "scoring of goals" held a prominent place in the race among nations.

On the other hand, some goals required international cooperation. As Sir Michael Foster (1836–1917), foreign secretary to the Royal Society, advised the Foreign Office in 1896, "The development of science has made it clear that certain scientific undertakings either cannot be carried out at all except by international co-operation, or can only by this means be carried out successfully, expeditiously, and economically."[87] As far as getting support was concerned, the situation was clear. Sir Clements Markham (1838–1916),

[82] See Roy C. Bridges, "The Historical Role of British Explorers in East Africa," *Terra Incognitae*, 14 (1982), 1–21.

[83] See Roy MacLeod, "Embryology and Empire: The Balfour Students and the Quest for Intermediate Forms in the Laboratory of the Pacific, 1885–1895," in *Darwin's Laboratory: Evolutionary Theory and Natural History in the Pacific*, ed. Roy MacLeod and P. F. Rehbock (Honolulu: University of Hawaii Press, 1994), pp. 140–65.

[84] See the University of Pennsylvania Web site, www.upenn.edu.

[85] Anita Herle and Sandra Rouse, eds., *Cambridge and the Torres Strait: Centenary Essays on the 1898 Anthropological Expedition* (Cambridge: Cambridge University Press, 1998).

[86] See, for example, A. Krämer, *Die Samoan Inseln* (Stuttgart: E. Schweizerbart, 1902, 1903), translated by T. Verhaaren as *The Samoan Islands* (Auckland: Polynesian Press, 1995).

[87] Royal Society Archives, Council Minutes, Sir Michael Foster to Undersecretary of State for Foreign Affairs on proposals to establish an International Geodetic Bureau, November 5, 1896.

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president of the Royal Geographical Society and a formidable expeditioner,[88] reminded the Royal Society that it had simply to persuade government of the benefits: "When this has been done it will follow that the needful outlay will be justified alike from a scientific, a naval, and an imperial point of view."[89]

Expeditions gave countries the chance to prove their mettle. Following the *Challenger*, for example, many problems in Pacific marine biology were solved by Austrian scientists under Max Weber (1852–1937), who sailed aboard the *Siboga* to the Netherlands East Indies in 1899–1900.[90] Polar exploration was another case. In 1878–9, the problem of the Northwest Passage was solved by Nils Nordenskjold (1832–1901), a Swedish explorer, who sailed east along the northern coast of Asia and through the Bering Strait. The passage from the Atlantic to the Pacific was first traversed in 1903–5 by Norwegian Roald Amundsen (1872–1928) after two years' study of the area around the north magnetic pole. In the fin de siècle "race to the poles," the nations of Europe presaged the "space race" of the twentieth century. One author has read this as a struggle for "Science or Glory."[91] From the Nordenskjold expedition to the Antarctic in 1901–3 and Robert Falcon Scott's (1868–1912) expedition in the *Discovery* in 1901–4 to Ernest Shackleton's (1874–1922) expedition in the *Endurance* in 1914–16, victory went to the swift and to the committed.[92]

In Scandinavia, polar exploration was a civilian effort; for Britain and the United States, it was largely a naval affair. In 1909, the American naval Captain (later Admiral) Robert E. Peary (1856–1920) claimed to have reached the North Pole. The first crossing of the pole by air was made by another American expedition, led by Admiral Richard E. Byrd (1888–1957). On March

[88] Ann Savours, "From Greenland's Icy Mountains to India's Coral Strand," *History Today*, 51 (2001), 44–51; Clive Holland, ed., *Antarctic Obsession: A Personal Narrative of the Origins of the British National Antarctic Expedition, 1901–1904 by Sir Clements Markham* (Alburgh: Erskine, 1986).

[89] Royal Society Archives, Council Minutes, Sir Clements Markham to Secretary of the Royal Society, December 3, 1894.

[90] See Florence F. J. M. Pieters and Jaap de Visser, "The Scientific Career of the Zoologist Max Wilhelm Carl Weber, 1852–1937," *Bijdragen Tot de Dierkunde*, 62, no. 4 (1993), 193–214; Gertraut M. Stoffel, "The Austrian Connection with New Zealand in the Nineteenth Century," in *The German Connection: New Zealand and German-Spreading Europe in the Nineteenth Century*, ed. James N. Bade (Auckland: Oxford University Press, 1993, pp. 21–34).

[91] David Mountfield, *A History of Polar Exploration* (London: Hamlyn, 1974), chapter "For Science or Glory," pp. 139–55. Mountfield recalls that it was once customary to distinguish four phases of polar exploration – first, a long period of self-styled adventure, from the Middle Ages to the late eighteenth century; second, a period associated with individual heroes such as Robert Peary and Sir Francis Leonard McClintock (who was knighted for discovering the fate of the Franklin expedition); third, a period that saw the application of new survival techniques, some pioneered by Peary (for which the Eskimos received belated credit); and fourth, our modern scientific exploration. Today, it is fashionable to see Amundsen and Shackleton as the "last flowering" of a more individualist age, after which science becomes the ultimate measure of success and the polar expedition becomes more a matter of technology and teamwork than of individual achievement.

[92] In polar exploration, the fame of being first could eclipse expeditions that achieved more for science but were less newsworthy. Consider, for example, the less well-known but similarly ill-fated 1913–18 Canadian Arctic Expedition that followed Peary, which was led by Vilhjalmur Stefansson in the *Karluk*. See William Laird McKinley, *Karluk: The Great Untold Story of Arctic Exploration* (London: Weidenfeld and Nicolson, 1976).

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17, 1959, the American nuclear submarine USS *Skate* became the first boat to visit the North Pole. It remains an irony that the scientific understanding of the Northwest Passage has proved of value not to commerce, or even to science, but to secret military traffic. The race was equally intense at the South Pole. Again, the Scandinavians and the British were rivals, but Russians, Austrians, and Germans also saw priority as a matter of national pride – a fact reflected in the naming of several island groups in the southern seas.[93]

On December 4, 1911, Roald Amundsen became the first man to reach the South Pole. Eighteen years later, Admiral Byrd was the first to cross the South Pole by air.[94] When flags flew at the poles, the last great problem of expeditionary science seemed solved. Perhaps this came just in time, as the outbreak of the First World War put the expeditionary spirit on hold, just as it ended immediate prospects of international cooperation. The postwar years saw the return to scientific exploration, particularly in relation to mineral resources. Moreover, for the first time, science-based military technologies became available – as when acoustic instruments for antisubmarine warfare permitted the first time graphs of the ocean floor – leading to knowledge of undersea topography and continental movements. These developments were soon followed by military efforts that gathered speed during and after the Second World War.[95]

Far less controversial were regular expeditions mounted by universities, museums, and private foundations. Beginning in the 1920s, the Rockefeller Foundation opened a new chapter in philanthropy, as in research, when it began archaeological and anthropological expeditions to China.[96] At the same time, learned societies continued to make important contributions, notably in the support of expeditions to the polar regions.

In the second half of the twentieth century – notably from *Sputnik* in 1957 onward – scientific exploration continued to serve military and political interests while many disciplines that were spun off from "exploration science" took on new life.[97] The scientific exploration of outer space has held

93 See Walter Lenz, "Die Treibenden Kräfte in der Ozeanographie seit der Gründung des Deutschen Reiches," *Berichte aus dem Zentrum für Meeres- und Klimaforschung*, no. 43 (2002).

94 Byrd's claim is now disputed – by supporters of Amundsen. See http://www.mnc.net/norway/roald.html.

95 See Naomi Oreskes and Ronald Rainger, "Science and Security before the Atomic Bomb: The Loyalty Case of Harold U. Sverdrup," *Studies in the History and Philosophy of Modern Physics*, 31 (2000), 356–63; Chandra Mukerji, *A Fragile Power: Science and the State* (Princeton, N.J.: Princeton University Press, 1989).

96 Between 1908 and 1915, the Rockefeller Foundation sponsored several educational and medical studies in China. See Mary Brown Bullock, *An American Transplant: The Rockefeller Foundation and Peking Union Medical College* (Berkeley: University of California Press, 1980). For later Rockefeller-sponsored expeditions, such as that which led to the discovery of "Peking Man," see Rockefeller Foundation Archives, RG 1.1, series 601D. For this information, I am indebted to Mr. Thomas Rosenbaum of the Rockefeller Foundation Archives.

97 William E. Burrows, *This New Ocean: The Story of the First Space Age* (New York: Random House, 1998).

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special priority, accelerated by the arms race between the United States and the former Soviet Union. Although it was once fashionable to dismiss the domestic applications arising from space exploration, its everyday benefits to communication and information technologies have been immense.

From the end of the Second World War, with hugely increased government support, marine scientists also began to target ambitious objectives. A century earlier, "marine science" lacked a framework of ideas and had no agreed agenda.[98] Within three decades, marine science made major contributions to the theory of plate tectonics, which in turn revolutionized understanding of the earth's dynamics.[99] At the same time, systematic exploration led to the discovery of valuable minerals and of previously unknown marine life forms, with many implications for theories of the age of the earth and the distribution of species.

## LOOKING AHEAD

Some years ago, it was customary to say that almost all of the earth's surface is now explored and most of it exploited. But we know this can be true only in a limited sense. Only a small fraction of the earth's biodiversity has been specified, let alone explained. There remain vast areas of ignorance about the earth and its habitat. Even calling the planet "Earth" has been described as "erdocentric," given that the oceans cover 71 percent of the globe, and less than 2 percent of the seabed has been explored. It is fitting that, in continuation of the processes begun in the eighteenth century and explored in this chapter, science has turned to the oceans, and especially the deep-ocean floor, to the regions beneath the earth's crust, and to outer space.[100] In retrospect, it is also remarkable how much the present owes to precedent. It is fitting that the space industry has borrowed the names of the *Discovery* and the *Challenger* for its shuttles[101] – and the Glomar undersea project, designed for drilling deep-floor samples, that of the *Challenger* for its research vessel.[102] It is similarly fitting that the deep-sea drilling ship of the Joint Oceanographic Institution for Deep Earth Sampling, which has already reached 8,300 meters, has been named, in honor of the lead ship on Cook's third voyage, the JOIDES *Resolution*.[103]

98 Deacon, *Scientists and the Sea*, p. xi.
99 Baker et al., *American Beginnings*, p. 634.
100 For specialist coverage of deep-sea expeditions and research, see the newsletter published by the Commission of Oceanography of the International Union of the History and Philosophy of Science – *History of Oceanography*.
101 See Robert A. Brown, *"Endeavour" Views the Earth* (Cambridge: Cambridge University Press, 1996).
102 See Kenneth J. Hsü, *"Challenger" at Sea: A Ship that Revolutionized Earth Science* (Princeton, N.J.: Princeton University Press, 1992).
103 For JOIDES, see http://joides.rsmas.miami.edu/.

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It is said that we live in a new era of internationalism in which knowledge is seen as an end as well as a means – at least until some end can be found for it. Certainly, despite deep ideological divisions, some of the finest expressions of internationalism – the International Geophysical Year of 1957–8, and the Antarctic Treaty of 1959, since renewed – were begun in the depths of the cold war and have resonances in space exploration today. The south polar region has the distinction of being the only place on earth where the claims of territorial sovereignty have been officially suspended in deference to the interests of nature and the claims of science.[104]

However, commercial and strategic interests continue to drive the search for minerals, groundwater, sources of geothermal energy, and sites suitable for storing radioactive wastes. In the interests of science, classic methods of drilling and sampling are today combined with radar mapping and remote sensing by satellite, and seismic studies remain important, but beneath the earth's surface remains a world of speculation. The high cost of drilling has limited the depths of understanding (so far to 20 km). Rather more progress has been made in ocean studies, on the interaction of sea and air, and on the phenomena that underlie El Niño and La Niña. In 1960, the deepest manned descent was achieved by a submersible that reached the bottom of the Marianas Trench, ten thousand meters below sea level.[105]

Today, the oceans remain the preserve of the wealthiest, most powerful nations on earth or else an opportunity open to all nations acting together. The seas, it is often said, are the ultimate "commons of mankind." Outer space has been similarly described. Medieval language well expresses a modern thought. To find a workable definition of "common heritage" – whether on land, in space, or beneath the seas – remains among the goals of mankind. At the beginning of the twenty-first century, the spirit of the scientific Enlightenment survives, as does the spirit of adventure. As this chapter was being written, over a hundred major scientific expeditions were under way.[106] Yet, their success has exposed deep fissures in public interest. Environmental pessimism is gaining ground, public resources are given into private hands, and governments and international organizations seem powerless to slow the effects of climate change. It is not clear that science has yet empowered mankind with twenty-first–century solutions to problems that have emerged during the last three centuries.

[104] Aant Elzinga, "The Antarctic as Big Science," in *Policy Development and Big Science*, ed. E. K. Hicks and W. Van Russum (Amsterdam: North-Holland, 1991), pp. 15–25; Aant Elzinga, "Antarctica: The Construction of a Continent by and for Science," in *Denationalising Science: The Contexts of International Scientific Practice*, ed. Elisabeth Crawford, Terry Shin, and Sverker Sörlin (Dordrecht: Kluwer, 1993), pp. 73–106; Allison L. C. de Cerreno and Alex Keynan, "Scientific Cooperation, State Conflict: The Roles of Scientists in Mitigating International Discord," *Annals of the New York Academy of Sciences*, 866 (1998), 48–54.

[105] See http//www.ocean.udel.edu/deepsea/level-2/geology/deepsea.html.

[106] "Geography around the World," *Geographical Magazine*, 71 (July 1999), 70–1.

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As the twentieth century drew to a close, two *Voyager* interstellar spacecraft began reporting to Earth (as they will until at least 2020) the conditions found in space around Jupiter, Saturn, Uranus, and Neptune. Their specific task is to define the outer limits of the sun's magnetic field and the outward flow of the solar wind.[107] Their success – offsetting the failure of *Beagle-2* – may well define the future of scientific exploration. Perhaps their larger mission is, in Francis Bacon's words, to secure "the advancement of science and its benefit for the uses of life." It remains to be seen whether it is by such benefits that the history of scientific exploration will best be remembered.

107 See NASA, "Voyager's Interstellar Mission," at http://vraptr.jpl.nasa.gov/voyager/vimdesc.html.

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# 4

# MUSEUMS

*Mary P. Winsor*

Whereas the general public experiences a natural history museum as a series of educational displays, particularly of fossils and stuffed animals, the scientific importance of these institutions lies in the much larger collections of specimens behind the scenes that make possible an inventory and analysis of the world's diversity. The history of natural history museums is more often studied as part of the history of culture rather than as belonging to the history of science, but the role of well-documented collections as an instrument that makes systematic comparison possible deserves investigation. It has been argued that museums were the focus for a new type of science that came to the fore around 1800 based on the analysis of large bodies of information by professional scientists. Although steps in this direction had been taken earlier, the Muséum d'Histoire Naturelle, founded by the revolutionary government in Paris in 1793, became the model for this new science.[1] The subsequent transformation and proliferation of natural history museums was responsible for a substantial increase in the kinds of science that depended on collections.

Plentiful raw material awaits historians in museums' records, in the scientific literature, and even in the physical evidence of collections and buildings. A comprehensive survey ought to pay attention to the related subjects of herbaria, botanical and zoological gardens, medical museums, ethnographic collections, and the international trade that gave specimens monetary value, as well as comparisons with art museums and other exhibitions, but here the focus will be on the zoological activity of major natural history museums.[2]

[1] John V. Pickstone, "Museological Science? The Place of the Analytical/Comparative in 19th-Century Science," *History of Science*, 32 (1994), 111–38.

[2] Sally G. Kohlstedt, "Essay Review: Museums: Revisiting Sites in the History of the Natural Sciences," *Journal of the History of Biology*, 28 (1995), 151–66; Gavin Bridson, *The History of Natural History: An Annotated Bibliography* (New York: Garland, 1994) pp. 393–407.



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## MUSEUMS TO 1792

Until recently, most descriptions of early collections aimed either to celebrate modern policy by exposing them as unscientific or to glorify them in order to enhance the pedigree of their successors. Although Renaissance *Kunst- und Wunderkammern*, or cabinets of curiosities, were often too eclectic and had too many freaks for our taste, historians are now inclined to assess sympathetically their role in the emergence of science. Some apothecaries, physicians, and professors did limit their collections to specimens from nature. One of the most influential was Ulisse Aldrovandi (1522–1605).[3] Sir Hans Sloane (1660–1753) spelled out in his 1739 will that his collection of books, manuscripts, antiquities, and natural objects could be of public benefit, should the state choose to compensate his widow and set up a trusteeship.[4]

During the second half of the eighteenth century, collections of natural specimens rapidly increased in number and in size. Exploration and imperialism provided the opportunity, but the motive was sometimes scientific curiosity, sometimes competitive vainglory. The growing fashion for natural history generated a new career niche for those who collected, cataloged, and preserved specimens for others. Two men who dominated these developments were Carl Linnaeus (1707–1778) and George-Louis Leclerc, comte de Buffon (1707–1788). Buffon in 1739 accepted the directorship of the Jardin du Roi in Paris, where he greatly increased the king's natural history collections. Among his assistants were Louis-Jean-Marie Daubenton (1716–1800) and Jean-Baptiste de Monet, chevalier de Lamarck (1744–1829). Buffon's very influential *Histoire Naturelle* included Daubenton's catalog of the royal cabinet. In spite of their notorious disagreements over principles of classification, Linnaeus and Buffon, innocent of the actual vastness of life's diversity, shared the goal of making an inventory of every kind of living thing.[5]

In 1753, Parliament reluctantly agreed to purchase Sloane's collections, which opened in 1759 in London as the British Museum. Linnaeus's student

[3] Krzysztof Pomian, *Collectors and Curiosities: Paris and Venice, 1500–1800*, trans. Elizabeth Wiles-Portier (Cambridge: Polity Press, 1990); Oliver Impey and Arthur MacGregor, eds., *The Origins of Museums: The Cabinet of Curiosities in Sixteenth- and Seventeenth-Century Europe* (Oxford: Clarendon Press, 1985); Ken Arnold, "Cabinets for the Curious" (PhD diss., Princeton University, 1991); Paula Findlen, *Possessing Nature: Museums, Collecting, and Scientific Culture in Early Modern Italy* (Berkeley: University of California Press, 1994); Andreas Grote, *Macrocosmos in Microcosmo: Die Welt in der Stube: Zur Geschichte des Sammelns 1450 bis 1800*, Berliner Schriften zur Museumskunde, vol. 10 (Opladen: Leske and Budrich, 1994).

[4] William T. Stearn, *The Natural History Museum at South Kensington* (London: Heinemann, 1981).

[5] Frans A. Stafleu, *Linnaeus and the Linneans: The Spreading of Their Ideas in Systematic Botany, 1735–1789* (Utrecht: A. Oosthoek, 1971); Lisbet Koerner, *Linnaeus: Nature and Nation* (Cambridge, Mass.: Harvard University Press, 1999); Charles Coulston Gillispie, *Science and Polity in France at the End of the Old Regime* (Princeton, N.J.: Princeton University Press, 1980); Jacques Roger, *Buffon: A Life in Natural History*, trans. Sarah Lucille Bonnefoi (Ithaca, N.Y.: Cornell University Press, 1997); Franck Bourdier, "Origines et transformations du cabinet du Jardin Royal des Plantes," *Histoire des Sciences*, 18 (1962), 35–50.

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Daniel Solander (1736–1782) was employed there from 1763. By the last quarter of the eighteenth century, serious naturalists everywhere, including the great experimentalist Lazzaro Spallanzani (1729–1799), were arranging their cabinets taxonomically and describing new species as contributions to the inventory. Linnaeus's widow sold his herbarium and books in 1784 to a young English gentleman, James Edward Smith. (The story that a Swedish warship sailed in futile pursuit as this national treasure slipped over the horizon is mythical.) Charles Willson Peale's Philadelphia Museum, founded in 1786, embodied his Enlightenment ideals about public education. Aiming to uplift the ordinary visitor, Peale made his exhibits attractive, arranging stuffed animals on a naturalistic mound covered with vegetation and painting scenery to stand behind the shelved specimens. In 1789, Charles III's recently founded Museo del Prado in Madrid displayed a mounted fossil skeleton of a giant ground sloth (megatherium).[6]

Up to the middle of the eighteenth century, knowledge of minerals, plants, and animals was assumed to be a pious field of recreational study, useful to medicine, but in the latter part of the century, the belief that knowledge of nature would yield economic benefit became common. A further reason to build collections was added by the end of the century, when naturalists began to believe that nature's own system could replace artificial classification. The 1784 classification of crystals according to their geometry by René-Just Haüy (1743–1822) encouraged biologists to expect that a rational order for living things would someday be found.[7]

## THE PARIS MODEL, 1793–1809

The French Revolution was a dangerous time for natural history, for although many republicans were prepared to support scientific education and research if useful, the king's cabinet and garden seemed suspiciously like a luxury. Yet by luck and political skill, the institution not only survived but flourished. Its first piece of luck was that Buffon died before the Revolution, which gave time for his canny former employees, led by gardener André Thouin

[6] Maria-Franca Spallanzani, "La collezione naturalistica di Lazzaro Spallanzani," *Lazzaro Spallanzani e la Biologica del Settecento: Teorie, Esperimenti, Istitutzioni Scientifiche*, Biblioteca della 'Rivista di Storia delle Scienze Mediche e Naturali,' vol. 22 (Florence: Leo S. Olschki Editore, 1982), pp. 589–602; Andrew Thomas Gage and William Thomas Stearn, *A Bicentenary History of the Linnean Society of London* (London: Academic Press, 1988); Charles Coleman Sellers, *Mr. Peale's Museum: Charles Willson Peale and the First Popular Museum of Natural History and Art* (New York: Norton, 1980); Sidney Hart and David C. Ward, "The Waning of an Enlightenment Ideal: Charles Willson Peale's Philadelphia Museum, 1790–1820," in *New Perspectives on Charles Willson Peale: A 250th Anniversary Celebration*, ed. Lilian B. Miller and David C. Ward (Pittsburgh, Pa.: University of Pittsburgh Press, 1991); Sidney Hart and David C. Ward, *Mermaids, Mummies, and Mastadons: The Emergence of the American Museum* (Washington, D.C.: American Association of Museums, 1992).

[7] Peter Stevens, "Haüy and A.-P. de Candolle: Crystallography, Botanical Systematics and Comparative Morphology, 1780–1840," *Journal of the History of Biology*, 17 (1984), 49–92.

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(1747–1824) and Daubenton, to work out a proposal for a self-governing establishment that could promise service to the nation. In the legislative decree of 1793, the name given to the whole enterprise (garden and herbarium as well as cabinet) was Muséum d'Histoire Naturelle. (The word "national" was added to the name during the first few decades of the nineteenth century, then omitted, and revived again early in the twentieth.) Courses of lectures, previously sporadic, were mandated, and the twelve curators were titled professors. Access to the collection was reserved for students on certain days.[8]

Another early stroke of luck was the 1795 arrival of the talented and ambitious Georges Cuvier (1769–1832), whose publications and teaching contributed greatly to the museum's soaring reputation. Lamarck and Etienne Geoffroy Saint-Hilaire (1772–1844) contested Cuvier's belief in the fixity of species, but all three men, and their students, contributed to demonstrating the effectiveness of comparative morphology.[9] The Paris museum embodied the concept that scientific research was a public good that should be paid for by the state but run by scientists. It published technical journals, and its staff wrote authoritative monographs. The collections were in the care of researchers, who kept their arrangement taxonomic, except for Cuvier's rooms, which followed the anatomical tradition of arrangement by organ system. Although the museum was open free to the general public for several days a week, the specimens were neither labeled nor explained.[10]

In medicine, too, museums were being used to display and classify anatomical and pathological specimens. Sometimes these collections expanded to include animal material to aid the study of comparative anatomy. In London, the anatomical collection of John Hunter (1728–1793) was not public but was used in his teaching. The Royal College of Surgeons took charge of it in 1806, although there was much dissatisfaction over the state of the

[8] Joseph-Philippe-François Deleuze, *Historie et description du Muséum Royale d'Histoire Naturelle*, 2 vols. (Paris: Royer, 1823); Ernest-Théodore Hamy, "Les derniers jours du Jardin du Roi et la fondation du Muséum d'Histoire Naturelle," in *Centenaire de la fondation du Muséum d'Histoire Naturelle* (Paris: Imprimerie Nationale, 1893), pp. 1–162; Paul Lemoine, "Le Muséum National d'Histoire Naturelle," *Archives de Muséum National d'Histoire Naturelle*, 12, ser. 6 (1935), 3–79; Camille Limoges, "The Development of the Muséum d'Histoire Naturelle of Paris, c. 1800–1914," in *The Organization of Science and Technology in France, 1808–1914*, ed. Robert Fox and George Weisz (Cambridge: Cambridge University Press, 1980), pp. 211–40.

[9] Toby A. Appel, *The Cuvier-Geoffroy Debate: French Biology in the Decades before Darwin* (New York: Oxford University Press, 1987); Pietro Corsi, *The Age of Lamarck: Evolutionary Theories in France, 1790–1830* (Berkeley: University of California Press, 1988); Dorinda Outram, *Georges Cuvier: Vocation, Science and Authority in Post-Revolutionary France* (Manchester: Manchester University Press, 1984); Peter F. Stevens, *The Development of Biological Systematics: Antoine-Laurent de Jussieu, Nature, and the Natural System* (New York: Columbia University Press, 1994).

[10] J. B. Pujoulx, *Promenades au Jardin des Plantes, à la Ménagerie et dans les Galeries du Muséum d'Histoire naturelle*, 2 vols. (Paris: La Libraire Économique, 1803); Georges Cuvier, "Notice sur l'établissement de la collection d'anatomie comparée du Muséum," *Annales du Muséum d'Histoire Naturelle*, 2 (1803), 409–14; Dorinda Outram, "New Spaces in Natural History," in *Cultures of Natural History*, ed. N. Jardine, J. A. Secord, and E. C. Spary (Cambridge: Cambridge University Press, 1996), pp. 249–65.

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collections.[11] In Philadelphia, Peale, having accomplished the exhumation of a mastodon skeleton, mounted and displayed it in his museum in 1801 to great public excitement.

## IMPACT OF THE PARIS MODEL, 1810–1859

Because people in charge of collections kept close watch on each other's progress, improvements in one location were often quickly copied elsewhere. This international network of awareness, which makes the history of museums remarkably coherent, deserves more study. The Paris museum, with its numerous and well-arranged specimens, immediately became a model. Visiting naturalists and statesmen returned home determined to emulate it; existing museums were reformed, and new ones reflected its example.[12]

The Paris achievement was imitated most effectively where an avid naturalist teamed up with a generous monarch. In Vienna, imperial collections dating back to 1748 were reconstituted in 1810 as the Vereinigten k[aiserlich und] k[öniglich] Naturalien-Cabinete. In Berlin, the new university was equipped with several distinct collections, established by the king in 1810 as the Museum für Naturkunde, to serve professors and students of mineralogy, paleontology, and zoology; many other German universities and cities followed suit. The king of Sweden was convinced to found a state museum by Baron Gustaf Paykull, who had visited foreign museums and whose collections, combined with those of the Academy of Science, comprised the new Naturhistorika Riksmuseum in Stockholm in 1819. Beginning in 1820, the Dutch king, convinced of the practical value of scientific knowledge, established and funded the new Rijksmuseum van Natuurlijke Historie. Although it was situated close to the University of Leiden, its first two directors, Coenraad Jacob Temminck (1778–1858) and Hermann Schlegel (1804–1884), maintained that research, not teaching, was its chief purpose. Well-supported expeditions to the Dutch East Indies helped it to grow into one of Europe's most impressive museums.[13]

[11] Phillip Reid Sloan, "Introductory Essay: On the Edge of Evolution," in Richard Owen, *The Hunterian Lectures in Comparative Anatomy: May and June 1837* (Chicago: University of Chicago Press, 1992), pp. 10–11.

[12] Claude Bankaert, Claudine Cohen, Pietro Corsi, and Jean-Louis Fisher, eds., *Le Museum au premier siecle de son histoire* (Paris: Muséum National d'Histoire Naturelle, 1997); Paul Farber, *The Emergence of Ornithology as a Scientific Discipline: 1760–1850* (Dordrecht: Reidel, 1982); C. E. O'Riordan, *The Natural History Museum, Dublin* (Dublin: The Stationery Office [1983]).

[13] Günther Hamann, *Das Naturhistorische Museum in Wien: Die Geschichte der Wiener naturhistorischen Sammlungen bis zum Ende der Monarchie unter Verwendung älterer Arbeiten von Leopold Joseph Fitzinger und Hubert Scholler mit einem Kapitel über die Zeit nach 1919 von Max Fischer – Irmgard Moschner – Rudolf Schönmann* (Vienna: Naturhistorisches Museum [Veröffentlichungen aus dem Naturhistorischen Museum, Neue Folge 13], 1976); Einar Lönnberg, "The Natural History Museum (Naturhistoriska Riksmuseum) Stockholm," *Natural History Magazine*, 4 (1933), 77–93; Agatha Gijzen, *'S Rijks Museum van Natuurlijke Historie, 1820–1915* (Rotterdam: W. L. & J. Brusse's Uitgeverscmaatschappij, 1938); Pieter Smit, "International Influences on the Development of Natural

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The Museum of the Royal College of Surgeons was opened for study (to approved medical people only) in 1813, but its poor arrangement in comparison with Cuvier's was an embarrassment. Richard Owen was appointed in 1827 to take over systematic cataloging of the collection, neglected by Everard Home, and Owen's work as a comparative anatomist would remain museum based throughout his career. The Linnean Society of London purchased the Linnaean collection after Smith's death. (It is not true that Smith had created the society to receive the herbarium, nor that he bequeathed it.) Natural history at the British Museum was neglected after the death of Solander, but improvement followed the 1813 appointment of William Elford Leach (1790–1836), an admirer of the Paris museum. Joseph Banks's plants from James Cook's circumnavigation went to the British Museum in 1827 in the custody of Robert Brown. In 1836, a Parliamentary Select Committee heard evidence of the inferiority of the British national museum to continental ones. Major reform followed the 1840 promotion of John Edward Gray (1800–1875) to keeper of the zoological department. Gray steered his department into a position of scientific authority. In 1856, Richard Owen left the Royal College of Surgeons to become Superintendent of the Department of Natural History of the British Museum; in the same year, the Zoological Society of London decided to transfer its collection to the British Museum.[14]

That transfer of specimens (which included Darwin's Galápagos birds) illustrates an important principle in the history of museums: the magnetic attraction that pulls small collections toward large. An individual who lovingly forms a collection, or his heirs, must one day face the problem of its survival, and institutions are the natural solution. In exchange for donated material, a state museum gives hope of immortality by registering the donor's name in its records and by making the specimens available to future users. The greater a museum's apparent permanence, the fussier it can be in choosing which donations to accept.

In the young American republic, Peale's sons attempted to carry on his museum business in the 1820s and 1830s, in Baltimore and New York as well as Philadelphia. Peale and his sons have been credited with having invented "the modern American museum: a truly democratic institution, a place for everyone," but they failed to invent a new way to finance it.[15] Denied government

History in the Netherlands and Its East Indian Colonies between 1750 and 1850," *Janus*, 65 (1978), 45–65.

[14] Nicolaas A. Rupke, *Richard Owen: Victorian Naturalist* (New Haven, Conn.: Yale University Press, 1994); Albert E. Günther, *A Century of Zoology at the British Museum through the Lives of Two Keepers: 1815–1914* (London: Dawsons, 1975); D. J. Mabberley, *Jupiter Botanicus: Robert Brown of the British Museum* (Braunschweig: J. Cramer, 1985); Frank Sulloway, "Darwin's Conversion: The Beagle Voyage and Its Aftermath," *Journal of the History of Biology*, 15 (1982), 325–96, at p. 356; Gordon McOuat, "Cataloguing Power: Delineating 'Competent Naturalists' and the Meaning of Species in the British Museum," *British Journal for the History of Science*, 34 (2001), 1–28.

[15] Joel J. Orosz, *Curators and Culture: The Museum Movement in America, 1740–1870* (Tuscaloosa: University of Alabama Press, 1990), p. 87.

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support, they were defeated by competition from sensational shows (sometimes calling themselves “museums”) and from purely scientific collections (such as the Academy of Natural Sciences in Philadelphia). Governments in the United States were reluctant to devote public funds to science, but in 1846 Congress accepted a private cash bequest and created the Smithsonian Institution in Washington. Its first director, physicist Joseph Henry, hired Spencer Fullerton Baird (1823–1887) “to take charge of the cabinet and to act as naturalist of the Institution” in 1850. It is a fable that Baird built up the museum without Henry’s knowledge, but certainly the original purpose of the collection was research, not exhibition. The prospects of what people were starting to call the United States National Museum brightened in 1858 when Congress began appropriating funds for it.[16]

Louis Agassiz, a Swiss emigré familiar with a dozen European museums, encouraged Baird to follow their model and focus on scientific research. Agassiz founded the Museum of Comparative Zoology in 1859, with funding from Harvard University, from private donors, and from the government of Massachusetts. Agassiz stressed that the richly ordered nature studied in his museum must be the product of divine thought, not a blind evolutionary process. His great impact on American culture was inseparable from his passion for the growth of his museum.[17] Many other colleges, convinced by their faculty that the scientific study of natural history required a collection, supported their own museums.[18] John Phillips was in 1857 appointed first Keeper of Oxford’s University Museum, which opened in 1860, just in time to be the site of Thomas Henry Huxley’s debate with Bishop Wilberforce.[19]

Smaller museums across Europe and around the world seem mostly to have been planted and grown by passionate individuals thanks to amateur helpers with local funds. The encouragement such museum-builders received from the naturalists at the leading museums, although in some cases considerable, resulted from their common interests, not government policy. The great museums stood to the smaller as centers of calculation, in Latour’s terms, and distant naturalists often deferred to the authority of the center in spite of their superior field knowledge.[20]

[16] Charlotte M. Porter, “The Natural History Museum,” in *The Museum: A Reference Guide*, ed. Michael Steven Shapiro (Westport, Conn.: Greenwood Press, 1990), pp. 1–29; E. F. Rivinus and E. M. Youssef, *Spencer Baird of the Smithsonian* (Washington, D.C.: Smithsonian Institution Press, 1992), p. 44.

[17] Elmer Charles Herber, ed., *Correspondence between Spencer Fullerton Baird and Louis Agassiz – Two Pioneer American Naturalists* (Washington, D.C.: Smithsonian Institution Press, 1963); Mary P. Winsor, *Reading the Shape of Nature: Comparative Zoology at the Agassiz Museum* (Chicago: University of Chicago Press, 1991).

[18] Sally G. Kohlstedt, “Curiosities and Cabinets: Natural History Museums and Education on the Antebellum Campus,” *Isis*, 79 (1988), 405–26.

[19] See Jack Morrell, *John Phillips and the Business of Victorian Science* (Aldershot: Ashgate, 2005).

[20] Maurice Chabeuf and Jean Philibert, “Le Musée d’Histoire Naturelle de Dijon de 1836 à 1976,” *Bulletin Scientifique de Bourgogne*, 33 (1980), 1–12; Ione Rudner, “The Earliest Natural History Museums and Collectors in South Africa,” *South African Journal of Science*, 78 (1982), 434–7; Sally Gregory Kohlstedt, “Australian Museums of Natural History: Public Priorities and Scientific Initiatives in

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## THE MUSEUM MOVEMENT, 1860–1901

All across the globe, wherever Europeans carried their culture and settled in sufficient numbers, natural history museums multiplied. In a general sense, this belongs to the story of imperialism and colonization, and the spread of botanists and botanic gardens has been well analyzed in that context.[21] The story of provincial natural history museums seems often to have depended on the determination of a single driven individual. Frederick McCoy (1823–1899) was the director of the National Museum of Victoria in Melbourne from its beginning in 1854, and Julius Haast (1822–1887) was a prime mover in the founding of the Canterbury Museum, which opened in 1870. Hermann Burmeister (1807–1892) in 1862 took over the Museo Publico de Buenos Aires, a museum that traced its origins back to 1812. John William Dawson (1820–1899), a professor at McGill University, had been content with a modest collection until the Geological Survey moved with its collections to Ottawa in 1881; industrialist Peter Redpath built him a museum in 1882. Francisco Moreno (1852–1919) of La Plata had been inspired as a child by Burmeister's museum; the government chose Moreno to head the new Museo General de La Plata in 1884. Usually such museums tried to display the world's diversity, not just local natural history. In Honolulu, the Bernice P. Bishop Museum opened in 1891, based on collections dating back to 1872. Its director, William Tufts Brigham (1841–1926), had studied with Agassiz and clung to his philosophy that a museum must be a research tool.[22]

Beginning in 1863, the Linnean Society sold or gave away most of its collections, except Linnaeus's and a few others, deciding it could best serve its members by publishing, maintaining a library, and hosting meetings.

The term "museum movement" is sometimes used to refer to the growth in the number of public museums – devoted to art, history, and industry as well as natural history – throughout the nineteenth century, but other authors more helpfully limit it to the lively period from about 1880 to 1920. Imprecision also exists around the term "the museum idea," which may refer broadly to the belief that people of all levels of education can benefit from visiting well-arranged museums but may include the idea that exhibits should be designed for visitors, at least by having good labels. Two events that helped launch the museum idea, by showing that liberal policies toward the public would not end in disaster, were the 1851 Great Exhibition in London and

the 19th Century," *Historical Records of Australian Science*, 5 (1983), 1–29; Bruno Latour, *Science in Action* (Cambridge, Mass.: Harvard University Press, 1987).

[21] Lucile H. Brockway, *Science and Colonial Expansion: The Role of the British Royal Botanic Garden* (New York: Academic Press, 1979); Richard Harry Drayton, "Imperial Science and a Scientific Empire: Kew Gardens and the Uses of Nature, 1772–1903" (PhD diss., Yale University, 1993).

[22] Susan Sheets-Peyenson, *Cathedrals of Science: The Development of Colonial Natural History Museums during the Late Nineteenth Century* (Montreal: McGill–Queen's University Press, 1988); W. A. Waiser, "Canada on Display: Towards a National Museum, 1881–1911," in *Critical Issues in the History of Canadian Science, Technology and Medicine*, ed. Richard A. Jarrell and Arnold E. Roos (Thornhill: HSTC Publications, 1983); Roger G. Rose, *A Museum to Instruct and Delight: William T. Brigham and the Founding of Bernice Pauahi Bishop Museum* (Honolulu: Bishop Museum Press, 1980).

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Sir Henry Cole's 1857 South Kensington Museum (now the Victoria and Albert Museum).[23]

Public interest in natural history museums was excited by the bones of big extinct animals. A megatherium, real or in replica, was de rigeur. Benjamin Waterhouse Hawkins (1807–1899), besides building dinosaur models, mounted the skeleton of a dinosaur (hadrosaurus) for Joseph Leidy in 1868, which drew crowds to the Academy of Natural Sciences in Philadelphia. An inevitable consequence of admitting more visitors was "dual arrangement," the policy of dividing a museum's holdings into certain objects on display and others reserved in storage for expert study. The advantages of this policy – better protection of research material and clearer presentation of information to the casual visitor – were plainly spelled out in 1864 by J. E. Gray, but the idea spread slowly. Schlegel was arguing in 1878 that every bird skin should be stuffed and put on a stand. As late as 1893, dual arrangement was called a "new" idea.[24]

Dual arrangement has important implications for museum architecture because it requires that some rooms be designed for crowds of people and others for storage and study. William Henry Flower (1831–1899), first director of the British Museum (Natural History), noted:

> It is a remarkable coincidence that . . . before they [ideas of dual arrangement] had met with anything like universal acceptance, the four first nations of Europe almost simultaneously erected in their respective capitals – London, Paris, Vienna and Berlin – entirely new buildings, on a costly, even palatial scale, to receive the natural history collections, which in each case had quite outgrown their previous insufficient accommodation.[25]

Contested ideas of proper arrangement had plagued the process of designing the new natural history museum in London. Some plans separated students from the general public, but Gray's advice to plan "generous areas for storage and research" was ignored. Owen proposed an "index museum" – a series of small alcoves off the main hall where representative specimens would give the public a synopsis of the main taxonomic groups of animals – but although the alcoves were built, the index idea was dropped. Agassiz proposed "synoptic"

[23] Rupke, *Richard Owen*; Sally Gregory Kohlstedt, "International Exchange and National Style: A View of Natural History Museums in the United States, 1850–1900," in *Scientific Colonialism: A Cross-Cultural Comparison*, ed. Nathan Reingold and Marc Rothenberg (Washington, D.C.: Smithsonian Institution Press, 1987), pp. 167–90.

[24] James Edward Gray, "On Museums, Their Use and Improvement, and on the Acclimatization of Animals," *Annals and Magazine of Natural History*, 14 (1864), 283–97, and in *Report of the British Association for the Advancement of Science* (1865), 75–86; Erwin Stresemann, *Ornithology from Aristotle to the Present* (Cambridge, Mass.: Harvard University Press, 1975), p. 213; William Henry Flower, "Modern Museums" (Presidential address to the Museums Association, 1893), in William Henry Flower, *Essays on Museums and Other Subjects Connected with Natural History* (London: Macmillan, 1898), pp. 30–53, at p. 37.

[25] Flower, *Essays on Museums and other Subjects Connected with Natural History*, p. 41.

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rooms in his own plans, but, like Owen, he intended to display as many specimens as possible in other rooms. The natural history collections of the British Museum were transferred to the South Kensington building, where the British Museum (Natural History) opened in stages between 1880 and 1883.[26] The Grande Galerie de Zoologie, a new building of the Muséum d'Histoire Naturelle that opened in 1889, was "a glorification of the old idea, pure and simple . . . every specimen is intended to be exhibited."[27] The architects of the enormous new Museum für Naturkunde in Berlin, which opened in 1890, had assumed in their 1884 plans that the bulk of the collection would be open to all visitors, but when Karl August Möbius (1825–1908) became director of the zoological portion in 1888, he put the exhibits on the ground floor and research collections upstairs, rendering the grand staircases useless. In Vienna, the Naturhistorische Hof-Museum, planned since 1871 and under construction from 1881, opened in 1889.[28]

The American Museum of Natural History in New York (founded in 1869 and opened in 1871) is often considered to be a landmark in the increasing service to the general public of natural history museums. It is credited, along with the major art museums founded in Boston and New York at the same time, with achieving a compromise between professional science and popular education. Public education was the purpose of the American Museum of Natural History from the start, but its scientific reputation did not begin until the 1880s. It was founded by wealthy businessmen who were impressed by Agassiz's museum and by the dreams of his renegade student Albert S. Bickmore (1839–1914). They started with thousands of donated and purchased specimens, and Bickmore did his best to put everything on display. A decade after its promising beginning, however, public attendance was ominously slight.[29] Agassiz's museum would doubtless have been in decline, too, after his death in 1873, if not for the loyalty of his son Alexander, a self-made millionaire. Between 1875 and 1884, he constructed efficient storage space and didactic exhibit halls in the Museum of Comparative Zoology. Alfred Russel Wallace praised the result as far superior to the old-fashioned

[26] Mark Girouard, *Alfred Waterhouse and the Natural History Museum* (London: British Museum (Natural History), 1981), p. 12; Sophie Forgan, "The Architecture of Display: Museums, Universities and Objects in Nineteenth-Century Britain," *History of Science*, 32 (1994), 139–62; Nicolaas A. Rupke, "The Road to Albertopolis: Richard Owen (1804–92) and the Founding of the British Museum of Natural History," in *Science, Politics and the Public Good: Essays in Honour of Margaret Gowing*, ed. Nicolaas A. Rupke (London: Macmillan, 1988), pp. 63–89.

[27] Flower, *Essays on Museums and other Subjects Connected with Natural History* , p. 43.

[28] Robert Graefrath, "Zur Entwurfs- und Baugeschichte des Museums für Naturkunde der Universität Berlin," *Beiträge zur Geschichte des Museums für Naturkunde der Humboldt-Universität zu Berlin und seinen aktuellen Forschungs- und Bildungsaufgaben. Wissenschaftlich Zeitschrift der Humboldt-Universität zu Berlin, Reihe Mathematik/Naturwissenschaften*, 38, no. 4 (1989), 279–86; Ilse Jahn, "Der neue Museumsbau und die Entwicklung neuer museuologischer Konzeptionen und Activitäten seit 1890," ibid., 287–307.

[29] John Michael Kennedy, "Philanthropy and Science in New York City: The American Museum of Natural History, 1868–1968" (PhD diss., Yale University, 1968).

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practice still standard in Europe.[30] In 1877, Baird hired George Brown Goode (1851–1896), who would succeed him ten years later and become a leader among museum directors. The United States National Museum embraced dual arrangement when it acquired its own building in 1881. In that same year, a retired financier, Morris Ketchum Jesup (1830–1908), became president of the American Museum of Natural History.

Taxidermy was a craft that served several kinds of clients. For private collectors, sportsmen, and expositions, shells could be polished or glued together to form fanciful designs, and frogs could go skating. William Bullock of London, Hermann Ploucquet of Stuttgart, and Jules Verreaux of Paris mounted theatrical groups: a tiger wrestling with a boa constrictor, hounds pulling down a stag, and an Arab on his camel beset by lions. Fine for a fair, these were not the sober poses suitable for a scientific institution. The American Museum of Natural History did nothing for its scientific reputation when it purchased Verreaux's camel scene in 1869; Agassiz at the same time was telling his supplier that stuffed animals, or pickled worms in a jar, could look boring or ugly for all he cared because their purpose was disciplined study. Leaving bones loose in a drawer made them easier for a researcher to compare, though a casual visitor would prefer to see an articulated skeleton. Dual arrangement altered the dynamics of the prepared-specimen market. Craftsmen responded by offering exquisite replicas of marine invertebrates and plants made of colored wax or glass and by developing artistic taxidermy.[31]

Artistic taxidermy entered the British Museum (Natural History) in 1883 thanks to the enthusiasm of Albert Günther (1830–1914) and R. Bowdler Sharpe (1847–1909). They commissioned a series of nesting birds, which the public loved. So did Jesup, coming to study European museums in 1884. He returned to New York with a better appreciation of the scientific as well as public function of museums. Mammalogist and ornithologist Joel Asaph Allen (1838–1921) left the Museum of Comparative Zoology for the American Museum of Natural History in 1884, bringing with him a clear understanding of dual arrangement, a commitment to scientific research, and an appreciation of artistic taxidermy. With techniques imported from the British Museum (Natural History), birds were displayed naturalistically at the American Museum of Natural History starting in 1886, and in 1887 Allen hired Frank M. Chapman (1864–1945) to further improve the exhibits. In 1888, the New York museum began to be open on Sundays, a change resisted in London until 1896.

30 Alfred Russel Wallace, "American Museums," *Fortnightly Review*, 42 (1887), 347–69; Mary P. Winsor, "Louis Agassiz's Notion of a Museum: The Vision and the Myth," in *Cultures and Institutions of Natural History*, ed. Michael T. Ghiselin and Alan E. Leviton, Memoirs of the California Academy of Sciences No. 25 (San Francisco: California Academy of Sciences, 2000), pp. 249–71.

31 S. Peter Dance, *A History of Shell Collecting* (Leiden: E. J. Brill, 1986); Karen Wonders, *Habitat Dioramas* (Uppsala: Uppsala University Press, 1993); P. A. Morris, "An Historical Review of Bird Taxidermy in Britain," *Archives of Natural History*, 20 (1993), 241–55.

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Artistic taxidermy spread only slowly in European halls of science. Was it considered unscientific? The experiences of the brilliant Swedish naturalist Gustaf Kolthoff suggest so. In 1889, he installed in the zoology department of Uppsala University an ambitious "biological museum," with lively specimens arranged against beautifully painted backgrounds. Although it was admired by visitors, the department found better use for the space after little more than a decade. In 1893, Kolthoff created in Stockholm a panoramic view of vegetation, rocks, stuffed birds, and 360° of painted scenery. Impressive in scale and detail and beloved by all, this Biological Museum came close to being dismantled within fifteen years (though it did survive); meanwhile the Swedish Museum of Natural History stuck to its old style of staid display.

The king of the museum supply business was Henry Augustus Ward (1834–1906). In 1862, he started Ward's Natural Science Establishment in Rochester, New York, hiring taxidermists and preparators from Europe, who taught his American "boys." Several of them, led by William Temple Hornaday (1854–1937), grouped specimens with appropriate ground and foliage beginning in 1879.[32] Their effort to capture the shape of muscle and bone was applauded, but museum professionals resisted the idea of painted backgrounds. At the United States National Museum, Goode hired Hornaday, and many group mountings (without backgrounds) were installed in Washington in the 1880s. In 1889, Carl E. Akeley (1864–1926), working for William Morton Wheeler at the Milwaukee Public Museum (founded in 1882 at Ward's instigation), installed a little diorama of muskrats, with bullrushes, a pond in cross section, and a painted background of more rushes and pond beyond. In 1893, the World's Columbian Exposition in Chicago was full of fancy taxidermy, most notably a landscape crammed with mammals in the Kansas Building. Chicago citizens purchased some of the exhibits, creating in 1893 the Columbian Museum of Chicago. Its name was changed the next year to Field Columbian Museum to honor a donor (later changed to Field Museum of Natural History, later still Chicago Museum of Natural History, and now again Field Museum of Natural History). In 1898, Chapman directed his assistants at the American Museum of Natural History to create new bird groups larger than nesting pairs. Before the century was over, exhibits called "habitat groups" – scores of seabirds nesting on a cliff, several bison posed among sagebrush and sedge – were features of many of America's public museums.[33]

In 1891, the anatomist and paleontologist Henry Fairfield Osborn (1857–1935) was hired jointly by the American Museum of Natural History and

[32] Sally Gregory Kohlstedt, "Henry A. Ward: The Merchant Naturalist and American Museum Development," *Journal of the Society for the Bibliography of Natural History*, 9 (1980), 647–61.

[33] Nancy Oestreich Lurie, *A Special Style: The Milwaukee Public Museum: 1882–1982* (Milwaukee, Wis.: Milwaukee Public Museum, 1983).

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Columbia University.[34] It is reported that "One of Osborn's young artisans, Adam Heismann, was able to devise a technique for boring through the extremely fragile center of fossil bones. He thus made it possible to mount, for the first time, free standing skeletons of fossil animals."[35] Previous fossil skeletons had been supported by external iron armatures (except for the mastodon, preserved in a bog).

It is generally assumed that the museum movement was progressive; that is, that making exhibits more attractive was a good thing. Undoubtedly public education must have benefited, but what has not been investigated is how the scientific use of the collections fared. At first, the process of separating the displays gave research collections room to grow because curators were freed from the need to make study specimens pretty. A drawer could hold many more bird skins than could stand stuffed on a shelf, and a box could hold loose shells that would take up more space if glued on a board. Everyone seemed to imagine that money and time would only have to be expended on exhibits once, after which the perennially unfinished business of cataloging, classifying, and publishing could be resumed. Such hopes were fated for disappointment, not only because success with the public brought pressure for expanded public activities but because donors of public as well as private monies, and even administrators, tended to lose interest in material they did not see.

Princeton University started a museum of natural history in 1873, begun, like those at Harvard and Yale, with a private cash gift. But the expense of maintaining a large collection became harder for colleges to justify toward century's end, when biology textbooks focused on dissections and microscopy. McGill University contributed little to the finances of the Redpath Museum.[36]

Darwin had said that if his ideas were accepted, "systematists will be able to pursue their labours as at present."[37] What he meant was that specialists could continue to describe new species, and judge their relationship to other species, on the basis of morphological characters of preserved specimens. Most taxonomists did exactly that, managing the ever-growing world inventory with techniques already familiar. There were a few modifications of method, however. Rules of nomenclature were negotiated, and a third name (in addition to genus and species) to indicate a local variety was allowed. Also, curators learned to give special care and documentation to a "type" – the individual specimen used by the describer of a species. Type specimens anchored nomenclature, though Darwin's theory showed that no specimen

[34] Ronald Rainger, *An Agenda for Antiquity: Henry Fairfield Osborn and Vertebrate Paleontology at the American Museum of Natural History, 1890–1935* (Tuscaloosa: University of Alabama Press, 1991).

[35] Kennedy, "Philanthropy and Science in New York City," p. 125.

[36] Susan Sheets-Peyenson, "'Stones and Bones and Skeletons': The Origins and Early Development of the Peter Redpath Museum (1882–1912)," *McGill Journal of Education*, 17 (1982), 45–64; Sally G. Kohlstedt, "Museums on Campus: A Tradition of Inquiry and Teaching," in *The American Development of Biology*, ed. Ronald Rainger, Keith Benson, and Jane Maienschein (Philadelphia: University of Pennsylvania Press, 1988), pp. 15–47.

[37] Charles Robert Darwin, *On the Origin of Species* (London: John Murray, 1859), p. 484.

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was typical in the ontological sense. Experimental scientists work with the ideal that their peers in another laboratory can replicate or falsify their results; taxonomists likewise need to make their material available for reexamination by another expert, and public museums make this possible, even though the second look may not come for a generation or more.[38]

Darwin had also predicted that his theory would make natural history far more interesting. In the same spirit, Ernst Mayr wrote, "One might have expected that the acceptance of evolution would result in a great flowering of taxonomy and enhancement of its prestige during the last third of the nineteenth century." Instead, its prestige among the sciences slumped, which Mayr explains "in part for almost purely administrative reasons," namely that museums had to bear the burden of "very necessary but less exciting descriptive taxonomy."[39] Some museum workers, particularly paleontologists, contributed to lively debates on the phylogeny of the higher taxa, such as the origin of vertebrates from invertebrates, but zoologists based in universities were equally prominent in discussing those evolutionary questions. Microscopy and experimental physiology, based in universities and field stations, took over at the cutting edge of biology in the second half of the nineteenth century, and in the competition for money and talent, museums lost out.[40]

A few museum directors were opposed to evolution, including Louis Agassiz, Dawson, Schlegel, and Giovanni Giuseppe Bianconi (1809–1898) in Bologna, but museums also housed some of evolution's most ardent supporters, including Edmond Perrier (1844–1921) and Albert Jean Gaudry (1827–1908) in Paris and Othniel C. Marsh (1831–1899) at Yale's Peabody Museum. Others, such as Alexander Agassiz, acknowledged the truth of evolution but avoided controversy. Möbius created in Berlin exhibits that illustrated his ecological ideas, including an oyster bed, a coral reef, and examples of mimicry and parasitism.

## DIORAMAS AND DIVERSITY, 1902–1990

In 1902, the masterful Akeley installed in Chicago habitat groups showing deer in the four seasons with trees against flat background paintings. In the

[38] Richard V. Melville, *Towards Stability in the Names of Animals: A History of the Internationl Commission on Zoological Nomenclature, 1895–1995* (London: International Trust for Zoological Nomenclature, 1995); Paul Lawrence Farber, "The Type-Concept in Zoology during the First Half of the Nineteenth Century," *Journal of the History of Biology*, 9 (1976), 93–119; Mark V. Barrow, Jr., *A Passion for Birds: American Ornithology after Audubon* (Princeton, N.J.: Princeton University Press, 1998); M. V. Hounsome, "Research: Natural Science Collections," in *Manual of Curatorship: A Guide to Museum Practice*, ed. John M. A. Thompson et al., 2nd ed. (Oxford: Butterworth-Heinemann, 1992), pp. 536–41; Keir B. Sterling, ed. *An International History of Mammalogy* (Bel Air, Md.: One World Press, 1987).

[39] Ernst Mayr, "The Role of Systematics in Biology," *Evolution and the Diversity of Life: Selected Essays* (Cambridge, Mass.: Harvard University Press, 1976), pp. 416–24, at p. 417.

[40] Peter J. Bowler, *Life's Splendid Drama: Evolutionary Biology and the Reconstruction of Life's Ancestry* (Chicago: University of Chicago Press, 1996).

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same year, workers under Chapman completed for the American Museum of Natural History a scene of terns in flight as well as nesting on a beach, the ocean sweeping back into the distance. "Although many museum scientists thought that it was too informal, even verging on the sensational, president Jesup declared the group to be beautiful and as a result of its success, a fund was set up to finance other such exhibits for the bird hall."[41] Also in 1902, Olof Gylling, inspired by Kohltoff, built for the Malmö Museum in Sweden a lovely diorama of the bird breeding ground of Makläppen Island. Gylling later created a stunning set of dioramas that opened in 1923 at the natural history museum in nearby Gothenburg. The era of imposing dinosaur displays was just beginning as well. The Carnegie Museum of Natural History, which opened in Pittsburgh in 1904, featured an enormous *Diplodocus*; the next year, Andrew Carnegie gave a copy to the British Museum of Natural History. The American Museum of Natural History followed with huge mounts of *Allosaurus* (1907) and *Tyrannosaurus* (1910).

After Akeley moved to New York in 1909, Osborn and other wealthy New Yorkers supported his determination to capture the dramatic scenery and threatened fauna of Africa in a series of dioramas, completed in 1936. These may have embodied attitudes of their builders that are, to modern sensibilities, sexist and racist.[42] They certainly expressed their builders' passionate concern about the vanishing wilderness, as did the other beautiful dioramas installed in the Carnegie Museum in Pittsburgh, the Museum of Natural History at Iowa State University, the Denver Museum of Natural History, the James Ford Bell Museum of Natural History in Minneapolis, the California Academy of Sciences, and the Los Angeles County Museum of Natural History. Their dioramas featured curved backgrounds and imitation foliage, artistic and accurate. Yet for all their expense and attractiveness, dioramas had little connection to science, and curators sometimes worried that the primary purpose of museums was being forgotten.

In the twentieth century, few schools and universities felt the same interest in museums that had motivated educators in the nineteenth, but there were exceptions according to local circumstances. The Museum of Vertebrate Zoology, where Joseph Grinnell trained his students, was accepted by the University of California at Berkeley in 1908 only because Annie M. Alexander supplied its funding. Under Alexander Grant Ruthven, the old museum at the University of Michigan in Ann Arbor flourished, as did the Museum of Natural History at the University of Kansas in Lawrence. In Toronto, the Royal Ontario Museum, opened in 1912, was designed to serve both the University of Toronto and the general public.[43]

[41] Wonders, *Habitat Dioramas*, p. 128.

[42] Donna Haraway, *Primate Visions: Gender, Race, and Nature in the World of Modern Science* (New York: Routledge, 1989), pp. 26–58.

[43] Barbara R. Stein, "Annie M. Alexander: Extraordinary Patron," *Journal of the History of Biology*, 30 (1997), 243–66; W. A. Donnelly, W. B. Shaw, and R. W. Gjelsness, eds. *The University of Michigan:*

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Dual arrangement, which kept the taxonomic work of museums invisible, left their research function vulnerable. Amateur volunteers continued to lend valuable help to the maintenance of some collections. After the rise of molecular biology, collection-based biology was nonexistent in most university biology programs, so that a museum that wanted to hire a curator with a PhD in systematics might find no suitable candidate. Ernst Mayr, an ornithologist trained in the Berlin Museum, was hired at the American Museum of Natural History in 1931. His *Systematics and the Origin of Species* (1942) placed museum work near the center of the evolutionary synthesis. As director of the Museum of Comparative Zoology from 1961 to 1970, he fought tirelessly to improve the status – both administrative and intellectual – of museum-based science in an age increasingly dominated by the experimental areas of biology.[44] Two theoretical innovations, numerical taxonomy (phenetics) and phylogenetic systematics (cladistics), helped raise the scientific stature of systematics in the second half of the twentieth century. Most of the key figures in these developments were based in museums (Daniele Rosa, Lars Brundin, C. D. Michener, Gareth Nelson, and Colin Patterson), but others were not (Willi Hennig, Robin John Tillyard, A. J. Cain, and Peter Sneath).[45]

Today, many natural history museums are struggling desperately, in an age of television and theme parks, to attract enough public interest to support their educational functions, and support for collection and preservation of specimens is harder to find. Yet the biodiversity crisis makes the work of systematists, who depend on large research collections, more important than ever. Perhaps even now the foundations of a second museum movement are being laid.

*An Encyclopedic Survey* (Ann Arbor: University of Michigan Press, 1958), vol. 4, pp. 1431–1518; Lovat Dickson, *The Museum Makers: The Story of the Royal Ontario Museum* (Toronto: Royal Ontario Museum, 1986).

44 Ernst Mayr and Richard Goodwin, "Biological Materials, Part I: Preserved Materials, and Museum Collections," pamphlet, Biology Council, Division of Biology and Agriculture, publication 399 (Washington, D.C.: National Academy of Sciences–National Research Council, [n.d., ca. 1955]).

45 David Hull, *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science* (Chicago: University of Chicago Press, 1988); Robin Craw, "Margins of Cladistics: Identity, Difference and Place in the Emergence of Phylogenetic Systematics, 1864–1975," in *Trees of Life: Essays in Philosophy of Biology*, ed. Paul Griffiths, Australian Studies in History and Philosophy of Science, vol. 11 (Dordrecht: Kluwer, 1992), pp. 65–107.

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# 5

# FIELD STATIONS AND SURVEYS

*Keith R. Benson*

Buoyed by the combination of optimism of understanding the natural world from Isaac Newton's version of the mechanical philosophy and the excitement of discovering natural artifacts of the natural world from naturalists such as Carl Linnaeus, Abraham Werner, and Georges Buffon, natural philosophers turned increasingly to studying nature *in* nature by the end of the eighteenth century and the beginning of the nineteenth century. Certainly the maturation of the cabinet tradition in the form of emerging national museums (Muséum d'Histoire Naturelle, British Museum) and national botanical gardens (Royal Botanical Gardens at Kew) at this same time underscores the importance of learning from the natural world. Furthermore, continued overseas expansion and exploration, especially in North America, the Indian subcontinent of Asia, and Australia, heightened European interests in this direction.

Many of these same eighteenth-century motivations continued into the nineteenth century and, moreover, may be described after the model of scientific transmission and development offered by George Basalla, which he developed by examining the early history of American science vis-à-vis science in England.[1] It is certainly appropriate to borrow from and to expand on Basalla, for much of the eighteenth-century interest in the natural world was exhibited by Europeans who observed nature outside of Europe, primarily within their colonial holdings. They collected specimens on voyages of discovery and recruited local colonialists to collect specimens that could later be sent back to European museums and universities following the return of the imperial explorers to their mother country (see MacLeod, Chapter 3, this volume). In large measure, however, Europeans did not build their own field stations or conduct their own national surveys until the latter half of the nineteenth century, roughly the same time these operations were conducted and constructed in the United States.

[1] George Basalla, "The Spread of Western Science," *Science*, 156 (1967), 611–22.



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The European model for nineteenth-century colonial exploitation of the natural world was patterned on the pioneering efforts of Joseph Banks (1743–1820), the English botanist, and Alexander von Humboldt (1769–1859), the Romantic German adventurer. Banks had accompanied Captain James Cook (1728–1779) on one of his early voyages to the Pacific Ocean, where Banks not only "discovered" a new penal colony for England (in New Holland's – or Australia's – Botany Bay) but also discovered many new specimens, several of which had potential horticultural value to England. Subsequent to his voyage and because of the newfound riches he discovered, Banks was able to convince the Admiralty Office to place a naturalist or a physician/naturalist aboard many of its voyages to the New World. Part of the job requirement was to collect specimens, which would then find their way back either to the British Museum or to Kew. Shortly after Banks's voyage, von Humboldt undertook his own visit to the New World, traveling to South America at the beginning of the nineteenth century and, following his return to Europe, publishing his romantic tale of adventure in the natural world along with his influential observations about the new landscapes he encountered. Both the Banksian collecting ideal and the Humboldtian notion of instrumental measurement informed and inspired most of the subsequent work done by Europeans in the nineteenth century.[2] Gradually, however, individual voyages of exploration were replaced by field stations, botanical gardens, and formally structured surveys, at least in territories colonized by Europeans. A system of organized investigation established by the European nations for their home territory, and rapidly copied in North America, soon expanded on a global scale.

Of course, one of the major preoccupations of these European naturalists was to understand the vexing but wonderful phenomenon of biogeographical distribution. Given the eighteenth-century ideas of species' placement and perfect adaptation, it was striking to these explorers that most geographical locations had distinctive faunal and floral characteristics, even if the physical characteristics of these landscapes resembled European settings. Banks wondered about the surprising diversity and uniqueness of the plants and animals he observed in Australia. Humboldt suggested that altitude mirrored latitude in regulating the distribution of floral species. It was therefore not surprising that other naturalists who pondered these same questions often desired to visit the New World and observe these characteristics for themselves. Thus, Charles Darwin (1809–1882) jumped at the opportunity to voyage aboard HMS *Beagle* in 1831, not knowing that his illness-filled voyage would last

[2] For more on Joseph Banks, see Harold B. Carter, *Sir Joseph Banks* (London: British Museum, 1988). Humboldt's exciting tale was translated as *Personal Narrative of Travels to the Equinoctial Regions of the New Continent during the Years 1799–1804* (London, 1814–29, 7 vols.). On Humboldt's role in developing the notion of Humboldtian Science, see Susan Faye Cannon, *Science in Culture* (New York: Science History Publications, 1978), pp. 73–110.

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almost five years instead of the planned two years.[3] Darwin's colleague with a specialty in botany, Joseph Dalton Hooker (1817–1911), and his protector ("Darwin's bulldog"), Thomas Henry Huxley (1825–1895), both set sail at mid-century for regions of the New World with an interest in the intriguing biogeographical forms.[4]

## SURVEYS IN NATURE

It would be erroneous, however, to overemphasize just the scientific dimension of these excursions into nature. After all, as David Allen and Lynn Barber have argued, the nineteenth century also represented the "heyday of natural history," not just within the scientific community but within the literate lay community as well.[5] With a long and vested interest in nature through the cabinet tradition and the new museum craze, Europeans represented a ready market for naturalists who were willing to venture into the still dangerous New World to bring back or to send back specimens for exhibit or commercial sale. Certainly Alfred Russel Wallace (1823–1913) and Henry Walter Bates (1825–1892) recognized the potential for financial gain, given the market conditions at mid-century. Traveling together in South America at mid-century, both naturalists experienced directly both the assets and the liabilities of such an undertaking. Despite early difficulties and personal tragedies in his first arduous journey throughout the Amazon River basin, Wallace undertook a second expedition in the early 1850s to the Malay Archipelago, a journey that combined entrepreneurial risks with collections, observations, and theory making. It was on this trip, for example, that Wallace attained his reputation as a naturalist and as the codiscoverer of evolution by means of natural selection.

Natural history also benefited from the popularity of natural theology in England as well as the turn-of-the-century German tradition of *Naturphilosophie*. Natural theology directed the attention of England's divines to study nature for evidence of God's beneficence. Romantic poets and writers found inspiration from the idealistic notions of *Naturphilosophie* and looked to

[3] The book often known as Darwin's *Voyage of the Beagle* was first published separately as *Journal of Researches into the Geology and Natural History of the Countries Visited by H.M.S. Beagle* (London: H. Colburn, 1839). This was a reissue of the work originally published under the title *Journal and Remarks* as volume 3 of Robert Fitzroy, *Narrative of the Surveying Voyages of H.M.S. Adventure and Beagle between the Years 1826 and 1836* (London: H. Colburn, 1839, 3 vols.).

[4] Hooker's and Huxley's biological work and relationship with Charles Darwin are related in two excellent biographies on Darwin: Adrian Desmond and James Moore, *Darwin* (London: Michael Joseph, 1991); and Janet Browne's two-volume biography, *Charles Darwin: Voyaging* (New York: Knopf, 1995), and *Charles Darwin: The Power of Place* (New York: Knopf, 2002). For additional information on Huxley, see Adrian Desmond, *Huxley: The Devil's Disciple* (London: Michael Joseph, 1994); Adrian Desmond, *Huxley: Evolution's High Priest* (London: Michael Joseph, 1997).

[5] David Allen, *The Naturalist in Britain: A Social History* (Princeton, N.J.: Princeton University Press, 1976); Lynn Barber, *The Heyday of Natural History, 1820–1870* (Garden City, N.Y.: Doubleday, 1980).

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the assumed goodness of the natural world to escape the dreary urban settings, often spoiled by industrial pollution by the early nineteenth century. But whatever the theoretical motivator, the outcome was a heightened interest in the study of nature in the natural world. By mid-century, Europeans who had wandered the globe began to return to the British Isles or to the Continent for natural history expeditions, sometimes as part of a new tradition suggestively referred to as *Wanderjahre* and sometimes as a continuation of the studies they had done in the New World. This activity was particularly popular in the German states. Johann Wolfgang von Goethe (1749–1832), Humboldt, and Ernst Haeckel (1834–1919) all undertook the naturalist's version of the Continental Tour, collecting innumerable naturalistic observations en route and inspiring countless devotees in the process.

As the century progressed, the emphasis increasingly switched to more organized surveys following the models already established in Europe itself. The Royal Botanical Gardens at Kew, just outside London, had become the center from which the botanical riches of the British Empire were explored and exploited. But soon there were botanical gardens in the colonies themselves – in British-controlled India, there was an important garden at Calcutta, and the Dutch established a garden at Buitenzorg in Java. Geological surveys were also established in many colonized countries, following the European and American models discussed here.[6]

Given the colonial implications of Basalla's thesis, perhaps it is not surprising that the strongest tradition of studying nature in nature occurred in North America. Following the War of Independence, the new country of the United States suddenly found itself cut off from its colonizers and from the institutions of the mother country. An embryonic community of naturalists soon began to establish societies and museums, chiefly in Philadelphia, Boston, and New York and other metropolitan centers on the East Coast, but also in the leading intellectual center of the South, Charleston.[7] One of the supporters of this movement was the diplomat, politician, and polymath Thomas Jefferson. While conducting his own survey of his native state of Virginia, Jefferson became particularly interested in refuting Georges Buffon's (1710–1788) claims that New World specimens, living in a colder climate than in Europe, should exhibit degenerated forms. Spurred on by the republican optimism inherent in the new country and his own bias toward proving the salubrious nature of North America, Jefferson sought and found larger (and better?) specimens of almost every animal analogue to the European forms.[8]

6 Lucille Brockway, *Science and Colonial Expansion: The Role of the Royal Botanical Garden, Kew* (New York: Academic Press, 1979).

7 Brooke Hindle, *The Pursuit of Science in Revolutionary America, 1735–1789* (Chapel Hill: University of North Carolina Press, 1956).

8 Thomas Jefferson, *Notes on the State of Virginia*, ed. William Peden (Chapel Hill: University of North Carolina Press, 1955).

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Jefferson soon expanded his interests beyond Virginia. Long interested in the western expanse of the new country, then prompted by purported Spanish and French collusion for territorial expansion in North America, Jefferson succeeded in obtaining the necessary funds to send an expedition to the Far West headed by Meriwether Lewis and William Clark.[9] Setting across the country in 1803, the explorers searched, mapped, and observed the western route to the Pacific up the Missouri River system and down the drainage area of the Columbia River. Returning to the East Coast in 1806, they carried back to the nation's capital their own magnificent visions of the West as well as many natural history artifacts.[10]

Although it would be an exaggeration to call the Lewis and Clark expedition a venture in science (neither Lewis nor Clark had sophisticated scientific training, except for a quick review of botany from Benjamin Rush), the expedition did point to the value of such undertakings to survey the largely "empty" western reaches of the country. Following the purchase of the Louisiana Territory in 1803, the federal government sent several other survey parties westward, many of which were Army expeditions. Again, science was not the major focus, although several naturalists accompanied these surveys, either to collect specimens, conduct critical meteorological or geographical observations, or depict the character of natural landscapes.

The most important government-sponsored survey for its influence on the early development of American science was the U.S. Exploring Expedition, sent out under the guidance of Charles Wilkes, a naval officer, in 1838. Accompanied by several naturalists (called "scientifics" by Wilkes), the expedition ventured southward in the Atlantic, accidentally (and unknowingly) observing Antarctica, before entering the Pacific and voyaging through the South Pacific. Eventually, the expedition sailed to the northwestern coast of the United States, exploring Puget Sound, the Oregon Territory, and northern California before returning to the East Coast.[11] The importance of the expedition was not apparent immediately after Wilkes and his men returned, however. Indeed, whereas many of the men expected a hero's welcome, their arrival barely generated any notice. Instead, the publications from the expedition and the natural artifacts that were collected along its routes were to remain its lasting legacy, along with its geographical charts. Especially

[9] Stephen E. Ambrose has written a best-seller documenting aspects of this trip, *Undaunted Courage* (New York: Simon and Schuster, 1996). The best sources of information about the trip are the journals; see Gary Moulton, ed., *The Journals of the Lewis & Clark Expedition* (Lincoln: University of Nebraska Press, 1988).

[10] Many of the natural history artifacts from the expedition found their way back to the American Philosophical Society, which for lack of space sent them to Peale's museum in Philadelphia. As Peale liquidated his holdings, some of the artifacts finally made it to the new (1812) American Academy of Natural Sciences in Philadelphia.

[11] William Stanton, *The Great United States Exploring Expedition of 1838–1842* (Berkeley: University of California Press, 1975). A beautiful edition of the voyage was produced by Herman J. Viola and Carolyn J. Margolis, eds., *Magnificent Voyagers* (Washington, D.C.: Smithsonian Institution Press, 1985).

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important were Charles Pickering's anthropological observations on indigenous populations, Horatio Hale's translation of the Chinook language, and James Dana's (1818–1895) influential work on coral islands, all finally published by 1850. The specimens they gathered were also influential, first stored in the basement of the U.S. Patent Office but eventually serving as the base for the natural history collections of the new Smithsonian Institution (1846) following the Civil War.

At the same time, the mere gathering of natural artifacts did not represent the sine qua non of nineteenth-century natural history. A. G. Werner's (1749–1817) influential geological system, which provided a useful classification of rock types at the end of the eighteenth century, enabled mineralogists not just to identify specific rock types but also to search with greater reliability for mineral deposits that had economic and/or industrial applications (see Lucier, Chapter 7, this volume). Similarly, the founders of the British Geological Survey in the early nineteenth century justified their project in terms of its value to the search for coal deposits rather than its contributions to the theoretical principles of geology, although the survey did become deeply embroiled in debates over stratigraphy. American naturalists, perhaps with an even greater interest in the application of science, eagerly undertook their own geological surveys, originally under the auspices of the country's many states. By 1840, many of these investigators had met together in Philadelphia to form the American Association of Geologists and Naturalists, one of the earliest "professional" societies for scientists in the United States (and the forerunner of the American Association for the Advancement of Science).[12]

It is worth noting that geological and other surveys were dependent on systematic mapping to provide them with a geographical framework. The British survey used the maps prepared by the Ordnance Survey, which had begun mapping the country for military purposes in the previous century. In India, the British instituted the Trigonometrical Survey, which provided the first measurement of the subcontinent's dimensions and also contributed to debates on the exact shape of the earth itself.[13] The name of its second director, George Everest (1790–1866), was eventually given to the world's highest mountain. Colonial expansion was a significant factor in the encouragement of wider exploration, Britain's Royal Geographical Society being typical of the kind of semiformal organization that promoted and sometimes financed expeditions to many parts of the world. Its most active director, Sir Roderick Murchison (1792–1871), had made his name in part by mapping parts of Russia using British geological techniques – a form of intellectual

[12] Sally Gregory Kohlstedt, *The Formation of the American Scientific Community* (Urbana: University of Illinois Press, 1976).

[13] For a popular account, see John Keay, *The Great Arc: The Dramatic Tale of How India Was Mapped and Everest Was Named* (London: HarperCollins, 2001).

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"conquest" that paralleled the rush to colonize underdeveloped parts of the world.[14]

Geological surveys were commonplace throughout the nineteenth century in Europe, England, and North America because of their great utility. But perhaps the locale that attracted the most geological interest in the nineteenth century was the vast and varied terrain of the American West. European geologists, most notably Charles Lyell (1797–1875), visited the region on several occasions, mainly to observe if the geological phenomena had any bearing on the theoretical debates between the catastrophists and uniformitarianists. American geologists, including James Dwight Dana, Edward Hitchcock (1793–1864), and James Hall (1811–1898), enjoyed reputations throughout England and Europe based on their observations of American geological phenomena. In large part, the observations were related to the work of a state geological survey or, after 1878, the U.S. Geological Survey.

Prior to the American Civil War, several other surveys also had a marked impact on the development of science in the United States. First, cartographers and meteorologists in the Army continued to survey the West, primarily for accurate determination of national boundaries along the country's northern and southern reaches. Then, beginning in the late 1840s, the federal government actively encouraged (through economic incentives) several transcontinental surveys to determine the best routing for railroad travel. These railroad surveys produced a treasure trove of geological and natural historical observations.[15] They were quickly followed by many societal and private surveys that often investigated the West for paleontological information, data that were given new importance with the publication of Charles Darwin's epochal work *On the Origin of Species* (1859). Searching for information that would shed light on Darwin's new ideas, fieldworkers soon made exciting, provocative, and controversial discoveries; exemplified by the competitive paleontologists Othniel Marsh (1831–1899) and Edward Drinker Cope (1840–1897), both of whom sent specimens to East Coast museums and reports to East Coast newspapers to document their paleontological priority. Finally, and probably most important, was the U.S. Coast Survey, begun early in the nineteenth century but reaching its most productive years when it was directed by Benjamin Franklin's great-grandson Alexander Dalles Bache (1806–1867), beginning in 1843.[16] The survey had as its goal the accurate mapping of the Atlantic and Pacific coastlines of the United States, both of which remained largely uncharted even at mid-century. At the same time, however, naturalists aboard the survey's vessels were encouraged to conduct

[14] Robert A. Stafford, *Scientist of the Empire: Sir Roderick Murchison, Scientific Exploration and Victorian Imperialism* (Cambridge: Cambridge University Press, 1989).

[15] John A. Moore, "Zoology of the Pacific Railroad Surveys," *American Zoologist*, 26 (1986), 311–41.

[16] On the complex politics surrounding the Coast Survey, see Thomas G. Manning, *US Coast Survey vs. Naval Hydrographic Office: A 19th-Century Rivalry in Science and Politics* (Tuscaloosa: University of Alabama Press, 1988).

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their own terrestrial observations about the natural world. Alexander Agassiz in this manner was exposed to the "natural history of the sea," an interest he was to pursue for most of his scientific lifetime. On the West Coast, the survey's local director in California, George Davidson, also had a more global perspective, using his San Francisco office of the Coast Survey to launch a natural history society, the California Academy of Science, in 1853.[17] This new organization played a crucial role in natural history explorations of the West Coast, especially because it predated any academic institutions with this orientation.

At the same time, natural history pursuits were not restricted to terrestrial habitats or shoreside studies. As mentioned earlier, voyages of discovery had enjoyed a long tradition by the nineteenth century. By the middle of the century, however, the character of many of these voyages began to change, both to reduce the geographical scope of the voyages and to increase their topical focus. The century's most famous voyage, the *Challenger* expedition (1872–6), commanded by Charles Wyville Thompson (1830–1882), was one such enterprise. Instead of focusing on distant landscapes, the crew of the HMS *Challenger* examined the sea itself; its depth, the regular oceanic currents, wind patterns, and its fauna and flora became the foci of the work of its crew and naturalists. The numerous reports that followed the completion of the expedition served both to compile information gathered on the voyage and to inspire other naturalists to continue the work. In the United States, Alexander Agassiz (1835–1910), once he had accrued a massive fortune from the copper industry and shed his inherited duties at Harvard's Museum of Comparative Zoology (founded by his father, Louis Agassiz, who died in 1873, leaving the MCZ under his control), followed the direction of Thompson's *Challenger*. Privately funding his studies aboard the *Albatross*, Agassiz picked up his nascent interest in oceanography from his 1859 cruise with the Coast Survey and rapidly developed a career in the new emerging discipline of oceanography, particularly studying the Atlantic Ocean and the Caribbean Sea at the end of the nineteenth century and the beginning of the twentieth. At the same time, in Southern Europe, Agassiz's marine colleague and Monaco's naturalist-inclined ruler, Prince Albert I, initiated his own oceanic research. His operations were based from a new institution on the cliffside of Monaco, the Museé Océanographique, and conducted on a number of seagoing vessels that plied the waters of the Mediterranean and central Atlantic.[18]

[17] For more on Davidson, the California Academy of Sciences, and geology in California during the latter half of the nineteenth century, see Michael L. Smith, *Pacific Visions: California Scientists and the Environment, 1850–1915* (New Haven, Conn.: Yale University Press, 1987).

[18] Jacqueline Carpine-Lancre, who was the archivist at the Museé Océanographique in Monaco, has written extensively on Prince Albert I's contributions to oceanography. A recent commemorative volume produced at the request of Prince Rainier was based on Carpine-Lancre's historical work. It is an excellent overview of Prince Albert I's life and scientific achievements. See *Albert Ier, Prince de Monaco, des oeuvres de science, de lumière et de paix* (Monaco: Palais de S. A. S. le Prince, 1998).

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On Europe's northern boundaries, interest in the ocean came from an additional and distinct concern, that of the health of the North Sea fishery. During the 1880s, annual declines in the profitable and plentiful fisheries of the North Sea and the Baltic led to several national and international biological surveys of the ocean, especially following the International Fisheries Exhibition in 1883, where T. H. Huxley called for scientific studies of the sea. Scandinavian naturalists, led by Otto Pettersson and C. G. J. Petersen, examined the benthic areas of the western Baltic, hoping to identify the source for the decline in the plaice population. German, Scandinavian, Dutch, and English naturalists, particularly those biologists associated with Victor Hensen (1835–1924) and his "Kiel school" of research, zeroed in on the dynamics of planktonic organisms floating near the ocean's surface, the "blood of the sea," to determine if these organisms held any clues to decreases in the cod fishery to the north.[19] By the early twentieth century, both efforts had coalesced into the formation of the International Council for the Exploration of the Seas (ICES), the first international cooperative scientific enterprise and one that eventually expanded its concerns from fisheries to pure research concerning the earth's oceans. Importantly, ICES also helped to establish the research agenda that was to form the disciplinary identity for twentieth-century oceanography.[20]

## FIELD STATIONS

For most of the nineteenth century, therefore, studies of nature in nature were usually conducted within the framework of the scientific survey. In Europe, the work of the survey was taken over, in the second half of the century, by the emergence of the scientific laboratory, most commonly in the form of marine laboratories and terrestrial field stations. These institutions, which varied in the character of their research pursuits, can be accurately traced to the hydrographic work of the oceanic surveys, the economic factors related to declines of intertidal and open-ocean fisheries as well as general agricultural concerns, the educational reforms leading to the development of research programs in biology and geology, and finally, but perhaps most importantly,

[19] Eric Mills, *Biological Oceanography: An Early History, 1870–1960* (Ithaca, N.Y.: Cornell University Press, 1989).

[20] There have been five international meetings on the history of oceanography, each producing a volume with selected papers from the meeting. See the special edition "Communications-Premier congrès international d'histoire de l'océanographie, Monaco, 1966," *Bulletin de l'Institut océanographique, Monaco*, 2 (1972), xlii–807; "Proceedings of Second International Congress on the History of Oceanography. *Challenger* expedition centenary; Edinburgh, September 12–20, 1972," *Proceedings of the Royal Society of Edinburgh*, 72 (1972), viii–462; 73 (1972), viii–435; Mary Sears and D. Merriam, eds., *Oceanography: The Past* (New York: Springer, 1980); Walter Lenz and Margaret Deacon, eds., "Ocean Sciences: Their History and Relation to Man," *Deutsche Hydrographische Zeitschrift, Ergänzungsheft*, 22 (1990), xv–603; Keith R. Benson and Philip F. Rehback, eds., *Oceanographic History: The Pacific and Beyond* (Seattle: University of Washington Press, 2002).

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to the publication of Darwin's influential work in 1859. The almost immediate importance accorded embryological investigations of marine organisms following the appearance of *On the Origin of Species* led to the necessity of studying the natural world no longer just in nature but in new biological laboratories located along the ocean's shore, where there were rich supplies of embryonic organisms.

The first of these stations was at Concarneau (1859), a small laboratory of the College de France, directed by Victor Coste and dedicated to marine zoology and physiology. This station set the pattern for several other small French marine laboratories scattered along France's Atlantic and Mediterranean coastlines, including Banyul (1863), Roscoff (1872), Wimereux (1874), and the fascinating Russo-Franco station (it had served as a Russian coaling depot and prison, then as a research station!) at Villefranche (1885). To the north, marine stations were established at the end of the nineteenth century in Kiel (1870), Kristeneberg (1877), Bergen (1892), and Helgoland (1892), primarily for economic reasons related to understanding problems associated with fisheries. Similar motivations led to the founding of several laboratories in the British Isles, including Millport (1885), Plymouth (1888), and Port Erin (1891), to name the most prominent.[21] The Plymouth laboratory was maintained by the Marine Biological Association, founded in part because of the efforts of one of T. H. Huxley's disciples, E. Ray Lankester (1847–1929). By the beginning of the twentieth century, when Charles Kofoid was sent by the U.S. government to survey the state of biology marine stations (including freshwater laboratories) in Europe, there were over one hundred in operation.

Most of these early stations were either adjunct summer laboratories for universities (French stations) or were directed to address fisheries-related problems and, as such, did not sponsor pure research in biology. However, a laboratory that offered a new direction and that became the main innovative influence behind the formation of twentieth-century biology stations was the Stazione Zoologica in Naples, founded by Anton Dohrn (1840–1909) in 1872 and opened for visiting researchers in 1874. It quickly became an international research station, investigating biological questions relating to marine organisms and marine habitats. Soon, Naples was considered to be the "Mecca for biologists," subsequently spawning similar laboratories with an aim toward pure research beside the ocean's shore.[22] E. Ray Lankester had been one of Dohrn's earliest students in Naples and was inspired by

[21] An excellent and comprehensive overview of marine laboratories was written in 1956. See C. M. Yonge, "Development of Marine Biological Laboratories," *Science Progress*, 173 (1956), 1–15.

[22] The phrase "Mecca for biologists" was from C. O. Whitman, "Methods of Microscopical Research in the Zoological Station in Naples,"*American Naturalist*, 16 (1882), 697–706, 772–85. It soon became commonplace at the end of the nineteenth century. See Christiane Groeben, "The Naples Zoological Station and Woods Hole," *Oceanus*, 27 (1984), 60–9. See also the collection "The Naples Zoological Station and the Marine Biological Laboratory: One Hundred Years of Biology" issued as a supplement to *Biological Bulletin*, 168 (1985).

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this experience in his campaign for the creation of the Marine Biological Association.

In North America, scientific stations were constructed shortly after the stations emerged in Europe. In fact, the same pattern in which natural history surveys gave way to biological field stations was repeated in the United States, as the federal government did not sponsor surveys after the Civil War to the same extent that they had been sponsored earlier in the century.[23] However, the rapid growth of these stations did not occur until the twentieth century, in large measure because the exact character of the early marine stations was distinctly different. Thus, the two "final" expeditions or surveys of the nineteenth century, the Columbia University expedition to the Puget Sound region of Washington state, directed by E. B. Wilson, and the Harriman expedition to Alaska, serve as symbolic endpoints of the survey tradition, both taking place in 1899.[24]

There were nineteenth-century marine summer laboratories in the United States, or more accurately "summer schools," starting along the East Coast in 1873. That summer, Louis Agassiz, borrowing an idea from Nathaniel Shaler's (1841–1906) summer geological field station, opened his own summertime seaside school for teachers, a two-year venture that closed in 1874, one year after Agassiz's death. The idea was continued by Agassiz's student, Alpheus Hyatt (1838–1902), who opened another laboratory near Boston in 1881. This latter station ultimately led to the permanent foundation of the Marine Biological Laboratory (MBL) at Woods Hole on Cape Cod in 1888, a station that began its long and distinguished career as an educational summertime laboratory, much like Agassiz's station at Penikese.[25] To the south, in Chesapeake Bay, William Keith Brooks (1848–1908) established Johns Hopkins University's transient laboratory, the Chesapeake Zoological Laboratory (CZL), in 1878, the nation's first graduate-level research station. Ultimately, Brooks's students and other American biologists who had had the good fortune to travel to Naples at the end of the nineteenth century redirected the orientation of the MBL in Woods Hole to combine the research objectives of the CZL with the American tradition of teaching beside the sea. Thus, a new American model for marine stations was established, although the CZL

[23] A. Hunter Dupree, *Science in the Federal Government: A History of Policies and Activities to 1940* (Cambridge, Mass.: Harvard University Press, 1957), p. 148.

[24] E. B. Wilson, the well-known Columbia University cytologist, brought a class of students to study the diverse marine fauna and flora from a base encampment at Port Townsend, a small town located on the western shore of Puget Sound. For more information on the Harriman Expedition, see William H. Goetzmann and Kay Sloan, *Looking Far North: The Harriman Expedition to Alaska, 1899* (Princeton, N.J.: Princeton University Press, 1983).

[25] On Woods Hole, see the comparisons with the Naples station cited in note 22 and also Philip J. Pauly, "Summer Resort and Scientific Discipline: Woods Hole and the Structure of American Biology, 1882–1925," in *The American Development of Biology*, R. Rainger, K. R. Benson, and J. Maienschein, eds. (Philadelphia: University of Pennsylvania Press, 1988), pp. 121–50. Robert Kohler argues that field stations were seen as laboratories in the field (and hence less removed from nature), see Robert Kohler, "Labscapes: Naturalizing the Laboratory," *History of Science*, 40 (2002), 473–501.

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did not last past century's end.[26] Similar stations soon emerged along the country's western shoreline, including Stanford University's marine station in Pacific Grove (1892), the marine station endowed by the Scripps family in La Jolla (1903), and the University of Washington's marine laboratory in the San Juan Islands (1904).[27]

The botanical gardens founded by colonial powers in various parts of the world were intended in part to investigate native species of potential economic value – the Dutch East India Company's garden at Buitenzorg in Java was a prime example (see Cittadino, Chapter 13, this volume). Another type of biological laboratory that emerged in the nineteenth century was the agricultural field station, which had a decided economic focus. In Europe, many of these were patterned after Justus von Liebig's (1803–1873) influential animal chemistry laboratory at Giessen, which investigated application of the "new chemistry" to the production of foodstuffs. Other laboratories continued the nineteenth-century interest in horticulture, studies that quickly illustrated the value of experimental breeding studies in both plants and animals. Gregor Mendel's influential work on the variable characters of *Pisum* was done in Eastern Europe within this tradition (see Burian and Zallen, Chapter 23, this volume). In the United States, national leaders pushed for similar "experimental stations" to be built in association with universities and colleges with agricultural programs in every state, which quickly proved their worth.[28] By the twentieth century, agricultural field stations had become a part of the university institutional landscape throughout the world. In fact, these stations eventually served as the locus of many experimental studies of genetics, including the application of Mendelian principles to wheat genetics at Pullman (Washington state), R. A. Fisher's (1890–1962) population genetics work at Rothamstead, and Sewall Wright's (1889–1988) experimental work on genetics and evolution at the agricultural station in Madison (Wisconsin).

One additional model for field stations sprang from a combination of biological and physical questions concerning the sea, again stemming from the oceanic adventures during the nineteenth century. Voyages such as those of the *Challenger* acted not just to spur scientists to study the sea from the shoreline but also emphasized the importance of continued investigations of the sea from shipboard laboratories. Certainly Alexander Agassiz's efforts

26 That these stations represented valuable new institutions in the United States is underscored by the observation that the Bureau of Education sent C. A. Kofoid, a biologist at Berkeley, to Europe to survey all the biological stations. This important work was published as C. A. Kofoid, *Biological Stations in Europe* (Washington, D.C.: United States Bureau of Education, 1910).

27 Keith R. Benson, "Laboratories on the New England Shore: The 'Somewhat Different Direction' of American Marine Biology," *New England Quarterly*, 61 (1988), 55–78.

28 Charles Rosenberg was among the first historians to emphasize the importance of agricultural field stations in American science. See Charles Rosenberg, *No Other Gods: On Science and American Social Thought* (Baltimore: Johns Hopkins University Press, 1961). Rosenberg's suggestion was extended in Barbara Kimmelman, "A Progressive Era Discipline: Genetics and American Agricultural Colleges and Experiment Stations, 1890–1920" (PhD diss., University of Pennsylvania, 1987).

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and Prince Albert's ships continued this tradition. But it was probably the combination of ICES and the research agenda of the Kiel school that led to the formation of oceanography as a new scientific discipline and to the construction of oceanographic laboratories and research vessels as new scientific institutions. Primarily a northern European research focus until after World War I, oceanography came to the United States as a result of the pioneering efforts of Henry Bigelow, Frank R. Lillie, and T. Wayland Vaughan, all of whom served on the Committee on Oceanography of the National Academy of Sciences in 1927. Three years later, the committee report led to the formation of one new oceanographic institution, the Woods Hole Oceanographic Institution (WHOI), and the establishment of oceanographic programs at two existing institutions, changing the field stations at Scripps and the University of Washington into oceanographic laboratories. Funding for these programs came from the important philanthropic source the Rockefeller Foundation, creating a discipline that combined the features of the biological survey (oceanic travel) and the laboratory (shipboard investigations).

The two disastrous world wars of the twentieth century wreaked havoc on national traditions in oceanography in Europe, but a flourishing research tradition was developed in the Soviet Union beginning in the 1930s, combining interests in fisheries and the oceans, a tradition that emerged largely unscathed from the war.[29] Soviet research expanded after World War I, especially as it related to national security concerns associated with submarine warfare. Additionally, ICES continued its international focus following the war, ultimately forming several major oceanographic expeditions to mount large research efforts to understand better the deep ocean, ocean currents, and meteorological phenomena associated with oceanic conditions. And although fisheries concerns represented one of ICES's continued concerns, it did not represent the primary objective of the new direction of oceanographic research in the twentieth century. Largely because of oceanography's perceived practical application to naval research, physical, chemical, and geological priorities took precedence, especially in the United States, until the latter part of the twentieth century.

This overview of field stations and surveys is hardly an exhaustive one because it does not include stations and surveys conducted outside of a western European and North American context. Interests within the scientific community in Europe and North America for information about biogeographic diversity led to many important surveys of Africa, South America, Australia, and the South Pacific in the twentieth century. Concerns about biological pest control have also led to surveys undertaken in the far reaches of the globe to search for new species that might be used to control agricultural

[29] The history of oceanography in the Soviet Union is just now coming to light, largely through the efforts of Daniel Alexandrov and several of his students working under the auspices of the Russian Academy of Sciences in both Moscow and St. Petersburg.

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pests.[30] Important discoveries of paleontological finds in Asia and Africa have resulted in focused field explorations and surveys for additional information, especially in the twentieth century. The economic pressure on the world's oceans has also led to the proliferation of fisheries centers, especially in the form of small coastal laboratories in Africa and South America. Parallel pressures from marine biologists to understand basic problems in biology have fueled the formation of marine field stations throughout the globe, many of which have followed the model from Naples. Thus, as we begin the twenty-first century, field stations and scientific surveys have become part and parcel of the modern scientific quest for information about the natural world.

[30] Richard C. Sawyer, *To Make a Spotless Orange: Biological Control in California* (Ames: Iowa State University Press, 1996).

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# 6

# UNIVERSITIES

*Jonathan Harwood*

Universities have been important to biology not merely by providing it with a home. Particular features of the university setting had a substantial impact on both the proliferation of new fields in the nineteenth century and the central questions that came to characterize those fields. The history of biological thought and practice must therefore make room for institutional history. Moreover, writing the history of "biology" poses particular problems. Unlike many subjects in the natural sciences (e.g., chemistry, physics) or the humanities (e.g., history, philosophy), "biology" has rarely been institutionalized as a single subject. Whenever the life sciences experienced growth within the universities, they displayed a remarkable tendency to be institutionalized separately rather than to remain together as an internally differentiated whole. Just why this has occurred is not clear, but its historiographical implication is that "biology" is best conceived as a collection of loosely connected areas of inquiry (I will call them "fields") sharing little more than their concern with living organisms.

That said, the status that these fields have occupied within the university has varied considerably. Some of them (e.g., zoology or botany) were *disciplines* in the sense that they were central to the curriculum and were institutionalized in separate departments (or "institutes") at most universities. But many fields were established for long periods of time without ever acquiring disciplinary status; for convenience, I will call them *specialties* (e.g., morphology, embryology, or cytology). Lacking a substantial clientele for their teaching, such fields nevertheless found a place at some universities either because they were seen to illuminate important theoretical issues (e.g., morphology studied the relations of form and function) or because they could provide a service to a lay clientele. Late nineteenth-century bacteriology, for example, initially gained a foothold via public health laboratories attached to medical schools because it could provide diagnostic information,

I thank my colleague John V. Pickstone for useful feedback on a draft of this chapter.



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while in some agricultural colleges bacteriologists provided pure cultures of nitrogen-fixing bacteria to farmers.[1]

Just why a given field came to occupy a particular status is an important question. To begin with, of course, statuses have varied over time; fields that achieved the status of disciplines typically began their academic careers as specialties. But some fields that enjoyed disciplinary status in the nineteenth century have since lost their centrality (e.g., plant systematics, natural history). In addition, some fields have had far more success than others in colonizing higher education. Institutes dedicated to botanical systematics, for example, were far more common in late nineteenth-century German universities than those for zoological systematics; departments of genetics were more common than departments of ecology at American or British universities before 1945. Finally, the status of a given field has varied considerably from one country to another. Departments of genetics or biochemistry were much more common in the United States before World War II than they were in Germany. In this chapter, I will suggest how we might account for these differences.

Since this chapter is intended as a contribution to historiographical discussion rather than a review of the literature, I have not tried to cover all of the life sciences and have largely omitted the earth sciences. I have also devoted relatively little space to the biomedical sciences (on which there is much literature)[2] and rather more to agricultural contexts because these have been surprisingly neglected by historians of biology. I begin with a rough chronological sketch of the emergence of various fields since about 1800. The second section focuses on the question of patronage in order to make sense of the patterns by which various fields were institutionalized. In the third section, I consider the impact of university structure on teaching and research. In the conclusion, I touch on certain issues that merit more attention.

## A MAP OF THE CHANGING TERRAIN

The life sciences found their earliest home within the medical faculty in the form of anatomy and botany. By the mid-eighteenth century, anatomy theaters had become the norm at German universities, but thereafter anatomical "institutes" – as sites for research – began to replace them. The earliest botanical gardens in Europe date from the sixteenth century and were usually

[1] Paul Clark, *Pioneer Microbiologists of America* (Madison: University of Wisconsin Press, 1961), p. 268. Much the same applied to entomology and biochemistry.

[2] See William Coleman and Frederic Holmes, eds., *The Investigative Enterprise: Experimental Physiology in 19th Century Medicine* (Berkeley: University of California Press, 1988); Andrew Cunningham and Percy Williams, eds., *The Laboratory Revolution in Medicine* (Cambridge: Cambridge University Press, 1992); W. F. Bynum and Roy Porter, eds., *Companion Encyclopedia of the History of Medicine* (London: Routledge, 1993).

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attached to medical faculties. By the eighteenth century, botany had become a standard part of the medical curriculum, taught by a separate professor of materia medica (e.g., Carl Linnaeus, who taught at Uppsala from 1741).[3]

But the life sciences were also to be found outside the medical faculty in a number of eighteenth-century universities. Although few then had nonmedical chairs of botany, chairs of "natural history" were more common, at least on the Continent. And by the early nineteenth century, there were chairs of natural history at half a dozen English, Scottish, and Irish universities, as well as at the older American universities (Harvard, Yale, Pennsylvania, Columbia, Princeton). By the late nineteenth century, the newly established American state universities were also generally equipped with a chair of natural history.[4] Latter-day wags have sometimes suggested that these chairs would be better described as "settees" because the occupant was expected to give courses on animals, plants, and minerals. But, from the late eighteenth century, mineralogy and geology were taught as separate subjects at Oxford, Cambridge, Edinburgh, and Dublin, and chairs of geology were established during the nineteenth century at most of the new British universities and in the United States at Pennsylvania, Columbia, Princeton, and several state universities.[5]

During the nineteenth century, the most significant new field to emerge within medical faculties was physiology. In Germany, responsibility for teaching physiology was initially assigned to professors of anatomy. By mid-century, only about a quarter of the German universities had independent chairs for the subject, but by 1870 nearly all did, and during the latter half of the century, the innovation spread to Britain and the United States.[6] Scholars have devoted an enormous amount of attention to the emergence of physiology, especially in Germany, for several reasons. Some sociologists interested in higher education have focused on this process as a case study of innovation within the reformed German university system, while some historians

[3] Hans-Heinz Eulner, *Die Entwicklung der medizinischen Spezialfaecher an den Universitaeten des deutschen Sprachgebietes* (Stuttgart: Ferdinand Enke, 1970); Lucille Brockway, *Science and Colonial Expansion: The Role of the British Royal Botanic Gardens* (New York: Academic Press, 1979); William Coleman, *Biology in the 19th Century: Problems of Form, Function and Transformation* (New York: Wiley, 1971); Ilse Jahn, Rolf Loether, and Konrad Senglaub, eds., *Geschichte der Biologie: Theorien, Methoden, Institutionen und Kurzbiographien* (Jena: Gustav Fischer, 1982), p. 268.

[4] Jahn, Loether, and Senglaub, *Geschichte der Biologie*, pp. 268–9; David Elliston Allen, *The Naturalist in Britain: A Social History* (Harmondsworth: Penguin, 1978). I have also drawn on a series of seventeen histories of American biology departments that were published between 1947 and 1953 in the journal *Bios* (for full bibliographical details, see *The Mendel Newsletter*, no. 17, 1979). I thank Ms. Ruth Davis, archivist at the Marine Biological Laboratory (Woods Hole, Mass.), for helping me to obtain these articles.

[5] Roy Porter, *The Making of Geology: Earth Sciences in Britain, 1660–1815* (Cambridge: Cambridge University Press, 1977), pp. 143–4; Roy Porter, "Gentlemen and Geology: The Emergence of a Scientific Career, 1660–1920," *Historical Journal*, 21 (1978), 809–36; *Bios* histories.

[6] Eulner, *Die Entwicklung*; Richard Kremer, "Building Institutes for Physiology in Prussia, 1836–1846: Contexts, Interests and Rhetoric," in Cunningham and Williams, *Laboratory Revolution in Medicine*, pp. 72–109.

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of medicine have seen it as marking the beginnings of "scientific medicine."[7] But, for historians of the life sciences, physiology was important because it was so often cited as a model by those in the late nineteenth century who championed the experimental method.

Outside of medical faculties, the most basic disciplines to be established during the nineteenth century were botany and zoology. In Europe, botany's shift away from medicine was often modeled on the Jardin des Plantes (1792), which had its own chairs of botany. By the early nineteenth century, for example, some universities had established chairs of botany linked to botanical gardens (e.g., at the new University of Berlin), the latter derived either from long-standing "medical gardens" or from royal gardens that had been donated for research. By the 1860s, nearly all German universities had chairs of botany. Separate chairs for zoology were established somewhat later. By the late eighteenth century, zoology (as well as botany) was being *taught* outside of medical faculties in Germany but usually by professors of "cameralism" (i.e., administrative sciences), who taught agriculture among other things. By the early nineteenth century, the Muséum d'Histoire Naturelle (of which the Jardin was a part) was again being seen as a model by, among others, Alexander von Humboldt, who persuaded the Prussian authorities to establish a chair of zoology jointly with a zoological museum at Berlin in 1810. Similar chairs, often combined initially with other subjects, spread gradually. By mid-century, only one-third of the nineteen German universities had chairs designated exclusively for zoology, and eight made no provision whatsoever. By the 1870s, nearly all had established separate chairs.[8]

In Britain, chairs of botany were established at both University College London and Kings College London at their foundings circa 1830, along with a chair of zoology at the former. Comparative anatomy began to be taught at several medical institutions in London in the late 1830s, but the next major institutional advances were chairs for zoology combined with comparative anatomy at Oxford (1860) and Cambridge (1866). In the United States, a few universities had chairs of zoology by the 1860s (e.g., Harvard, Yale, Wisconsin), but most were established in the 1880s and 1890s. During the latter period, a number of universities assigned their life scientists

[7] For a review of the literature to 1989, see J. V. Pickstone, "Physiology and Experimental Medicine," in *Companion to the History of Modern Science*, ed. R. C. Olby, G. N. Cantor, J. R. R. Christie, and M. J. S. Hodge (London: Routledge, 1990), pp. 728–42. On physiology and innovation, see Steven Turner, E. Kerwin, and D. Woolwine, "Careers and Creativity in 19th Century Physiology: Zloczower Redux," *Isis*, 75 (1984), 523–9. On physiology as "scientific medicine," see Arlene Tuchman, *Science, Medicine and the State in Germany: The Case of Baden, 1815–1871* (Oxford: Oxford University Press, 1993); Coleman and Holmes, *Investigative Enterprise*; Cunningham and Williams, *Laboratory Revolution in Medicine*. On physiology and experimental method, see Coleman, *Biology in the 19th Century*, chap. 7.

[8] Lynn Nyhart, *Biology Takes Form: Animal Morphology and the German Universities, 1800–1900* (Chicago: University of Chicago Press, 1995); Vera Eisnerova, "Botanische Disziplinen," in *Geschichte der Biologie*, 3rd ed., ed. Ilse Jahn (Jena: Gustav Fischer, 1998), pp. 302–23; Armin Geus, "Zoologische Disziplinen," in Jahn, *Geschichte der Biologie*, pp. 324–55.

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to departments of "biology": at Johns Hopkins, of course, though also at Pennsylvania, Columbia, Texas, North Carolina, and Wisconsin. Significantly, however, in most cases these had split within a decade into separate departments for zoology and botany.[9]

Broadly speaking, early nineteenth-century botany (in Germany) was dominated by plant systematics, whereas zoologists pursued a kind of animal biogeography. By mid-century, moves were well under way to make both disciplines more "scientific," by which the reformers meant laboratory investigation in histology, embryology, physiology, and comparative anatomy. Toward the end of the century, another round of methodological reforms, based on claims for the superiority of experiment, spawned a remarkable number of new specialties, usually originating in Germany before spreading elsewhere. The period between about 1870 and 1910 saw the emergence of experimental embryology, plant ecology, plant physiology, bacteriology, biochemistry, and genetics.[10] All of these fields soon had their own professional societies and journals, but the last four had also acquired departmental status at some universities by the First World War. So stark was the scale and speed of these changes that by 1920 "specialization" had become a source of concern among a number of biologists.

In the twentieth century, the most important new field was undoubtedly molecular biology. While taking shape during the 1930s and 1940s through the coalescence of older research traditions in genetics, microbiology, biochemistry, and physical chemistry, this interdisciplinary study of heredity, as well as the structure and function of macromolecules, was often conducted outside the universities: at the Institut Pasteur in Paris, Medical Research Council units at Cambridge and London, or Kaiser-Wilhelm Institutes in Berlin. In the United States, such work was usually carried out within universities, probably because funding from the Rockefeller Foundation made it easier for researchers to collaborate across departmental boundaries.[11] A slew of Nobel Prizes for such work in the 1950s and 1960s gave the field a very high profile, prompting several prominent biologists, especially in the

[9] Allen, *Naturalist in Britain*; Adrian Desmond, *The Politics of Evolution: Morphology, Medicine, and Reform in Radical London* (Chicago: University of Chicago Press, 1989); Mark Ridley, "Embryology and Classical Zoology in Great Britain," in *A History of Embryology*, ed. T. J. Horder, J. A. Witkowski, and C. C. Wylie (Cambridge: Cambridge University Press, 1985), pp. 35–68. On the United States, see the *Bios* histories.

[10] Garland Allen, *Life Science in the 20th Century* (New York: Wiley, 1975); Eugene Cittadino, "Ecology and the Professionalization of Botany in America, 1890–1905," *Studies in the History of Biology*, 4 (1980), 171–98. On microbiology in various countries, see Keith Vernon, "Pus, Beer, Sewage and Milk: Microbiology in Britain, 1870–1940," *History of Science*, 28 (1990), 289–325; Clark, *Pioneer Microbiologists of America*; Andrew Mendelsohn, "Cultures of Bacteriology: Formation and Transformation of a Science in France and Germany, 1870–1914" (PhD diss., Princeton University, 1996).

[11] Robert Olby, *The Path to the Double Helix* (London: Macmillan, 1974); Horace Judson, *The Eighth Day of Creation: The Makers of the Revolution in Biology* (New York: Simon and Schuster, 1979); Robert Kohler, *Partners in Science: Foundations and Natural Scientists, 1900–1945* (Chicago: University of Chicago Press, 1991); Lily Kay, *The Molecular Vision of Life: Caltech, the Rockefeller Foundation and the Rise of the New Biology* (New York: Oxford University Press, 1993).

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United States, to complain that organismic and populational biology were being devalued. An important outcome of this conflict at some universities was the proposal to dissolve existing departments and redistribute their staff along radically different lines (often in new departments dedicated to molecular, cellular, organismic, or population biology), a movement that has undoubtedly gained ground through the intense commercial interest in academic molecular biology since the 1980s. Although this represents perhaps the most important reorganization of the institutional landscape over the last century, so far we know very little about either the processes that led up to it or the cognitive consequences it may have had for research and teaching.[12]

So much for the general institutional transformations of the life sciences over the last two centuries. How are we to account for the *particular* ways in which specific fields have developed within universities?

## THE POWER OF PATRONS

A "patron" is usually taken to be a powerful individual or institution whose support, whether financial or sociopolitical, for some activity is crucial to its survival. But, in discussing the development of a science, it is important to define the term more broadly so as to include those groups or institutions who may not be particularly wealthy or powerful in themselves but who constitute en masse an important clientele for the activity. In what follows, accordingly we will look at how the status of various fields has been shaped by two kinds of patronage: the *supply* of funding for research and the *demand* for particular kinds of expert or knowledge.

Patronage in some form has been – and continues to be – essential for the establishment of any subject within the universities. Who counts as a patron has varied, depending on the structure of the university system

[12] On the arguments by Theodosius Dobzhansky, Ernst Mayr, and George Gaylord Simpson around 1960 defending the legitimacy of nonmolecular inquiry, see John Beatty, "Evolutionary Antireductionism: Historical Reflections," *Biology and Philosophy*, 5 (1990), 199–210. Some of the effects of these institutional tensions on research are discussed in Michael Dietrich, "Paradox and Persuasion: Negotiating the Place of Molecular Evolution within Evolutionary Biology," *Journal of the History of Biology*, 31 (1998), 85–111. On the events at Harvard in the late 1950s, see E. O. Wilson's insider account in his *Naturalist* (Harmondsworth: Penguin, 1996), chap. 12. On the reorganization at Berkeley, see Martin Trow, "Leadership and Organization: The Case of Biology at Berkeley," in *Higher Education Organization: Conditions for Policy Implementation*, ed. Rune Premfors (Stockholm: Almqvist & Wiksell, 1984), pp. 148–78. For British reorganizations, see Duncan Wilson, *Reconfiguring Biological Sciences in the Late Twentieth Century: A Study of the University of Manchester* (published by the Faculty of Life Sciences, University of Manchester, in association with the Centre for the History of Science, Techonology and Medicine, and produced by Carnegie Publishing, Lancaster, 2008), Duncan Wilson and Gael Lancelot, "Making Way for Molecular Biology: Implementing and Managing Reform of Biological Science in a UK University," *Studies in the History and Philosophy of Science, Part C: Biological and Biomedical Sciences,* 49 (forthcoming 2008), and Gael Lancelot, "The Many Faces of Reform: The Reorganisation of Academic Biology in Britain and France, 1965–1995" (PhD diss., University of Manchester, 2007).

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as well as on the political order of which it is a part. For the academic champions of a new field at an American private university before 1914, for example, cultivating good relations with wealthy individuals was essential. At a European state university, attention was more likely to be focused on the officials in relevant ministries. In democratic societies, it has made sense for academic entrepreneurs to direct their sales pitches at well-organized interest groups within the general public, such as farmers or physicians, whereas in dictatorships personal ties to high-ranking party officials or the military have been more important.

Clearly, patrons had to be persuaded that a new field was potentially important. But "utility" has been perceived in a variety of ways. To be sure, fields have often been valued for their practical relevance. As we have seen, the medicinal importance of plants accounts for botany's relatively early establishment in universities compared with zoology. But one reason for zoology's institutionalization at German universities from the early nineteenth century was its success in attaching itself to natural history museums, whose popularity among various social strata was by then well established.[13] In other cases, fields have secured institutional advantage by virtue of their ideological utility. At Oxford and Cambridge, as at numerous Protestant colleges in the United States in the early nineteenth century, for example, natural history found a place in the curriculum because of its importance for natural theology.

The diverse perceptions of utility are well illustrated in the recent literature on the establishment of physiology in the German states. The older view was that state support (at least in Prussia) was prompted by a commitment to the value of scholarship for its own sake (*Wissenschaft*). More recently, however, those historians who have begun to look at smaller German states have argued that the latter's aims in promoting physiology were utilitarian in several other senses. In Saxony, for example, the ministry of education was keen on experimental sciences as a spur to economic development. And the evidence is growing that, even in Prussia, when the state finally began to support scientific research on a large scale in the 1860s, its aims were economic rather than cultural. In Baden, state officials regarded physiology as appropriate for a modernizing society because it was "practical" in the sense that laboratory sciences conferred hands-on experience and manipulative skills as well as teaching students independent and analytical thinking. But the state was not the only influential agent that saw value in the new physiology. Although mid-century physiology possessed no demonstrable therapeutic value, medical students also found it attractive, and some doctors believed that physiologists' new instruments would increase their diagnostic skill, whereas others saw "scientific" reform of the medical curriculum as a way

[13] Jahn, Loether, and Senglaub, *Geschichte der Biologie*, pp. 269–71; Ilse Jahn, *Grundzuege der Biologiegeschichte* (Jena: Gustav Fischer, 1990), p. 301.

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of enhancing professional status. More generally, some have suggested that science of the laboratory sort enjoyed a definite cachet among those early nineteenth-century middle-class circles who were championing the development of a new and progressive bourgeois culture.[14]

By the end of the nineteenth century, however, the form of utility that counted in most industrializing countries was broadly economic in character. For new fields in the life sciences, one principal route into the universities was via medicine; as we have seen, botany and physiology developed within the universities primarily via the medical connection. To some extent, the same was true for biochemistry. Around the turn of the century, many scientists studying the chemical basis of biological processes were employed either in departments of organic chemistry (in Germany) or physiology (in Germany, Britain), and the first departments created for the new field – in the United States around the First World War – were located in medical schools.[15]

On the other hand, despite its obvious importance, historians have so far paid much less attention to agricultural patronage. In the United States, from the 1860s, for example, an emphasis on increased agricultural productivity (tied to industrialization) prompted the rapid expansion of agricultural colleges and agricultural experiment stations, and from the 1880s the U.S. Department of Agriculture's (USDA) research divisions. Demand for agricultural scientists completely outstripped the supply, thus creating jobs aplenty for those trained as botanists or zoologists.[16] Similarly, certain newly emerging fields thought to be especially relevant to agriculture were institutionalized in agricultural colleges earlier than in the universities. In the United States, for example, "the new botany" got off to a fast start in the agricultural faculties of midwestern state universities, and by the mid-1880s most of the important American botany laboratories were located in such institutions. In Britain, William Thiselton-Dyer began his career in the 1870s at various agricultural institutions, as did a number of young Cambridge botany graduates in the 1890s. In Germany, Julius Sachs's first academic jobs were at colleges of forestry and agriculture; in Denmark, Wilhelm Johannsen spent the first twenty years of his career as a plant physiologist, initially at the Carlsberg

[14] For the classic view of Prussian science policy, see R. Steven Turner, "The Growth of Professorial Research in Prussia, 1818–1848: Causes and Context," *Historical Studies in the Physical Sciences*, 3 (1971), 137–82. For the revisionist view of physiology, see Coleman and Holmes, *Investigative Enterprise*; Cunningham and Williams, *Laboratory Revolution in Medicine*; Tuchman, *Science, Medicine and the State in Germany*.

[15] Robert Kohler, *From Medical Chemistry to Biochemistry* (Cambridge: Cambridge University Press, 1982); Harmke Kamminga and Mark Weatherall, "The Making of a Biochemist I: Frederick Gowland Hopkins' Construction of Dynamic Biochemistry," *Medical History*, 40 (1996), 269–92.

[16] The number of botanists employed in the USDA increased nearly twenty-fold (and entomology fifteen-fold) between 1897 and 1912. See Margaret Rossiter, "The Organisation of the Agricultural Sciences," in *The Organization of Knowledge in Modern America, 1860–1920*, ed. A. Oleson and J. Voss (Baltimore: Johns Hopkins University Press, 1979), pp. 211–48, at pp. 216–20; Barbara Kimmelman, "A Progressive Era Discipline: Genetics at American Agricultural Colleges and Experiment Stations, 1900–1920" (PhD diss., University of Pennsylvania, 1987), chap. 2.

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Laboratory and later at an agricultural college; and the American mycologist W. G. Farlow was first employed at Harvard's school of agriculture. Along with plant physiology, ecology was another main strand of the new botany. In the United States, almost all of the major centers of grassland ecology from the late nineteenth century to the mid-1950s were located at midwestern state universities, notably at the University of Nebraska, where Charles E. Bessey had promoted the new botany from his arrival in 1884.[17]

Microbiology fared similarly. In Britain, bacteriologists found jobs in departments of brewing (at Birmingham and Heriot-Watt), dairy science (University College Reading), and plant pathology (Cambridge School of Agriculture, Imperial College). In the United States, the greatest opportunities for both bacteriology and mycology were provided by departments of plant pathology (established at Berkeley in 1903, Minnesota in 1907, Cornell in 1907, and Wisconsin in 1909), though also in soil science or veterinary science. Biochemistry also took root in agricultural soil. A substantial minority of the early members of the American Society of Biological Chemists (established in 1906), for example, were employed at agricultural institutions. The situation in Germany was similar; during the decade between his classic demonstration of cell-free fermentation and his award of a Nobel Prize, Eduard Buchner held the chair of chemistry at the Berlin Agricultural College. Before the First World War, Carl Neuberg was head of the Chemical Division in the Institute for Animal Physiology at the college, while others worked in the college's institutes for fermentation chemistry, enzymology, and carbohydrate chemistry, as well as at Berlin's Veterinary College.[18]

[17] In 1896–7, for example, a USDA committee on educational reform recommended that all agricultural college curricula should include both general botany (including plant physiology and pathology) and general zoology (including entomology and physiology). See Kimmelman, "Progressive Era Discipline," chap. 2. On the new botany in the United States, see Cittadino, "Ecology and the Professionalization of Botany in America"; Richard Overfield, *Science with Practice: Charles E. Bessey and the Maturing of American Botany* (Ames: Iowa State University Press, 1993), chap. 4; Ronald Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895–1955* (Berkeley: University of California Press, 1981), chap. 5 and App. Table 4. On Ward and Thiselton-Dyer, see J. Reynolds Green, *A History of Botany in the United Kingdom* (London: Dent, 1914); Bernard Thomason, "The New Botany in Britain ca. 1870 to ca. 1914" (PhD diss., University of Manchester, 1987); Martin Bopp, "Julius Sachs," *Dictionary of Scientific Biography*, XII, 58–60; L. C. Dunn, "Wilhelm Johannsen," *Dictionary of Scientific Biography*, VII, 113–15. On Farlow, see W. M. Wheeler, "History of the Bussey Institution," in *The Development of Harvard University since the Inauguration of President Eliot, 1869–1929*, ed. Samuel E. Morison (Cambridge, Mass.: Harvard University Press, 1930), pp. 508–17.

[18] On microbiology, see A. H. Wright, "Biology at Cornell University," *Bios*, 24 (1953), 123–45; Vernon, "Pus, Beer, Sewage, and Milk"; Clark, *Pioneer Microbiologists of America*; Kenneth Baker, "Plant Pathology and Mycology," in *A Short History of Botany in the United States*, ed. Joseph Ewan (New York: Hafner, 1969), pp. 82–8. On American agricultural chemistry, see Rossiter, "Organisation of the Agricultural Sciences," pp. 228–9; Charles Rosenberg, *No Other Gods: On Science and American Social Thought* (Baltimore: Johns Hopkins University Press, 1976), chap. 9. On German biochemistry, see Herbert Schriefers, "Eduard Buchner," *Dictionary of Scientific Biography*, II, 560–63; Michael Engel, "Paradigmenwechsel und Exodus: Zellbiologie, Zellchemie und Biochemie in Berlin," in *Exodus von Wissenschaften aus Berlin: Fragestellungen, Ergebnisse, Desiderate*, ed. Wolfram Fischer et al. (Berlin: Walter de Gruyter, 1994), pp. 296–341.

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In the case of genetics, the first American departments were located in the agricultural faculties at California (Berkeley), Cornell, and Wisconsin, and one of the principal professional societies in which the new Mendelians met before 1914 was the American Breeders Association. When genetics was first established at Harvard, it was situated not in botany or zoology but in the School of Agriculture, and in Germany the only department dedicated exclusively to genetics before 1945 was at the Agricultural College in Berlin. In Britain, the major center for postgraduate training before 1945 was the Department of Research in Animal Breeding at Edinburgh. Numerous early Mendelians were initially employed in agricultural institutions, among them Hermann Nilsson-Ehle (Swedish Plant-Breeding Station at Svaloef), Erich von Tschermak (Agricultural College in Vienna), William Bateson (John Innes Horticultural Institution), and Raymond Pearl (Maine Agricultural Experiment Station).[19]

Thus the rising demand for expertise relevant to agriculture created important opportunities in several countries around 1900. At first sight, it may seem puzzling that such expansion also took place in Britain, where agriculture had been in decline for a generation. Although few historians have yet begun to explore the reasons for such expansion, it is likely that it was fueled in large part by imperial developments. A variety of colonial institutions employed biologists. Some colonial botanical gardens, for example, originally established in the eighteenth century as collection stations for valuable plants, became important research centers in the nineteenth (e.g., at Calcutta, Perideniya in Sri Lanka, and Buitenzorg in Java). Furthermore, colonial agricultural societies, experiment stations, and colleges of agriculture (e.g., the Imperial College of Tropical Agriculture in Trinidad, established in 1922) also employed substantial numbers of life scientists.[20] As Michael Worboys pointed out many years ago, colonial demand for botany and

[19] Kimmelman, "Progressive Era Discipline"; Barbara Kimmelman, "The American Breeders Association: Genetics and Eugenics in an Agricultural Context, 1903–1913," *Social Studies of Science*, 13 (1983), 163–204; Wheeler, "History of the Bussey Institution"; Jonathan Harwood, *Styles of Scientific Thought: The German Genetics Community, 1900–1933* (Chicago: University of Chicago Press, 1993); Margaret Deacon, "The Institute of Animal Genetics at Edinburgh: The First 20 Years," typescript, 1974; Arne Muentzing, "Hermann Nilsson-Ehle," *Dictionary of Scientific Biography*, X, 129–30; Robert Olby, "Scientists and Bureaucrats in the Establishment of the John Innes Horticultural Institution under William Bateson," *Annals of Science*, 46 (1989), 497–510; Kathy Cooke, "From Science to Practice, or Practice to Science? Chickens and Eggs in Raymond Pearl's Agricultural Breeding Research, 1907–1916," *Isis*, 88 (1997), 62–86. At Cambridge University, several plant breeders enthusiastic about the new Mendelism were located in the School of Agriculture, though the chair of genetics (est. 1912) was not (Paolo Palladino, "The Political Economy of Applied Research: Plant-Breeding in Great Britain, 1910–1940," *Minerva*, 28 (1990), 446–68); ibid., "Between Craft and Science: Plant-Breeding, Mendelian Genetics, and British Universities, 1900–1920," *Techonology and Culture*, 34 (1993), 300–23.

[20] Brockway, *Science and Colonial Expansion*; Eugene Cittadino, *Nature as the Laboratory: Darwinian Plant Ecology in the German Empire, 1880–1900* (Cambridge: Cambridge University Press, 1990); Christophe Bonneuil, "Crafting and Disciplining the Tropics: Plant Science in the French Colonies," in *Science in the Twentieth Century*, ed. John Krige and Dominique Pestre (Amsterdam: Harwood, 1997), pp. 77–96.

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zoology graduates was high. It has been estimated that about one-quarter of the life sciences graduates from Oxford, Cambridge, and Imperial College during the 1920s went into the Colonial Service. By 1932, one report indicated that of government jobs for biologists, there were 319 in Britain but 840 in the Colonial Empire. More concerned than any other government department with the supply of graduates in the life sciences, the Colonial Office made recommendations for the expansion of biological education, and certain fields were in particular demand. One of the activities undertaken by the African Entomological Research Committee (established in 1909) was to promote economic entomology through endowing posts and funding courses. At Imperial College, plant physiology and plant pathology flourished, thanks to J. B. Farmer's close connections with imperial organizations. And in 1922 the Empire Cotton Growing Corporation established a scholarship scheme for those studying genetics and plant breeding at the Cambridge School of Agriculture.[21]

The Empire's "pull" can be seen in the careers of young British graduates. On graduating from Cambridge in 1903, for example, the botanist W. L. Balls had a choice of two jobs: one in British Guiana and the other with an agricultural society in Cairo. Some young biologists stayed for only a few years until postgraduate training or an academic post back in Europe was obtained. On graduating from Cambridge in 1879, for example, the mycologist H. Marshall Ward spent two years as a government botanist in Sri Lanka, studying the causes of disease in the coffee plant, before returning to Britain; he soon became professor of botany at the Royal Indian Engineering College, where he prepared forestry students for jobs in the Empire. Ward's younger German contemporary Theodor Roemer did likewise; receiving his PhD in 1910, he entered the Colonial Service in German East Africa, where he spent two years in cotton breeding before returning to make an academic career in Germany. Others spent most of their careers in the colonies. A few years after graduating in botany, for example, Sydney Harland (1891–1982) took up a post at the experiment station in St. Croix (Danish West Indies), moving in 1923 to become professor of botany and genetics at the Imperial College of Tropical Agriculture, where his research was supported by the Empire Cotton Growing Corporation. Working thereafter at a series of colonial research institutions, he did not return to Britain until 1949, when he took up a chair at the University of Manchester.[22]

[21] Michael Worboys, "Science and British Colonial Imperialism, 1895–1940" (PhD diss., Sussex University, 1979), chaps. 5 and 7. On Imperial College, see Thomason, "New Botany in Britain," pp. 193–7. On Cambridge, see G. D. H. Bell, "Frank Leonard Engledow, 1890–1985," *Biographical Memoirs of Fellows of the Royal Society*, 32 (1986), 189–217.

[22] S. C. Harland, "William Lawrence Balls," *Biographical Memoirs of Fellows of the Royal Society*, 7 (1961), 1–16. On Ward, see Thomason, "New Botany in Britain," chap. 5; Lilly Nathusius, *Theodor Roemer: Lebensabriss und bibliographischer Ueberblick* (Halle: Universitaets- u. Landesbibliothek Sachsen-Anhalts, 1955); Joseph Hutchinson, "Sydney Cross Harland," *Biographical Memoirs of Fellows of the Royal Society*, 30 (1984), 299–316. See also D. W. Altman, Paul Fryxell, and Rosemary

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If we want to understand the paths along which the biological sciences have developed over the past century, we must therefore consider their perceived relevance to medical education and agriculture, whether domestic or colonial. Helpful though this utilitarian perspective is, it still leaves unexplained the rapid growth within the universities of those fields that *lacked* evident practical relevance. In these cases, the philanthropic foundations often played a decisive role. Although foundations have existed in Europe since the early twentieth century, their impact on academic science was limited prior to the Second World War because their resources were small compared with the scale of state funding. In the United States, however, where state support for basic sciences was very limited before 1945, the great wealth of the foundations – in particular, the Rockefeller and Carnegie philanthropies, both established in the years before the First World War – gave them considerable influence on the development of biological sciences in the universities during the interwar period.

It is well known that the Rockefeller Foundation played a major role during the 1930s and 1940s in funding the work that would later become "molecular biology." What has so far attracted less attention from historians, however, is the more general pattern of Rockefeller support for the life sciences during the interwar period; namely, that its funding was channeled heavily toward *laboratory* specialties. In the United States, genetics, embryology, general physiology, and reproductive biology (along with biochemistry and biophysics) were generously funded. During the 1920s, the Rockefeller Foundation's influence also extended to European universities via its International Education Board. In Britain, the IEB invested in microbiology at Oxford, Cambridge, and the London School of Hygiene and Tropical Medicine, as well as in genetics at Edinburgh. In Germany, the Rockefeller Foundation targeted genetics, biochemistry, experimental biology, and biomedical sciences. In contrast, evolution, systematics, and ecology received far less support. That is not to say that the Foundation never funded field biology; it did, but usually because the projects in question had some connection to laboratory biology. Thus, during the 1930s, Theodosius Dobzhansky got support for fieldwork on the population genetics of *Drosophila pseudoobscura*, and Ernest Babcock was funded to work on plant genetics and systematics. But when George Gaylord Simpson asked for money to study speciation in paleontological samples, his request was rejected on the grounds that the project did not "have much bearing on genetics or the problems of experimental biology."[23]

D. Harvey, "S. C. Harland and Joseph B. Hutchinson: Pioneer Botanists and Geneticists Defining Relationships in the Cotton Genus," *Huntia*, 9 (1993), 31–49.

[23] The quotation is from Joseph Cain, "Common Problems and Cooperative Solutions: Organizational Activity in Evolutionary Studies, 1936–1947," *Isis*, 84 (1993), 1–25, at p. 21. On the Rockefeller Foundation and molecular biology, see Kohler, *Partners in Science*; Pnina Abir-Am, "The Discourse of Physical Power and Biological Knowledge in the 1930s: A Reappraisal of the Rockefeller Foundation's

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Thus the pattern of patronage – be it the supply of funding for research or the demand for expertise – can explain why some academic fields have flourished and others languished at any given time. But the effects of patronage are not direct and unmediated; instead the effects of funding and demand have always been mediated by the institutional setting in which a field was practiced. This means that we must look more closely at the institutions in which life scientists have been employed because these constitute their immediate work environment. And we shall then see how institutions – organized in diverse ways with diverse consequences – have had a *formative* impact, shaping the intellectual development of fields.

## THE CONSEQUENCES OF INSTITUTIONAL LOCATION

A number of historians have drawn attention to the consequences for a field when it is situated in a medical environment. Biochemistry provides a good example. The most favorable circumstances for the establishment of this field as a discipline were to be found in newly reformed medical schools in the United States in the years before the First World War. But in this kind of niche, American biochemistry came to be dominated between the world wars by what Robert Kohler has called a "clinical style" of work that focused on developing analytical methods for the clinic and studies of human nutrition, respiration, and endocrinology. A "general biochemistry" – concerned with fundamental biological problems such as intermediary metabolism, growth, and cellular physiology – only emerged when biochemists could establish schools outside medical institutions, as did F. Gowland Hopkins at Cambridge or Otto Warburg in Berlin.[24]

The situation in physiology was similar. In Britain, physiology had been shaped by anatomical concerns until the 1870s, when Michael Foster began to argue for physiology as a branch of "biology" at Cambridge. Foster could promote this nonmedical vision of the field partly because he was based in Trinity College but also because the university's School of Medicine was

Policy in Molecular Biology," *Social Studies of Science*, 12 (1982), 341–82, and the responses to Abir-Am's paper by several authors in *Social Studies of Science*, 14 (1984), 225–63. The evidence for the Rockefeller Foundation's funding of other areas of biology is scattered throughout the literature, but see Robert Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994), chap. 7; Vassiliki Betty Smocovitis, "Botany and the Evolutionary Synthesis: The Life and Work of G. Ledyard Stebbins" (PhD diss., Cornell University, 1988). On European grant programs, see Paul Weindling, "The Rockefeller Foundation and German Biomedical Sciences, 1920–1940: From Educational Philanthropy to International Science Policy," in *Science, Politics and the Public Good: Essays in Honour of Margaret Gowing*, ed. N. Rupke (London: Macmillan, 1988), pp. 119–40; Jonathan Harwood, "National Styles in Science: Genetics in Germany and the United States between the World Wars," *Isis*, 78 (1987), 390–414; Robert Kohler, "Science and Philanthropy: Wickliffe Rose and the International Education Board," *Minerva*, 23 (1985), 75–95.

[24] Kohler, *From Medical Chemistry to Biochemistry*, chaps. 9–11; Kamminga and Weatherall, "Making of a Biochemist."

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restricted to preclinical teaching. In the United States, a generation later, Jacques Loeb, Charles Otis Whitman, and others also sought to promote a broad conception of physiology, but because it often brought them into conflict with the mainstream American physiological community, they found niches in institutions that either had no medical school (e.g., Chicago, the Rockefeller Institute for Medical Research) or in which they could keep their distance from clinicians (e.g., California-Berkeley, Harvard). More generally, Philip Pauly has argued that in the United States around the turn of the century, research programs in "biology" flourished at universities where medical faculties were weak (e.g., Columbia) or nonexistent (e.g., Chicago or Johns Hopkins through the 1880s).[25]

Historians of bacteriology have noticed a comparable phenomenon. The most common institutional locus for bacteriology before 1945 was the medical school, where research focused on culturing and classifying pathogenic strains or developing antibacterial agents. A more general "bacterial physiology" – the study of bacterial variation, adaptation, metabolism and nutrition, and ecology as phenomena of interest in their own right – tended to grow up in agricultural faculties (e.g., Iowa, Wisconsin, Helsinki), departments of biology (e.g., Stanford, Delft, the Calfornia Institute of Technology), or in biomedical research institutes, which were buffered from medical constraints (e.g., the Pasteur Institute, the Rockefeller Institute for Medical Research, several Medical Research Council–funded units in Britain). In Paris, André Lwoff and Jacques Monod, sharing a contempt for physicians, insisted on doing work of no direct relevance to medicine. After 1945, they therefore turned to the Centre Nationale de la Recherche Scientifique (CNRS), the Rockefeller Foundation, and American research councils in order to build up bacteriological and biochemical research.[26]

Once again, however, historians have so far paid less attention to the impact of agricultural contexts. In some cases, new fields have taken their fundamental assumptions or practices directly from agriculture. Many of those who championed the new Mendelism after 1900, for example, were already familiar with some of its basic methods (e.g., hybridization) and concepts

[25] Gerald Geison, *Michael Foster and the Cambridge School of Physiology* (Cambridge: Cambridge University Press, 1978); Philip Pauly, *Controlling Life: Jacques Loeb and the Engineering Ideal in Biology* (New York: Oxford University Press, 1987); Philip Pauly "General Physiology and the Discipline of Physiology, 1890–1935," in *Physiology in the American Context, 1850–1940*, ed. Gerald Geison (Bethesda, Md.: American Physiological Society, 1987), pp. 195–207; Jane Maienschein, "Physiology, Biology and the Advent of Physiological Morphology," in Geison, *Physiology in the American Context*, pp. 177–207; Philip Pauly, "The Appearance of Academic Biology in Late 19th Century America," *Journal of the History of Biology*, 17 (1984), 369–97.

[26] Robert Kohler, "Bacterial Physiology: The Medical Context," *Bulletin of the History of Medicine*, 59 (1985), 54–74; Olga Amsterdamska, "Medical and Biological Constraints: Early Research on Variation in Bacteriology," *Social Studies of Science*, 17 (1987), 657–87; Jean-Paul Gaudilliere, "Paris–New York, Roundtrip: Transatlantic Crossings and the Reconstruction of the Biological Sciences in Postwar France," paper presented at the Max-Planck-Institute for History of Science, Berlin, November 14, 2000.

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(e.g., the genotype–phenotype distinction) because during the 1890s they had been working in plant breeding, where these practices were well known.[27] What are often now referred to as the first international conferences of "genetics" were actually conferences devoted to plant breeding and hybridization, the vast majority of whose participants were either commercial horticulturists or employed in public-sector agricultural institutions. Turning to bacteriology, Andrew Mendelsohn has argued that the late nineteenth-century French emphasis on the ubiquity of germs and their capacity for productive work – in contrast with the Koch school's vision of germs as invasive and destructive agents – derives from the agricultural origins of Pasteur's early work (in contrast with the medical context of Koch's).[28]

Although we have so far been discussing only medical and agricultural contexts, the point is a general one. Where there is no single obvious institutional base for a new field in the life sciences, the kind of department, faculty, or university in which it is placed matters. A case in point is paleontology, which was sometimes situated in geology departments and at others in biological ones. When located in zoology departments, such as those at Columbia and Chicago (initially), paleontologists addressed general biological issues concerned with development, comparative anatomy, or evolution. In Germany and Austria, however, paleontology was routinely located in geology departments, with the consequence that its practitioners did not become interested in evolutionary theory until much later.[29]

So far I have been referring in rather general terms to "the university" as though this was a more or less homogeneous institution during the nineteenth and twentieth centuries. This was, of course, not the case; variations in the organization of universities as well as in their unspoken ethos have had

[27] Although breeders did not formally distinguish between "genotype" and "phenotype," they were well aware, at the latest by mid-century, that a plant's visible properties were not a reliable guide to its heritable ones. It was this knowledge that prompted the development of the "pedigree method" of individual selection. See Jean Gayon and Doris Zallen, "The Role of the Vilmorin Company in the Promotion and Diffusion of the Experimental Science of Heredity in France, 1840–1920," *Journal of the History of Biology*, 31 (1998), 241–62.

[28] On nineteenth-century hybridization work, see Kimmelman, "Progressive Era Discipline"; Barbara Kimmelman, "The Influence of Agricultural Practice on the Development of Genetic Theory," *Journal of the Swedish Seed Association*, 107 (1997), 178–86; Robert Olby, *Origins of Mendelism*, 2nd ed. (Chicago: University of Chicago Press, 1985). For the early conferences, see "Hybrid Conference Report," *Journal of the Royal Horticultural Society*, 24 (1900), 1–349; "Proceedings of the International Conference on Plant-Breeding and Hybridization," *Memoirs of the Horticultural Society of New York*, 1 (1902). Although the term "genetics" was eventually introduced at the 1906 meeting, the conference's full title was the "Third International Conference 1906 on Genetics; Hybridisation (the Cross-breeding of Genera or Species), the Cross-Breeding of Varieties, and General Plant-Breeding" (London: Royal Horticultural Society, 1906). See Mendelsohn, "Cultures of Bacteriology."

[29] Ronald Rainger, "Vertebrate Paleontology as Biology: Henry Fairfield Osborn and the American Museum of Natural History," in *The American Development of Biology*, ed. Ronald Rainger, Keith Benson, and Jane Maienschein (Philadelphia: University of Pennsylvania Press, 1988), pp. 219–56; Ronald Rainger, "Biology, Geology or Neither or Both: Vertebrate Paleontology at the University of Chicago, 1892–1950," *Perspectives on Science*, 1 (1993), 478–519; Wolf-Ernst Reif, "The Search for a Macroevolutionary Theory in German Paleontology," *Journal of the History of Biology*, 19 (1986), 79–130.

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substantial effects on the development of the life sciences. One such respect in which universities differed was the extent to which they saw fit to address "practical" problems. Around 1900, for example, one thinks in Britain of the civic universities of the industrial North versus Oxford and Cambridge, in the United States of the state land-grant universities versus the East Coast private universities, and in Germany of the technical colleges (*Technische Hochschulen*) versus the traditional universities. The life sciences found a home in all of these types of universities, a fact that would make it possible to assess the impact of such differences in ethos on the research process, though few historians have yet taken advantage of this opportunity.[30]

But universities have also varied in other ways. For example, although the same new fields emerged in several countries around 1900, it is noticeable that the problems deemed central to such fields varied from one place to another. Geneticists in the United States, for example, tended to focus on the more narrowly defined problems of transmission, whereas those in Germany or France took up genetic aspects of the long-standing problems of development or evolution. Something similar occurred in biochemistry. One reason for these differences of emphasis was that structural differences between the American and German universities made it relatively easy for academics in the former system to specialize (so that those in new fields could ignore the problems enshrined in older disciplines). In the German university, however, practitioners in new fields did not enjoy this freedom because they had to make their careers within established disciplines.[31]

This contrast between the "generalist" and the "specialist" conceptions of a field is also evident in British sciences, though its causes may have been somewhat different. In his history of the sciences at Oxford between the world wars, Jack Morrell has drawn attention to the consequences for research of the tutorial system of teaching. Because many colleges between the wars were quite small – two-thirds of them had not a single fellow in the life sciences, and most of the others had just one – they were keen to appoint fellows who could teach across the board. To send students outside the college in order to be taught by specialists was thought by some to be "dreadfully provincial." In their research, Morrell argues, fellows were inclined to turn this state of affairs to their own advantage by tackling wide-ranging problems, and it was work of this kind that also won approval within the colleges. Consistent with

[30] For a suggestive discussion of grassland ecology in the United States, see Tobey, *Saving the Prairies*, pp. 122–33. On the contrast between genetics at the University of Goettingen and that at the Berlin Agricultural College, see Harwood, *Styles of Scientific Thought*, chap. 6. For contrasts in England, especially between Cambridge and the Northern civic universities, see John V. Pickstone, "Science in Nineteenth-Century England: Plural Configurations and Singular Politics," in *The Organisation of Knowledge in Victorian Britain*, ed. Martin Daunton (published for the British Academy by Oxford University Press, 2005), 29–60.

[31] Kohler, *From Medical Chemistry to Biochemistry*; Richard Burian, Jean Gayon, and Doris Zallen, "The Singular Fate of Genetics in the History of French Biology, 1900–1940," *Journal of the History of Biology*, 21 (1988), 357–402; Harwood, *Styles of Scientific Thought*, chap. 4.

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this hypothesis is the remarkable number of Oxford zoologists between the wars who drew on the findings and methods of both field and laboratory specialties in their work on the evolutionary synthesis (Julian Huxley, E. B. Ford, Gavin de Beer) and animal ecology (Charles Elton).[32]

A good deal of evidence therefore suggests that the kinds of problems that biologists have selected, the methods that they favored, and the kinds of theories that they devised have all been affected by the particular structure and ideology of the institutions in which they worked.

## CONCLUSION

Although the development of the life sciences has evidently been affected by the peculiarities of academic settings, our understanding of these relationships is still hampered by substantial gaps in the literature. And this makes it more difficult to address some of the major historiographical issues in this field. For example, it is well known that from the late nineteenth century to the Second World War, the overall "shape" of the life sciences changed significantly as the laboratory grew in importance and experiment became the dominant form of investigation. The key question is why this transformation occurred. Although it is sometimes suggested (or more often simply assumed) that this shift is attributable to the epistemological superiority of experiment, the point has never been seriously argued. From the foregoing, it should be clear why the nature of patronage is a more likely explanation, but in order to establish this, we need to know more about the essentially "political" processes within universities that have tended to marginalize field- and museum-based specialties such as systematics, paleontology, or ecology (albeit with important variations between countries as well as between universities in the same country).[33]

In order to get at these processes (as Frederick Churchill pointed out long ago), we need to pay more attention to institutional history. But even the most basic work of this kind – longitudinal studies of the development of particular disciplines at particular universities (an ideal dissertation topic, one would have thought) – is remarkably rare. The literature on ecology, for example, devotes relatively little attention to institutional history and none at all to the institutional relations between ecology and laboratory fields in the twentieth century. And in the literature on the evolutionary synthesis,

[32] Jack Morrell, *Science at Oxford, 1914–1939: Transforming an Arts University* (Oxford: Oxford University Press, 1997), pp. 54–65 and chap. 7. The quotation is on p. 62.

[33] Although Jan Sapp's important study of the disciplinary politics of the new Mendelism did not specifically address the lab–field divide, its focus on the competition among biological specialties for scarce resources was nevertheless a step in the right direction, and it is unfortunate that it seems not to have prompted further work of this kind. See Jan Sapp, "The Struggle for Authority in the Field of Heredity, 1900–1932," *Journal of the History of Biology*, 16 (1983), 311–42.

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far more attention has been paid to the intellectual relations between lab and field specialties – in particular their mutual ignorance and incomprehension – than to their institutional relations.[34]

Finally, understanding the rise of laboratory biology is made more difficult by the fact that the literature has focused so heavily on American developments (reflecting the numerical strength of historians of biology in the United States). This lopsidedness is unfortunate because the way in which this transformation took place in the United States was quite different from European developments at the time. Already by the First World War, for example, fields such as experimental embryology, biochemistry, and genetics had made greater institutional gains in the United States, and in other specialties where both laboratory and field approaches were being used during the 1930s and 1940s, there are signs of an American preference for the former.[35] Thus, if we are to get at the causes of this transformation, comparative analysis will be essential. And that will require a good deal more work on other countries.

[34] For an exception, see Keith Vernon, "Desperately Seeking Status: Evolutionary Systematics and the Taxonomist's Search for Respectability, 1940–1960," *British Journal for the History of Science*, 26 (1993), 207–27. For Frederick Churchill's assessment of the relevant literature, see his "In Search of the New Biology: An Epilogue," *Journal of the History of Biology*, 14 (1981), 177–91. For a recent longitudinal history of the life sciences at one university, see Alison Kraft, "Building Manchester Biology, 1851–1963: National Agendas, Provincial Strategies" (PhD diss., University of Manchester, 2000).

[35] On his visit to the United States in 1907, William Bateson was quite overwhelmed by the scale of enthusiasm for his work. See Beatrice Bateson, *William Bateson, FRS, Naturalist* (Cambridge: Cambridge University Press, 1928), pp. 109–12. On the remarkably rapid growth of laboratory specialties in the United States (compared with Germany), see Nyhart, *Biology Takes Form*, pp. 304–5; Kohler, *From Medical Chemistry to Biochemistry*; Harwood, *Styles of Scientific Thought*, chap. 4. On field and laboratory approaches in ethology, see Gregg Mitman and Richard Burkhardt, "Struggling for Identity: The Study of Animal Behavior in America, 1930–1945," in *The Expansion of American Biology*, ed. Keith Benson, Jane Maienschein, and Ronald Rainger (New Brunswick, N.J.: Rutgers University Press, 1991), pp. 164–94.

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# 7

# GEOLOGICAL INDUSTRIES

*Paul Lucier*

The relation between geology and industry remains a significant, challenging, yet overlooked topic within the history of the earth sciences. Anyone surveying the subject confronts the glaring fact that very little has been written on it either by historians or geologists themselves.[1] Industry is nevertheless important to understanding the history of geology if for no other reason than the tremendous amount of research that scientists (and engineers) have done on mineral resources. It would have been difficult to find a prominent nineteenth- or twentieth-century geologist who was unfamiliar with coal, petroleum, iron, copper, silver, or gold, not to mention building stones, water, and salt. Practically every textbook had some description of the origin and occurrence of useful minerals, whether the author was studying them or not. On the surface, economic resources seem to occupy a central place in geology, but explaining industry's influence on the development of the science is another matter entirely.

This chapter addresses the relation between geology and industry from four perspectives: mining schools, government surveys, private surveys, and industrial science. The first two sections discuss institutions that served as intermediaries between science and commerce. The third section addresses the settings and conditions in which geologists worked directly for private enterprise, and the last section treats the emergence of new research fields that industry encouraged. This analytical framework follows a rough chronology, beginning in the late eighteenth century and ending in the mid-twentieth,

[1] William M. Jordan, "Application as Stimulus in Geology: Some Examples from the Early Years of the Geological Society of America," in *Geologists and Ideas: A History of North American Geology*, ed. Ellen T. Drake and William M. Jordan (Boulder, Colo.: Geological Society of America, 1985), pp. 443–52; Peggy Champlin, "Economic Geology," in *Sciences of the Earth: An Encyclopedia of Events, People, and Phenomena*, ed. Gregory A. Good (New York: Garland, 1998), I: 225–6. Frederick Leslie Ransome, "The Present Standing of Applied Geology," *Economic Geology*, 1 (1905), 1–10.

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which itself reveals the increasing influence of industry on geology. Taken together, the sections advance the argument that industry made significant contributions in terms of its impact on social, professional, and institutional organization as well as on scientific theories, methods, and practices. By way of conclusion, the chapter touches on the ways in which geology aided the growth of industry.

## MINING SCHOOLS

Mining schools have been regarded as one of the birthplaces of geology, and some historians of science have considered them de facto institutional expressions of the close relation between mining and geology.[2] The most prominent schools were established in continental Europe, where the state owned the mines and minerals. During the second half of the eighteenth century, such schools as the Royal Hungarian Mining Academy in Schemnitz (1760) and the École des Mines in Paris (1783) were organized to improve methods of extraction and to train administrators to operate mines profitably. The most famous of these schools was the Freiberg Academy in Saxony (1765), where Abraham Gottlob Werner (1749–1817) was professor of mineralogy. Werner developed a practical system for identifying minerals in the field as well as a theory (geognosy) for explaining the temporal deposition and structural order of the earth's major rock units. As the most influential teacher of his time, Werner's numerous students carried his "school of geognosy" across Europe and to North America. Freiberg thus became the key place to learn geology at the end of the eighteenth century.[3]

For the development of nineteenth-century geology, mining schools seem to be of much less importance. The predominant scholarly interpretation treats them as training centers for engineers, not geologists. That might be an accurate generalization of the majority of students, but it is necessary to stress that mining schools continued to educate scientists as well; for example, one can think of Werner's illustrious students Alexander von Humboldt (1769–1859) or Leopold von Buch (1774–1853). Likewise, mining schools remained places of employment for many distinguished scientists, including Léonce

[2] Rachel Laudan, *From Mineralogy to Geology: The Foundations of a Science, 1650–1830* (Chicago: University of Chicago Press, 1987), especially chap. 5; Theodore M. Porter, "The Promotion of Mining and the Advancement of Science: The Chemical Revolution and Mineralogy," *Annals of Science*, 38 (1981), 543–70; Martin Guntau, "The Emergence of Geology as a Scientific Discipline," *History of Science*, 16 (1978), 280–90, especially p. 281.

[3] Alexander M. Ospovat, "Introduction," in *Short Classification and Description of the Various Rocks*, ed. A. G. Werner (New York: Hafner, 1971); Alexander M. Ospovat, "Reflections on A. G. Werner's 'Kurze Klassification,'" in *Toward a History of Geology*, ed. Cecil Schneer (Cambridge, Mass.: MIT Press, 1969), pp. 242–56; Ezio Vaccari and Nicoletta Morello, "Mining and Knowledge of the Earth," in *Sciences of the Earth: An Encyclopedia of Events, People, and Phenomena*, ed. Gregory A. Good (New York: Garland, 1998), II: 589–92; V. A. Eyles, "Abraham Gottlob Werner (1749–1817) and His Position in the History of the Mineralogical and Geological Sciences," *History of Science*, 3 (1964), 102–15.

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Elie de Beaumont (1798–1874) at the École des Mines or Friedrich Mohs (1773–1839), Carl Bernhard von Cotta (1808–1879), and Johann Breithaupt (1791–1873) at Freiberg.

Another place to look for the impact of mining schools on nineteenth-century geology is in the United States. For many aspiring American scientists, including Josiah D. Whitney (1819–1896), Raphael Pumpelly (1837–1923), and Samuel Franklin Emmons (1841–1911), Freiberg was *the* school of choice. Its methods, theories, and practical interests were transferred to the United States by those who studied there in the 1850s and 1860s.[4] In 1864, the Columbia School of Mines was founded in New York City and in many ways was comparable with its European counterparts. Columbia forged close links between science and industry; prominent geologists such as John S. Newberry (1822–1892) taught there, and its students dominated the mining industry, especially in the western United States.[5] Unlike the European schools, Columbia was not a government institution. In fact, all of the American mining schools established in the late nineteenth century were private initiatives. This might have allowed for a different degree of industrial influence on education and research; it certainly put American mining schools in a more precarious financial position. The Harvard School of Mining and Practical Geology, for instance, run by the distinguished scientists Whitney and Pumpelly, failed after only ten years (1865–75) for lack of students and funding.[6] In short, future historical research might investigate the ways in which American mining schools designed their curricula and set their research agendas in response (or perhaps in reaction) to industrial demands.

An example of the difficulty in using mining schools as the vehicle for exploring how industry shaped geology is the British case. Britain did not have a school of mines until 1851, arguably well past the first industrial exploitation of mineral resources. Nor did the British government own or operate mines. Private enterprises discovered and exploited coal and iron, and miners had little to do with geologists, which presents a problem to historians trying to find a role for science in the British Industrial Revolution.[7] As Roy Porter has shown, the apparent paradox can be resolved by considering class dynamics: gentlemanly geologists and enterprising mine owners had almost nothing in common, especially after 1820, when gentlemanly amateurs based in the

[4] According to one observer, about one-fourth of the students at Freiberg were Americans, who contributed roughly half of the academy's revenue. See John A. Church, "Mining Schools in the United States," *North American Review*, 112 (1871), 62–81.

[5] The Columbia School of Mines graduated nearly half of the mining engineers in the United States in the second half of the nineteenth century. See Clark C. Spence, *Mining Engineers and the American West: The Lace-Boot Brigade, 1849–1933* (New Haven, Conn.: Yale University Press, 1970), p. 40.

[6] Peggy Champlin, *Raphael Pumpelly: Gentleman Geologist of the Gilded Age* (Tuscaloosa: University of Alabama Press, 1994).

[7] See Guntau, "Emergence of Geology as a Scientific Discipline," p. 282, or Margaret C. Jacob, *Scientific Culture and the Making of the Industrial West* (Oxford: Oxford University Press, 1997).

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Geological Society of London became the leading force in British geology.[8] Still, the situation might be studied from another angle. The scholarly attention fixed on gentlemen of science might just as well reflect the bias of well-bred historians, who tend to employ a narrow conception of science in which geology is defined as an intellectual endeavor fit for gentlemen, not a utilitarian practice.[9] As a result, the history of British geology (and to an extent the history of geology in general) has become an account of the travels and writings of elite specialists who pursued the theoretical and disdained the practical.[10] It is time to reexamine our genteel preferences.

## GOVERNMENT SURVEYS

As with European mining schools, geological surveys were government institutions. The idea behind their establishment was straightforward: Geologists possessed specialized knowledge that might aid in the location, identification, and evaluation of mineral resources. That governments should support surveys was based on an argument in political economy about the state's role in promoting the general welfare of its people. Surveys proved to be politically acceptable and effective means for encouraging industry and advancing learning simultaneously. They appealed to capitalists, geologists, and the public alike. Commercial interests gained information about mining (locating coal or gold or petroleum), manufacturing (identifying fuel or building materials), agriculture (evaluating soils or mineral fertilizers), and transportation (topographic mapping or reconnaissance of routes for roads, canals, and railways) without having to invest in costly searches. Geologists received government patronage to explore new lands, and the public, it was argued, gained through both an increase in knowledge and a prosperous economy.

Surveys brought science, industry, and government into a close relationship, and it is perhaps not surprising that the first national survey was established in continental Europe. Between 1825 and 1835, Elie de Beaumont

[8] Roy S. Porter, "The Industrial Revolution and the Rise of the Science of Geology," in *Changing Perspectives in the History of Science: Essays in Honour of Joseph Needham*, ed. Mikuláš Teich and Robert Young (London: Heinemann, 1973), pp. 320–43.

[9] James Secord argued this point with respect to Charles Lyell's attempt to make geology a science by making it respectable. See Charles Lyell, *Principles of Geology*, edited with an introduction by James A. Secord (London: Penguin, 1997), p. xvi.

[10] Rachel Laudan referred to this approach as the "received view." See Laudan, *From Mineralogy to Geology*, pp. 224–5. For a similar critique of the "usual overemphasis" on British geology, see Mott T. Greene, *Geology in the Nineteenth Century: Changing Views of a Changing World* (Ithaca, N.Y.: Cornell University Press, 1982), p. 15. In their defense, it must be stressed that studies of British gentlemanly geologists are among the finest examples of the cultural history of science. See, for example, James A. Secord, *Controversy in Victorian Geology: The Cambrian-Silurian Dispute* (Princeton, N.J.: Princeton University Press, 1986); Martin J. S. Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985); Nicolaas A. Rupke, *The Great Chain of History: William Buckland and the English School of Geology (1814–1849)* (Oxford: Clarendon Press, 1983).

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and Ours Pierre Dufrénoy (1792–1857), under the auspices of the Corps des Mines, completed a geological reconnaissance of France. Designed to locate mineral resources, especially coal, the survey was thought necessary to enable France to compete with industrial Britain. There was a certain measure of irony in the French initiative; Britain had inspired the survey, yet in the 1820s the British government gave no such encouragement to geology or industry at home. The work of Beaumont and Dufrénoy did, however, provide the model for how to integrate geology within government, and beginning in 1832 the British government took steps that within a few years would lead to the establishment of the first permanent national survey, the Geological Survey of Great Britain.[11] Similarly, in the early 1830s, American state governments began to experiment with surveys; the federal government, however, did not sponsor a national survey until the second half of the nineteenth century.

The impact of surveys on the development of geology was much greater than that of mining schools in large part because surveys emerged as the principal institution for the support of geology by the second quarter of the nineteenth century. In fact, as scientific institutions, surveys reached their heyday in the nineteenth century and employed a significant number (perhaps the majority) of geologists. As part of a broad trend toward the institutionalization of science within government bureaucracies, surveys functioned as a source of employment and legitimation for geologists and geology, respectively. As part of the social history of science, their establishment has often been regarded as an advancement toward professionalization. Surveys were the training ground (so to speak) for geologists, the place where most received their experience in the field – identifying rocks, fossils, and formations, as well as drawing and mapping these phenomena. In short, surveys were one of the driving engines of nineteenth-century geology.[12]

This important contribution has attracted the attention of a number of historians, who have analyzed why, when, and where surveys were organized and the governments that sponsored them. Once a survey's organization has been discussed, however, scholarly attention wanes. With the exception of James Secord's study of the early years of the Geological Survey of Great Britain, these institutions have not been treated as centers of scientific research. Rather, survey geology has been described as routine, unenlightening, and often reflecting an uncreative "mapping mentality."[13] Here, then, is an opportunity for historical research.

11 Rudwick, *Great Devonian Controversy*, p. 91; James A. Secord, "The Geological Survey of Great Britain as a Research School, 1839–1855," *History of Science*, 24 (1986), 223–75.

12 Secord, "Geological Survey of Great Britain as a Research School," Stephen P. Turner, "The Survey in Nineteenth-Century American Geology: The Evolution of a Form of Patronage," *Minerva*, 25 (1987), 282–330.

13 Secord, "Geological Survey of Great Britain as a Research School." On the mapping mentality, see David R. Oldroyd, *Thinking about the Earth: A History of Ideas in Geology* (London: Athlone, 1996), chap. 5.

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Another can be found in explaining the relation between surveys and industry. The following review of British and American surveys suggests some connections between the two and highlights questions that might be worth investigating. It is important to note beforehand that most surveys have official histories, which, despite their limitations, contain a wealth of untapped information on the goals, practices, and problems facing survey geologists.[14] They also reveal the crucial scientific contribution of surveys beyond any institutional or professional significance, namely systematic exploration – the advancement of geology by investigating new regions. Although this might seem obvious, it warrants emphasis if only because it draws attention to the characteristic feature of nineteenth-century geology – fieldwork. But the next questions, of which regions to map and how to study them, focus our examination on industry's influence on nineteenth-century geology.

The Geological Survey of Great Britain was founded on the promise of its utility. Although some scholars have dismissed the rhetoric as commonplace, it is worthwhile to reconsider the economic content of the survey's work. The extensive publications of survey geologists, as well as the two official histories, reveal that practical concerns were decisive factors in its design and prosecution, especially in the second half of the nineteenth century, if not in the survey's early years. This raises doubts about the persistence of gentlemanly amateur values on the survey. Historians, notably Porter and Secord, have maintained that the preference for theoretical over practical science extended to the survey through the programs and personalities of its directors: Henry De la Beche (director 1832–55), Roderick Murchison (1855–71), Andrew Ramsay (1871–81), Archibald Geikie (1882–1901), and J. J. H. Teall (1901–14).[15] Yet, as is well known, under De la Beche the survey began in the mining districts of Cornwall and then moved to the coalfields of South Wales in the 1840s. Between 1850 and 1855, the survey began work on the coalfields of the Midlands.

The place of coal research within the Survey's work might be one way to uncover the role of mining in British geology. When the Royal Commission on Coal (1866–71) was set up to investigate the subject of resource exhaustion, the survey responded by devoting much of its staff to coalfield surveys, particularly during Ramsay's directorship. In addition, the commission's recommendations affected geological methods. Whereas early survey maps had been done on the scale of one inch to the mile, coalfields required much

[14] For Britain, see Edward Bailey, *Geological Survey of Great Britain* (London: Thomas Murby, 1952); John Smith Flett, *The First Hundred Years of the Geological Survey of Great Britain* (London: His Majesty's Stationery Office, 1937). For the United States, see the four volumes of Mary C. Rabbitt, *Minerals, Lands, and Geology for the Common Defense and General Welfare* (Washington, D.C.: U.S. Government Printing Office, 1979–86).

[15] Secord, "Geological Survey of Great Britain as a Research School"; Roy Porter, "Gentlemen and Geology: The Emergence of a Scientific Career, 1660–1920," *The Historical Journal*, 21 (1978), 809–36.

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more detail, and hence the transformation of survey maps to six inches to the mile.[16]

In the twentieth century, the survey gave top priority to economic resources. Since 1901, when another Royal Commission on Coal (Final Report 1905) considered the state of maps and the extent and structure of coalfields, "all programmes of work for the Geological Survey have given special attention to the survey of our coalfields."[17] Beginning with Teall, Survey research has included chemical analyses of economic minerals. World War I, in particular, gave a great boost to practical studies, such as the *Special Reports on Mineral Resources* (1919), which included three volumes on iron.

Mineral resources were thus central to the survey. De la Beche and his successors concentrated their energies on regions that held the greatest prospect of economic return. Given this and the fact that the most prominent geologists of the time worked for the survey, the question of the relation between British geology and industry seems to warrant further study.

American geological surveys provide the best examples for understanding the role of industry in the development of nineteenth-century geology. Designed to discover, describe, and develop natural resources, the principal reason for their establishment was economic. As in the case of the British survey, American ones were justified through a rhetoric of utility.[18] In practice, American geologists, for the most part, put economic results before theoretical work. This is not to say that theory was absent, but rather Americans were keenly aware of the need to balance the standards of good research with the demands for useful information. In effect, the surveys wedded research to public service, the scientific to the utilitarian. This dynamic shaped much of American geology.[19]

During the early and mid-nineteenth century, individual states, not the federal government, were primarily responsible for surveys. States invested in them as part of internal improvements or, in other words, public works projects. The federal government had neither the political will nor the constitutional right to fund a national survey. The first state to sponsor a survey was North Carolina in 1823. Others, particularly in the North, soon followed.

[16] The survey mapped the coalfields of Lancashire, Yorkshire, Durham, Northumberland, and Cumberland in the 1850s and 1860s, and in the following decade it covered the Scottish coalfields, starting in Midlothian. See Flett, *First Hundred Years of the Geological Survey of Great Britain*, pp. 73–92; Bailey, *Geological Survey of Great Britain*, pp. 75–82.

[17] Flett, *First Hundred Years of the Geological Survey of Great Britain*, p. 144.

[18] On the political economy of government surveys, see, for example, Hugh Richard Slotten, *Patronage, Practice, and the Culture of American Science: Alexander Dallas Bache and the US Coast Survey* (Cambridge: Cambridge University Press, 1994); Howard S. Miller, *Dollars for Research: Science and Its Patrons in Nineteenth-Century America* (Seattle: University of Washington Press, 1970); Walter B. Hendrickson, "Nineteenth-Century State Geological Surveys: Early Government Support of Science," *Isis*, 52 (1961), 357–71.

[19] Edward Hitchcock, director of the first geological survey of Massachusetts (1830–3), was the first state geologist to publish his results and established a precedent by dividing his report into two halves: "Economical" and "Scientific." See Edward Hitchcock, *Final Report on the Geology of Massachusetts*, 2 vols. (Northampton, Mass.: J. H. Butler, 1841).

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By 1850, twenty-one of the thirty states in the union had established one; by 1900, almost forty (of the forty-five states) had funded at least one survey.[20]

Agriculture, transportation, and mining were the economic concerns of the early surveys.[21] Farming was particularly important to southern and mid-western states. In North Carolina (1823), South Carolina (1824), Georgia (1836–40), and Michigan (1837–42), legislators wanted detailed reports on soils, marl deposits, and other mineral fertilizers. In Ohio (1837–9), the director William W. Mather (1804–1859) was charged with exploring for minerals useful to industry, including the "agricultural industries." When certain members of the legislature complained that the survey only benefited iron and coal districts, the appropriation bill failed to pass.[22] Other states demanded information about feasible routes for roads, canals, and railroads. In Maryland (1833–40) and Virginia (1835), geologists worked for the agency overseeing transportation.[23] In Connecticut (1835) and Indiana (1837–8), surveys were tied directly to canal and railroad construction. The New York State Natural History Survey (1836–42), the largest and wealthiest in the antebellum period, was nearly abolished because of problems with funding further canals. Governor William Seward justified expenses with the promise of future returns to mining. Ironically, the geologists were instructed to discover the extent and usability of coal, which by 1839 they had proved was not to be found in New York State; the rocks were too old. With some clever politicking, the survey continued another three years, with instructions to report on other mineral resources.[24]

Although state surveys were temporary, short-term tasks to be accomplished, their impact was nonetheless dramatic. At a time when there were few if any colleges, universities, or mining schools in the United States for education and research in geology or science in general, state surveys became the primary institutional base for the growth of American geology and the

[20] Several states, including Alabama, New Hampshire, and Pennsylvania, established a survey in the antebellum period and one in the Gilded Age. Others, such as Indiana, Kentucky, Missouri, and New Jersey, established three or more during the nineteenth century. The best study of American surveys remains George P. Merrill, *The First One Hundred Years of American Geology* (New Haven, Conn.: Yale University Press, 1924). See also George P. Merrill, *Contributions to a History of American State Geological and Natural History Surveys*, Smithsonian Institution, United States National Museum, Bulletin 109 (Washington, D.C.: U.S. Government Printing Office, 1920).

[21] Michele L. Aldrich, "American State Geological Surveys, 1820–1845"; William M. Jordan, "Geology and the Industrial Revolution in Early to Mid Nineteenth Century Pennsylvania," both in *Two Hundred Years of Geology in America*, ed. Cecil J. Schneer (Hanover, N.H.: University Press of New England, 1979), pp. 91–103, 133–43, respectively; Michele L. Aldrich, *New York State Natural History Survey, 1836–1842: A Chapter in the History of American Science* (Ithaca: Paleontological Research Institution, 2000).

[22] Merrill, *Contributions to a History of American State Geological and Natural History Surveys*, especially pp. 390–7.

[23] Michele L. Aldrich and Alan E. Leviton, "William Barton Rogers and the Virginia Geological Survey, 1835–1842," in *The Geological Sciences in the Antebellum South*, ed. James X. Corgan (Tuscaloosa: University of Alabama Press, 1982), pp. 83–104; R. C. Milici and C. R. B. Hobbs, "William Barton Rogers and the First Geological Survey of Virginia, 1835–1841," *Earth Sciences History*, 6 (1987), 3–13.

[24] Aldrich, *New York State Natural History Survey*.

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precedent for government patronage of science.[25] The scientific results were no less permanent and impressive: reports, vertical sections, and maps covering most of the North American continent east of the Mississippi River. In this regard, the creation of the New York System – the identification, ordering, and naming of the oldest Paleozoic rocks in eastern North America – stands out. It became the standard for future stratigraphic correlations with western parts of the Continent and Europe.[26]

The Pennsylvania survey (1836–42) made an enduring theoretical contribution stemming directly from mining concerns. The director Henry Darwin Rogers (1808–1866) and his assistant J. Peter Lesley (1819–1903) devoted their energy to unraveling the structure of the anthracite basins in the northeast portion of the state and the bituminous coalfields in the western areas around Pittsburgh. Rogers and Lesley showed that the anthracite and bituminous coal had been deposited at the same time. The difference in the two types reflected the amount of heat and pressure to which the deposits had been subjected: Anthracite had undergone more intense conditions. What distinguished anthracite from bituminous coal then was not a function of different organic material or conditions of deposition, as some British geologists had theorized, but rather the result of subsequent alteration. In Rogers's words, the anthracite had been "de-bituminized" during the formation of the Appalachians, which meant the forces that produced the mountains had been greater in the East than in the West. The industrial uses of this theory were obvious; companies searching for anthracite west of the Appalachians would not find it. The scientific usefulness was equally great. The explanation of the origin of anthracite and bituminous coal provided crucial evidence for Rogers's theory of mountain building, which attributed the Appalachians to subterranean forces concentrated in the East and progressively diminishing westward rather than a gradual and continuous uplift of the general area. Coal thus became a key to international debates between catastrophists and uniformitarians over the causes of mountain building.[27]

After the Civil War, the exciting research in American geology moved from the eastern part of the continent to the immense region west of the Mississippi River with a new sponsor, the federal government. In 1867, the U.S. Congress authorized two surveys: the Geological Exploration of the Fortieth

[25] Surveys often supported other sciences, including paleontology, mineralogy, botany, zoology, and agriculture or soil chemistry.

[26] Patsy A. Gerstner, "Henry Darwin Rogers and William Barton Rogers on the Nomenclature of the American Paleozoic Rocks," in Schneer, *Two Hundred Years of Geology in America*, pp. 175–86; Cecil J. Schneer, "Ebenezer Emmons and the Foundations of American Geology," *Isis*, 60 (1969), 437–50.

[27] Henry Darwin Rogers, *The Geology of Pennsylvania*, 2 vols. (Philadelphia: Lippincott, 1858); Paul Lucier, *Scientists and Swindlers: Consulting on Coal and Oil in America, 1820–1890* (Baltimore: Johns Hopkins University Press, 2000); Patsy A. Gerstner, *Henry Darwin Rogers, 1808–1866: American Geologist* (Tuscaloosa: University of Alabama Press, 1994). On anthracite mining and geologists' role, see Anthony F. C. Wallace, *St. Clair: A Nineteenth-Century Coal Town's Experience with a Disaster-Prone Industry* (Ithaca, N.Y.: Cornell University Press, 1988).

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Parallel under Clarence King (1842–1901) and the Geological and Geographical Survey of the Territories under Ferdinand V. Hayden (1829–1887). King's party followed the route of the first transcontinental railroad (completed in 1869), and Hayden's covered Nebraska, Wyoming, and Colorado. In 1871, Congress commissioned two more parties: the Geological and Geographical Survey of the Rocky Mountain Region under John Wesley Powell (1834–1902) and the Geographical Surveys West of the One Hundredth Meridian under Lieutenant George M. Wheeler, Corps of Engineers. Although historians are familiar with the general results of these expeditions, their economic side should not be underestimated. Their object was to collect, sort, and distribute useful information that might guide future development of the region. To this end, King prioritized mining, and his survey's first publication discussed the silver-lead mines of the Comstock Lode in Nevada. In general, however, federal patronage of geology in the 1860s and 1870s remained haphazard and piecemeal.[28]

In an effort to consolidate these diverse projects, Congress created the United States Geological Survey (USGS) in 1879. In one legislative act, geology became a permanent administrative function of the U.S. government. Gathering information on mining resources became a continuous process that would be coeval with developing industry. Clarence King, the first director of the USGS (1879–81), set an agenda that concentrated on economic resources. Dividing the survey into two divisions, Mining Geology and General Geology, King began a program of detailed studies of western mining regions. The goal of the USGS, he thought, was to provide information to industry. Hence most of the USGS funding and personnel were directed toward investigations of gold, silver, and copper, the richest resources in the West.[29]

From its inception to the present day, the USGS has been devoted to the exploration and evaluation of natural resources. Within this broad economic framework, science of the first order was produced – one might think of the research of Grove Carl Gilbert (1843–1918), George F. Becker (1847–1919), S. F. Emmons (1841–1911), and Charles Van Hise (1857–1918), among

[28] James D. Hague (with geological contributions by Clarence King), *Mining Industry*, vol. III: *Report of the U.S. Geological Exploration of the Fortieth Parallel* (Washington, D.C.: U.S. Government Printing Office, 1870); Mary C. Rabbitt, *Minerals, Lands, and Geology for the Common Defense and General Welfare*, vol. 1: *Before 1879* (Washington, D.C.: U.S. Government Printing Office, 1979); Thomas G. Manning, *Government in Science: The U.S. Geological Survey, 1867–1894* (Lexington: University of Kentucky Press, 1967); Thurman Wilkins, *Clarence King: A Biography* (New York: Macmillan, 1958); James G. Cassidy, *Ferdinand V. Hayden: Entrepreneur of Science* (Lincoln: Univ. of Nebraska Press, 2000); Donald Worster, *A River Running West: The Life of John Wesley Powell* (New York: Oxford Univ Press, 2001).

[29] Besides his role in the USGS, King organized a systematic review of mineral resources for the Tenth Census of the United States. The massive volumes on precious metals, iron ores, and petroleum are rich sources of geological information waiting to be mined. See Mary C. Rabbitt, *Minerals, Lands, and Geology for the Common Defense and General Welfare*, vol. 2: *1879–1904* (Washington, D.C.: U.S. Government Printing Office, 1979).

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others.[30] The ability to pursue both scientific and economic investigations characterized the USGS. Still, it bears repeating that the success of state and federal government surveys as scientific institutions rested on the public's and industry's belief in the usefulness of geology.

## PRIVATE SURVEYS

Although most nineteenth-century geologists held positions with government-sponsored surveys, there was another type of employment with more direct ties to industry, namely private surveys. This commercial practice goes back at least to the late eighteenth century, when mineral surveyors or engineers, as they were sometimes styled, became actively involved in searching for coal, iron, or other resources.

In Britain, mineral surveyors prospered (financially and intellectually) during the second half of the eighteenth century and into the early nineteenth. These practitioners usually received support either from public subscription or wealthy estate owners. They made valuable contributions through their use of new systems for identifying, ordering, and tracing rocks. Such well-known surveyors as John Farey (1766–1826), Robert Bakewell (1768–1843), Arthur Aikin (1773–1854), and John Taylor (1779–1863) extended the exploration and mapping projects.[31] The most famous surveyor was William Smith (1769–1839), whose private surveys, beginning with those in southern England, had far-reaching effects. Hailed as the "father of English geology," Smith was among the first to use characteristic fossils to identify similar groups of rocks across distant geographic regions.[32] He pioneered a method for ordering formations in a structural sequence and produced a geological map of

[30] R. H. Dott, Jr., "The American Countercurrent – Eastward Flow of Geologists and Their Ideas in the Late Nineteenth Century," *Earth Sciences History*, 9 (1990), 158–62; Stephen J. Pyne, *Grove Karl Gilbert: A Great Engine of Research* (Austin: University of Texas Press, 1980); John W. Servos, "The Intellectual Basis of Specialization: Geochemistry in America, 1890–1915," in *Chemistry in Modern Society: Historical Essays in Honor of Aaron J. Ihde*, ed. John Parascandola and James C. Whorton (Washington, D.C.: American Chemical Society, 1983), pp. 1–19.

[31] Hugh S. Torrens, "Patronage and Problems: Banks and the Earth Sciences," in *Sir Joseph Banks: A Global Perspective*, ed. R. E. R. Banks et al. (London: Kew Royal Botanic Gardens, 1994), pp. 49–75; Hugh S. Torrens, "Arthur Aikin's Mineralogical Survey of Shropshire 1796–1816 and the Contemporary Audience for Geological Publications," *British Journal for the History of Science*, 16 (1983), 111–53; Roger Burt, *John Taylor: Mining Entrepreneur and Engineer, 1779–1863* (London: Moorland, 1977).

[32] Several historians have discussed William Smith's work. See, for example, Hugh S. Torrens, "Le 'Nouvel Art de Prospection Minière de William Smith et le 'Projet de Houillère de Breham': Un Essai Malencontreux de Recherche de Charbon dans le Sud-Ouest de l'Angleterre, entre 1803 et 1810," in *Livre Jubilaire pour Francois Ellenberger* (Paris: Société; Schneer, géologique de France, 1988), pp. 101–18; Joan M. Eyles, "William Smith: Some Aspects of His Life and Works," in *Toward a History of Geology*, pp. 142–58. Martin Rudwick has argued for the central role of Alexandre Brongniart (1770–1847) and Georges Cuvier (1769–1832) in the emergence of stratigraphical or geohistorical geology. See Martin Rudwick, "Minerals, Strata and Fossils," in *Cultures of Natural History*, ed. N. Jardine, J. A. Secord, E. C. Spary (Cambridge: Cambridge University Press, 1996), pp. 266–86; Martin Rudwick, "Cuvier and Brongniart, William Smith, and the Reconstruction of Geohistory," *Earth Sciences History*, 15 (1996), 25–36.

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England, Wales, and parts of Scotland on which he traced and colored his stratigraphic units. In all likelihood, there were other mineral surveyors, but their names along with their work have been excluded from histories of geology.[33] It is usually taken for granted that after 1820 the gentlemanly specialists of the Geological Society of London, who pursued "polite ornamental non-industrial geology," prevailed over the practical surveyors.[34] Whether private surveys continued,[35] or how they might have been subsumed within the professional activities of the Geological Survey of Great Britain, would be topics well worth studying.

In the United States, several prominent geologists welcomed the opportunity and the offers to undertake surveys for mining enterprises, especially coal and iron companies. Scientific consulting, as the practice became known, thrived during the middle decades of the nineteenth century (and it continues to the present day). That nineteenth-century Americans were innovators and leading practitioners is important for scholars trying to explain the relations between science and industry.[36] Consulting constituted a precedent in the commercialization of scientific expertise.[37] Americans wrestled with new and knotty problems about industrial influence on research and results. They confronted doubts about their professional ethics and questions about the propriety of private enterprise underwriting science.[38] With regard to social and

[33] Hugh Torrens's work is the exception. He has brought to life a number of eighteenth- and early nineteenth-century mineral prospectors. See, for example, Hugh Torrens, "Joseph Harrison Fryer (1777–1855): Geologist and Mining Engineer, in England 1803–1825 and South America 1826–1828. A Study in Failure," in *Geological Sciences in Latin America: Scientific Relations and Exchanges*, ed. S. Figueirôa and M. Lopes (Campinas: UNICAMP/IG, 1995), pp. 29–46.

[34] Jack Morrell, "Economic and Ornamental Geology: The Geological and Polytechnic Society of the West Riding of Yorkshire, 1837–53," in *Metropolis and Province: Science in British Culture, 1780–1850*, ed. Ian Inkster and Jack Morrell (Philadelphia: University of Pennsylvania Press, 1983), pp. 231–56, at p. 233. Nicolaas Rupke characterized the "English School of Geology" by its low regard for and lack of interest in the economic aspects of geology. See Rupke, *Great Chain of History*, pp. 15–18. See also Porter, "Gentlemen and Geology"; Roy S. Porter, *The Making of Geology: Earth Science in Britain, 1660–1815* (Cambridge: Cambridge University Press, 1977); Jean G. O'Connor and A. J. Meadows, "Specialization and Professionalization in British Geology," *Social Studies of Science*, 6 (1976), 77–89. Rachel Laudan argued that the gentlemen of the Geological Society, in contrast to the practical men, hindered the development of geology in the early years of the nineteenth century. See Rachel Laudan, "Ideas and Organizations in British Geology: A Case Study in Institutional History," *Isis*, 68 (1977), 527–38.

[35] Jack Morrell, *John Phillips and the Business of Victorian Science* (Aldershot: Ashgate, 2005).

[36] Paul Lucier, "Commercial Interests and Scientific Disinterestedness: Consulting Geologists in Antebellum America," *Isis*, 86 (1995), 245–67.

[37] For recent work on consulting chemists, see, for example, Colin A. Russell, *Edward Frankland: Chemistry, Controversy, and Conspiracy in Victorian England* (Cambridge: Cambridge University Press, 1996); Katherine D. Watson, "The Chemist as Expert: The Consulting Career of Sir William Ramsay," *Ambix*, 42 (1995), 143–59.

[38] Lucier, *Scientists and Swindlers*; Gerald White, *Scientists in Conflict: The Beginnings of the Oil Industry in California* (San Marino, Calif.: Huntington Library, 1968). Perceived and actual abuses of scientific consulting sparked a backlash against commercialization, the "pure" science ideal of the late nineteenth century. See Owen Hannaway, "The German Model of Chemical Education in America, Ira Remsen at Johns Hopkins (1876–1913)," *Ambix*, 23 (1976), 145–64; George H. Daniels, "The Pure Science Ideal and Democratic Culture," *Science*, 15 (1967), 1699–1705; Henry Rowland, "Plea for Pure Science," *Science*, 29 (1883), 242–50.

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institutional circumstances, the United States might have been exceptional; it had neither a class of gentlemanly amateurs of independent means as in Britain nor government mining academies as in continental Europe. What government support existed – the state surveys – was temporary. Furthermore, Americans exhibited a distinct cultural acceptance of practical science. They certainly appeared more amenable to engaging directly with industry than their European colleagues; however, as noted, more research needs to be done on mineral surveyors and consultants in other countries as well as in the United States.[39]

## INDUSTRIAL SCIENCE

Historians and scientists would agree that industry aided the development of geology at its most basic level – exploration. In excavating the earth, industry literally revealed once-hidden rocks, fossils, and formations to geologists' inspection. Mines, quarries, wells, roadworks, and canal cuts became vital incentives to inquiry, *places to do geology*; that is, if companies allowed geologists to investigate such exposures. Industry occasionally turned up something interesting and unsuspected that might lead to new research or perhaps new scientific specialties. Petroleum geology and economic geology are two examples of this type of industrial stimulus.

The discovery of oil in western Pennsylvania in 1859 literally fueled a new industry as well as scientific questions about the origin and occurrence of petroleum. In the early 1860s, geologists (many of whom were consultants) thought the best guides to exploration were surface indications, namely oil springs. As industry spread, geologists soon realized that springs did *not* necessarily correlate with subsurface pools. In fact, some of the most prolific wells were in areas without any surface indications. Accordingly, geologists reinterpreted the presence of oil springs to mean that the oil had escaped. As a liquid, petroleum is unique among mineral resources: It migrates vertically through the strata to the surface as well as horizontally through a formation, which makes it difficult to find. The formation in which it is trapped might not be the same as its source, and, conversely, even if conditions seem right for the creation of oil, subsurface conditions might not be suitable for its accumulation. Understanding the factors controlling reservoirs became crucial for industry and science.[40]

[39] On consulting in Britain, see Geoffrey Tweedale, "Geology and Industrial Consultancy: Sir William Boyd Dawkins (1837–1929) and the Kent Coalfield," *British Journal for the History of Science*, 24 (1991), 435–51.

[40] The best general history of the U.S. oil industry remains Harold F. Williamson and Arnold R. Daum, *The American Petroleum Industry, 1859–1899: The Age of Illumination* (Evanston, Ill.: Northwestern University Press, 1959). For petroleum geology, see Edgar Wesley Owen, *Trek of the Oil Finders: A History of Exploration for Petroleum* (Tulsa, Okla.: American Association of Petroleum Geologists, 1975); and Lucier, *Scientists and Swindlers.*

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Throughout the 1870s and 1880s, American geologists (most of whom were now working on geological surveys in Pennsylvania, Ohio, West Virginia, and Canada) introduced theories about the structure of oil reservoirs and the dynamics of subsurface fluid flow. In broad outline, they established three principles for finding oil: (1) source (decomposition of animal or vegetable material), (2) porous and permeable reservoir rock (usually sandstone or limestone), and (3) impervious cap or cover rock (such as shale). The predominant structures controlling accumulation were thought to be anticlines: Oil migrated to their crests.[41] By the last decades of the nineteenth century, geologists had formulated a theoretical and practical science of petroleum, one of the chief intellectual contributions of nineteenth-century Americans.

At least the American part of the history of economic geology is similar. Gold rushes, silver booms, and copper strikes in the trans-Mississippi West stimulated scientific investigation of these minerals. Under the auspices of the USGS, economic geology took shape in the 1880s and 1890s during studies of the principal mining districts – the Comstock Lode, Nevada; Eureka, Nevada; and Leadville, Colorado.[42] These surveys set a model for research involving detailed mapping (surface and subsurface), microscopic petrography, and chemical analysis. They also established the meteoric theory as the predominant explanation of ore genesis. According to this theory, ores formed when surface waters, *descending* through the rock, were heated and enriched with metallic ions, which were then deposited and concentrated in fissures in the host rock. This theory would be challenged in the early twentieth century by other geologists (most of whom were working for the USGS in other mining districts), who supported the magmatic theory, in which ores formed as a result of enriched waters *ascending* from a magmatic intrusion. In either explanation, geologists had come to a consensus on some principles (very much like those in petroleum geology): (1) source (host rock or magmatic intrusion), (2) medium of transport (water, either descending or ascending), and (3) deposition (veins). They also agreed that detailed studies of mining districts were the bedrock of economic geology.[43]

[41] The best evidence for this came from the second and third Ohio surveys (1869–85 and 1889–93) under the direction of Edward Orton (1829–1899), who is often given credit for establishing the anticlinal theory, despite stubborn opposition from J. Peter Lesley and the second Pennsylvania survey (1874–88). See Keith L. Miller, "Edward Orton: Pioneer in Petroleum Geology," *Earth Sciences History*, 12 (1993), 54–9; Stephen F. Peckham, *Report on the Production, Technology, and Uses of Petroleum and Its Products*, vol. 10: *U.S. Tenth Census*, U.S. Congress 2nd Session, H. R. Misc. Doc. 42 (Washington, D.C.: U.S. Government Printing Office, 1884).

[42] G. F. Becker, *Geology of the Comstock Lode and Washoe District: U.S. Geological Survey Monograph 3* (Washington, D.C.: U.S. Government Printing Office, 1882); S. F. Emmons, *Geology and Mining Industry of Leadville, Colorado: U.S. Geological Survey Monograph 12* (Washington, D.C.: U.S. Government Printing Office, 1886); Arnold Hague, *Geology of the Eureka District, Nevada: U.S. Geological Survey Monograph 20* (Washington, D.C.: U.S. Government Printing Office, 1892).

[43] S. F. Emmons, "Theories of Ore Deposition, Historically Considered," *Bulletin of the Geological Society of America*, 15 (1904), 1–28; L. C. Graton, "Ore Deposits," in *Geology, 1888–1938: Fiftieth Anniversary Volume* (New York: Geological Society of America, 1941), pp. 471–509.

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In the twentieth century, petroleum geology, economic geology, and many other subdisciplines would be reorganized and occasionally redefined with the incorporation of geology within industry. Before 1900, geologists (and scientists in general) had shied away from industry jobs and the prospect of becoming *dependent* employees. They preferred to be *independent* experts, hence the part-time and limited character of scientific consulting as well as the emergence of such specialties as petroleum geology and economic geology within surveys, institutions with *indirect* connections to industry. The employment of geologists by industry and the impact this has had on scientific theories, methods, and practices is arguably the critical change in twentieth-century geology and one that is badly in need of historical analysis.

In the petroleum industry, geologists first became employees during the 1890s in California. Production companies turned to graduates of Stanford and Berkeley to find oil as part of a broad strategy for challenging the monopoly of John D. Rockefeller's Standard Oil.[44] Other companies, mostly American (such as Texaco and Gulf Oil) but including one British firm, Mexican Eagle Oil (El Aguila), began sending geologists to explore parts of Oklahoma, Texas, and Mexico. Exploration was their job, and the oil industry quickly became the largest employer of geologists. By the 1950s, oil companies operated the most extensive and expensive earth science research laboratories in the world.

Mining companies began to hire geologists just at the turn of the century. The Anaconda Copper Mining Company of Butte, Montana, was the first in the United States to establish a geological department. Other big firms, such as International Nickel, followed the "Anaconda school" in establishing laboratories for geological research as well as metallurgical studies. In the 1920s, powerful mining organizations started to set up subsidiaries, for instance the Guggenheim Exploration Company, for the continuous and aggressive exploration of new properties, especially in Africa. By World War II, most large mining companies had geological departments.[45]

As industry increasingly relied on geology, the scientists themselves sought professional recognition.[46] As early as 1917, a small group organized the Southwestern Association of Petroleum Geologists in Tulsa, Oklahoma. The following year, they changed the name to the American Association of Petroleum Geologists (AAPG). The timing reflected the centrality of

[44] Frank J. Taylor, *Black Bonanza: How an Oil Hunt Grew into the Union Oil Company of California* (New York: McGraw-Hill, 1950); Gerald T. White, *Formative Years in the Far West: A History of Standard Oil Company of California and Predecessors through 1919* (New York: Appleton-Century-Crofts, 1962).

[45] L. C. Graton, "Seventy-Five Years of Progress in Mining Geology," in *Seventy-Five Years of Progress in the Mining Industry, 1871–1946*, ed. A. B. Parsons (New York: American Institute of Mining and Metallurgical Engineers), pp. 1–39.

[46] Michael Aaron Dennis referred to this as the occupational style of petroleum geologists. See Michael Aaron Dennis, "Drilling for Dollars: The Making of US Petroleum Reserve Estimates, 1921–25," *Social Studies of Science*, 15 (1985), 241–65.

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petroleum to the U.S. economy (gasoline for internal combustion engines had by then become the principal product, thereby surpassing the illuminant kerosene) as well as petroleum's strategic value to the military. By 1920, petroleum geology was the fastest-growing subject within the earth sciences, and the AAPG became the world's largest geological society.[47] A similar pattern emerged with mining geologists. They organized the Society of Economic Geologists in 1920, and by 1940 economic geology had become the largest division of the Geological Society of America (the AAPG is not an affiliate of the GSA).[48] In short, industry has had a dramatic impact on the social and professional organization of twentieth-century American geology.

Its influence has extended far beyond the mere provision of employment and professional identity. Industry has also shaped the content of the earth sciences. As companies have sought to develop or exploit new techniques and theories to aid in finding mineral resources, they have promoted scientific innovation. The oil industry provides several good examples. Industry has encouraged not only petroleum geology but such new specialties as economic paleontology, microlithology, exploration geophysics, and sedimentology. (Mining companies have also relied on geophysical techniques, especially magnetometers.) Each new subdiscipline has in turn developed its own knowledge base, practice, and professional identity. The proliferation of these industrial sciences accounts for much of the branching and growth of the earth sciences in the twentieth century.[49]

To put it another way, the strategy and structure of twentieth-century geological industries have, in large degree, determined the nature of the earth sciences that served them. Companies have recruited experts and expertise that make exploration less expensive and more comprehensive; scientists in turn received financial rewards and institutional support. This is not to say that industry dictated the direction of twentieth-century earth sciences. New specialties have tried to maintain their autonomy. But as the largest and richest employer of earth scientists, industry has had significant sway over theories, methods, and practices, along with social, professional, and institutional organizations. *How* significantly is the pressing question.

## GEOLOGY AND INDUSTRY

If the role of industry in the development of geology has been neglected by historians, the influence of geology on industry has likewise been dismissed

[47] By 1960, the membership had grown to slightly more than 15,000. See Owen, *Trek of the Oil Finders*, p. 1570.

[48] Graton, "Ore Deposits."

[49] William B. Heroy, "Petroleum Geology," in *Geology, 1888–1938*, pp. 511–48; Donald C. Barton, "Exploratory Geophysics," in *Geology, 1888–1938*, pp. 549–78; John Law, "Fragmentation and Investment in Sedimentology," *Social Studies of Science*, 10 (1980), 1–22.

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by business historians, economists, and students of the mining industries. In accounts of gold rushes and oil booms, geologists play such minor parts as to be invisible.[50] Although, generally speaking, it is accurate to say that the rich and famous strikes of the nineteenth and early twentieth centuries were not made by scientists, some consideration of geology is required when discussing subsequent operations. Geologists often participated in further exploration, extensions of mines, and especially in litigation over ownership of property and mineral rights.[51] Likewise, governments often established surveys in *response* to wasteful exploitation of resources caused by chaotic rushes and booms.[52]

Historians can find plenty of evidence of the relations of nineteenth-century geology and industry in the biographies and autobiographies of geologists as well as in government surveys and consulting reports. Geologists apparently worked well with mineral prospectors, mine superintendents, and other industry managers. In a few instances, they even helped locate mineral resources![53] The point is that other examples can surely be found, but historians have not been looking for them. Too often, the interactions between geology and industry have been discounted because they were temporary, practical, or commercial. This was precisely the design; nineteenth-century mining did not require continuous scientific exploration.[54] Relations were more subtle and complex, not least because they were often mediated by government. To assert a division between theoretical and practical geology is to create a dichotomy that did not exist.

For the twentieth century, the impact of geology on industry seems self-evident. The establishment of research laboratories at multinational oil corporations and mining companies speaks to the relevance and value of the earth

[50] Harold Williamson and Arnold Daum provided a typical example: Geologists were "useless" to early petroleum companies because they could not agree on "basic geological principles" such as the "validity" of Darwin's theory of evolution by natural selection. See Williamson and Daum, *American Petroleum Industry*, p. 90.

[51] Geologists often served as expert witnesses in apex litigation in the western mining regions of the United States. According to U.S. federal law, the discoverer of a mineral vein had the right to exploit it from its top (apex) downward to any depth. The difficulty, of course, came in deciding where one vein ended, or branched, and the next began. See Spence, *Mining Engineers and the American West*, pp. 195–230.

[52] The second Pennsylvania Geological Survey was established because of the glut of oil in the early 1870s. See J. Peter Lesley, "Pennsylvania," in Merrill, *Contributions to a History of American State Geological and Natural History Surveys*, p. 436. On the Geological Survey of Great Britain's response to gold rushes in Australia and other colonies, see Robert A. Stafford, *Scientist of Empire: Sir Roderick Murchison, Scientific Exploration and Victorian Imperialism* (Cambridge: Cambridge University Press, 1989).

[53] T. A. Rickard, doyen of nineteenth-century American mining engineers, thought the USGS study of Leadville, Colorado, was "epoch-making." See T. A. Rickard, *A History of American Mining* (New York: McGraw-Hill, 1932), pp. 132, 140–1. On scientific consultants, see Lucier, *Scientists and Swindlers*.

[54] Mining companies increasingly relied on continuous *technical* expertise from engineers for efficient exploitation of proved discoveries. See Kathleen H. Ochs, "The Rise of American Mining Engineers: A Case Study of the Colorado School of Mines," *Technology and Culture*, 33 (1992), 278–301.

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sciences to the discovery, description, and evaluation of mineral resources. Geology has become a permanent part of industry. It is therefore somewhat odd and disconcerting that historians have not asked how the industrial institutionalization of geology has affected the science. In the future, it can only be hoped that the geological industries will receive the careful study that they surely deserve.

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# 8

# THE PHARMACEUTICAL INDUSTRIES

*John P. Swann*

Despite its importance and impact on our daily lives, the pharmaceutical industry has not attracted nearly as much attention as many other areas in the history of science and medicine.[1] It is not entirely clear why this is the case, though it is not for lack of reminders in the popular press.[2] The elusiveness of primary documentation on the pharmaceutical industry may help explain the lag in scholarly historical inquiries. But whatever the reason, more scrutiny is merited. Pharmaceuticals is one of the most research-intensive industries, it is an entity that usurped a central function of the pharmacist by the late nineteenth century, and it arguably can (and does) label itself the primary broker in the chemotherapeutic revolution of the twentieth century. It has been as consistently profitable throughout the twentieth century as any corner of the private sector; the global market for pharmaceuticals by the mid-1990s was estimated by one source to be $200 billion (U.S.) annually. By 2000, that figure had climbed to $317 billion, with North America accounting for about half that amount.[3] Pharmaceuticals is also an enterprise that can

[1] Many firms have produced corporate histories, but these often have the usual problems of this genre; see, for example, Gregory J. Higby and Elaine C. Stroud, eds., *The History of Pharmacy: A Selected Annotated Bibliography* (New York: Garland, 1995), pp. 43–54. Although the volume of studies on the pharmacuetical industry per se pales compared with, say, Darwiniana or the study of scientific disciplines, there appears to be increasing interest by historians. See, for example, James H. Madison, *Eli Lilly: A Life, 1885–1977* (Indianapolis: Indiana Historical Society, 1989); Geoffrey Tweedale, *At the Sign of the Plough: 275 Years of Allen & Hanburys and the British Pharmaceutical Industry* (London: John Murray, 1990); Ralph Landau, Basil Achilladelis, and Alexander Scriabine, eds., *Pharmaceutical Innovation: Revolutionizing Human Health* (Philadelphia: Chemical Heritage Press, 1999), an otherwise uneven book that has a useful and lengthy introductory chapter by Achilladelis; and Jordan Goodman and Vivien Walsh, *The Story of Taxol: Nature and Politics in the Pursuit of an Anti-cancer Drug* (Cambridge: Cambridge University Press, 2001), which addresses some core issues on pharmaceutical industrialization. Several other examples could be cited.

[2] For example, Donald Drake and Marian Uhlman, *Making Medicine, Making Money* (Kansas City, Mo.: Universal Press Syndicate, 1993), based on their series on the pharmaceutical industry in the *Philadelphia Inquirer*.

[3] P. J. Brown, "The Development of an International Business Information Service for the Pharmaceutical Industry," *Pharmaceutical Historian*, 24 (March 1994), 3; IMS Health, "The Global



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produce drugs like thalidomide, a medicine emblematic of therapeutics gone wrong – and drug regulation simply gone. In the legislatures of the world's leading producers of pharmaceuticals, the drug industry and its trade groups wield considerable influence. Therefore, the lag in historical attention to this industry cannot be for lack of impact by the subject.

The modern pharmaceutical industry began humbly; ironically, the industry evolved principally from the pharmacy itself. Antoine Baumé (1728–1804) of France was the first to begin large-scale production out of his pharmacy laboratory. The techniques he developed and applied in his laboratory – scaled up, of course – were the basis of Baumé's industrial practice. By 1775, his manufacturing operation was producing about 2,400 products, mostly botanicals but also many chemical preparations.[4] Thereafter, the births of European pharmaceutical concerns from retail pharmacies multiplied steadily into the nineteenth century. In England, Allen and Hanbury's derived from a partnership between pharmacists William Allen (1770–1843) and Luke Howard (1772–1864) in the famous Plough Court pharmacy; the two began to manufacture chemical preparations in 1797.[5] German pharmacist Johannes Trommsdorff (1770–1837), who had propagated practical and scientific pharmacy since the 1790s as an educator and editor, started a chemical preparations factory in 1813.[6]

## INFLUENCE FROM ALKALOIDS AND THE DYESTUFF INDUSTRY

The discovery of the alkaloids, beginning with Friedrich Wilhelm Sertürner's (1783–1841) isolation and discovery of morphine as the hypnotic principle of opium in 1805, was among the most significant therapeutic advances of the early nineteenth century.[7] This stimulated a search for active principles in other medicinal plants, and eventually this would contribute to the development of the pharmaceutical industry. Alkaloids were powerful, often

Pharmaceutical Market in 2000 – North America Sets the Pace," March 15, 2001, at http://www.imsglobal.com/insight/news_story/0103/news_story_010314.htm (accessed December 30, 2002).

4 George Urdang, "Retail Pharmacy as the Nucleus of the Pharmaceutical Industry," *Supplements to the Bulletin of the History of Medicine*, no. 3 (1944), 325–46, see 328–30; Glenn Sonnedecker, "The Rise of Drug Manufacture in America," *Emory University Quarterly*, 21 (1965), 75–6.

5 Ernest Charles Cripps, *Plough Court: The Story of a Notable Pharmacy, 1715–1927* (London: Allen and Hanbury's, 1927); Tweedale, *At the Sign of the Plough;* Urdang, "Retail Pharmacy as the Nucleus of the Pharmaceutical Industry," pp. 334–6.

6 Urdang, "Retail Pharmacy as the Nucleus of the Pharmaceutical Industry," p. 330, and Sonnedecker, "Rise of Drug Manufacture in America," p. 76.

7 John E. Lesch, "Conceptual Change in an Empirical Science: The Discovery of the First Alkaloids," *Historical Studies in the Physical Sciences*, 11 (1981), 305–28; Eberhard Schmauderer, "Sertürner, Friedrich Wilhelm Adam Ferdinand," *Dictionary of Scientific Biography*, , 320–1; Georg Lockemann, "Friedrich Wilhelm Serturner, the Discoverer of Morphine," trans. Ralph E. Oesper, *Journal of Chemical Education*, 28 (1951), 305–28; Franz Kromeke, *Friedrich Wilh. Serturner, der Entdecker des Morphiums* (Jena: Gustav Fischer, 1925).

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poisonous, and not easily isolable. As the active ingredients of medicinal plants, alkaloids revolutionized plant drug posology because drugs of known strength could be administered to the patient (similar plants could vary significantly in the proportion of alkaloid). French pharmacists Pierre-Joseph Pelletier (1788–1842) and Joseph-Bienaimé Caventou (1795–1877) probably were the most productive alkaloid workers. The pair discovered several active plant principles, including strychnine (1818), quinine (1820), and caffeine (codiscoverers, 1821). Pelletier went on to establish a firm to produce some of these products.[8] Many other firms that sprang from pharmacies in the early nineteenth century began manufacturing primarily to produce alkaloids. By the late 1820s, two German pharmacists moved in this direction, H. E. Merck (1794–1855) in Darmstadt and Johann Riedel (1786–1843) in Berlin (both of whom later had more success in the production of chemical preparations). Seven years later, English pharmacist John May (1809–1893) started what eventually became the May & Baker industrial concern.[9]

The rise of the synthetic dye industry in the nineteenth century also figured prominently in the growth of pharmaceutical manufacturing. In the early and mid-nineteenth century, August Wilhelm von Hofmann (1818–1892), Friedlieb Ferdinand Runge (1794–1867), and others initiated chemical studies of coal tar – the abundant by-product of coke and coal gas – which yielded a wide range of useful products, including napthalene, aniline, and benzene. Hoffman's assistant at the Royal College of Chemistry in London, William Henry Perkin (1838–1907), in 1856 prepared a synthetic aniline dye, mauve, which launched a flurry of activity to produce other dyes from coal tar in England, France, Germany, and Switzerland. Fueled by the coal tar frenzy, Germany (and, to a lesser extent, Switzerland) soon overshadowed England and France in the production of dyestuffs and other chemicals. This was in no small part due to the character and level at which chemical research was institutionalized in these countries, evidenced, for example, by Liebig's laboratory. Many academic centers became closely involved with industrial enterprises.[10]

Several pharmaceutical firms emerged from dyestuff interests in the late nineteenth century, and a number of commercially significant drugs came out

[8] Alex Berman, "Caventou, Joseph-Bienaimé," *Dictionary of Scientific Biography*, III, 159–60; Alex Berman, "Pelletier, Pierre-Joseph," *Dictionary of Scientific Biography*, X, 497–9; Marcel Delépine, "Joseph Pelletier and Joseph Caventou," trans. Ralph E. Oesper, *Journal of Chemical Education*, 28 (1951), 454–61; *Revue du paludisme et de medicine tropicale*, Numero special a la memoire de Pelletier et de Caventou, 1951.

[9] Urdang, "Retail Pharmacy as the Nucleus of the Pharmaceutical Industry," pp. 331–3, 337; Tom Mahoney, *The Merchants of Life: An Account of the American Pharmaceutical Industry* (New York: Harper and Brothers, 1959), p. 193.

[10] Fred Aftalion, *A History of the International Chemical Industry*, trans. Otto Theodor Benfy (Philadelphia: University of Pennsylvania Press, 1991), pp. 32–48; Aaron J. Ihde, *The Development of Modern Chemistry* (New York: Harper and Row, 1964), pp. 454ff.; John J. Beer, "Coal Tar Dye Manufacture and the Origins of the Modern Industrial Research Laboratory," *Isis*, 49 (1958), 123–31.

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of that tradition, in which chemical synthesis formed the basis for new product development. Farbwerke Hoechst emerged in 1863 outside of Frankfurt to manufacture aniline dyes, and in 1884 this firm introduced the first of several synthetic febrifuges later shown to be analgesics, Antipyrine (phenazone). In 1896, Hoechst marketed a similar drug, Pyramidon (admidopyrine). Ten years later, the firm introduced the enduring local anesthetic Novocaine (procaine).[11] Bayer was founded in the same year as Hoechst in Barmen, Germany. Like Hoechst, Bayer expanded its dyeworks into the manufacture of synthetic pharmaceuticals later in the 1880s with another antifever, painkilling agent, Phenacetin (acetophenetidin, 1888). Although not a by-product of the dye industry, Bayer's biggest antipyretic/analgesic, Aspirin (acetylsalicylic acid), which came on the market in 1897, was evidence of its prudent investment in in-house pharmaceutical research.[12] Another German chemical firm, Boehringer Ingelheim, founded in 1885, did not turn to pharmaceuticals until shortly after the turn of the century and initially focused on alkaloids rather than synthetics.[13]

Several Swiss pharmaceutical firms, all based in Basel, shared a similar origin. Ciba's roots can be traced back to a dyeworks of 1838, though it did not enter the pharmaceuticals market until the late 1880s. One of its first successful drugs was Vioform (iodochlorhydroxyquinoline), an antiseptic agent introduced in 1900.[14] The firm with which Ciba is currently linked, Geigy, began as a trading company under founder Johann Rudolf Geigy (1733–1793) in the eighteenth century. By the 1850s, the firm was entrenched as a dyeworks. Geigy's interests in drugs lagged much longer than for similar firms. Shortly after the turn of the twentieth century, some in the company wanted to move Geigy more toward medicines, but the firm did not create a unit dedicated to drug development until 1938.[15] Sandoz emerged as a dye manufacturer in 1885, and though it produced some antifebrile analgesics beginning in the 1890s, it did not move to pharmaceuticals in earnest until World War I. In 1917, Sandoz created a department dedicated to pharmaceutical research,

[11] Aftalion, *History of the International Chemical Industry*, pp. 41, 49; Gary L. Nelson, ed., *Pharmaceutical Company Histories*, vol. 1 (Bismarck, N.D.: Woodbine, 1983), pp. 39–40. See also Ernst Bäumler, *Farben, Formeln, Forscher: Hoechst und die Geschichte der industriellen Chemie in Deutschland* (Munich: Piper, 1989); A. E. Schreier, *Chronik der Hoechst Aktiengesellschaft, 1863–1988* (Frankfurt am Main: Hoechst, 1990).

[12] Patrice Boussel et al., *History of Pharmacy and Pharmaceutical Industry* (Paris: Asklepios Press, ca. 1982), pp. 217–20. See also Erik Verg et al., *Milestones: The Bayer Story, 1863–1988* (Leverkusen: Bayer, 1988); Charles C. Mann and Mark L. Plummer, *The Aspirin Wars: Money, Medicine, and 100 Years of Rampant Competition* (New York: Random House, 1991).

[13] Boussel et al., *History of Pharmacy and Pharmaceutical Industry*, pp. 223–5.

[14] Renate A. Riedl, "A Brief History of the Pharmaceutical Industry in Basel," in *Pill Peddlers: Essays on the History of the Pharmaceutical Industry*, ed. Jonathan Liebenau, Gregory J. Higby, and Elaine C. Stroud (Madison, Wis.: American Institute of the History of Pharmacy, 1990), pp. 66–8. See also Ciba, *The Story of the Chemical Industry in Basel* (Olten: Urs Graf, 1959).

[15] Riedl, "Brief History of the Pharmaceutical Industry in Basel," pp. 63–4. See also Alfred Bürgin, *Geshichte des Geigy Unternehmens von 1758 bis 1939* (Basel: Geigy, 1958).

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which focused on active ingredients in naturally occurring substances, such as ergot.[16]

## IMPACT OF BIOLOGICAL MEDICINES

In addition to the discovery of alkaloids and the growth of the chemical industry, the therapeutic application of advances in bacteriology and immunology in the late nineteenth century also stimulated the pharmaceutical industry. In 1890, Emil von Behring (1854–1917) and Shibasaburo Kitasato (1852–1931) discovered an effective antitoxin for diphtheria in the blood serum of animals injected with diphtheria toxin. Émile Roux (1853–1933) considerably extended these results at the Pasteur Institute. In 1894, he found that the horse produced a higher titer of diphtheria antitoxin than other animals, and his report on laboratory and clinical investigations using serum therapy clearly established the therapeutic value of the antitoxin.[17]

Roux's results stimulated widespread interest in the manufacture of diphtheria antitoxin among public health and commercial organizations. Burroughs, Wellcome and Co. in Britain and H. K. Mulford Co. in the United States were among those firms that changed significantly as a result of this medical breakthrough. Established in 1880, Burroughs Wellcome was known for its "Tabloids," a compressed tablet dosage form for both the standard drugs of the day, such as digitalis and opium, as well as more unusual preparations, such as Forced March, a combination of coca leaf and cola nut that "allays hunger and prolongs the power of endurance."[18] Obviously, not all labeling in this era was deceptive.

Burroughs Wellcome was one of the earliest producers of diphtheria antitoxin in Britain, announcing its readiness to supply the treatment late in 1894. A significant cultural barrier to production ensured that one manufacturing element – bioassay of the antitoxin – took place off the premises. The Cruelty to Animals Act of 1876 required licenses for experiments on animals and as the first commercial enterprise to request a license, Burroughs Wellcome's application was debated for a year and a half until finally accepted in 1901.[19] This action was particularly significant to the growth and reputation of the firm

[16] Riedl, "Brief History of the Pharmaceutical Industry in Basel," pp. 60–1. See also *Sandoz, 1886–1961: 75 Years of Research and Enterprise* (Basel: Sandoz, 1961).

[17] Ramunas A. Kondratas, "Biologics Control Act of 1902," in *The Early Years of Federal Food and Drug Control*, ed. James Harvey Young (Madison, Wis.: American Institute of the History of Pharmacy, 1982), pp. 9–10. See also Hubert A. Lechevalier and Morris Solotorovsky, *Three Centuries of Microbiology* (New York: McGraw-Hill, 1965; New York: Dover, 1974).

[18] E. M. Tansey, "Pills, Profits and Propriety: The Early Pharmaceutical Industry in Britain," *Pharmaceutical Historian*, 25 (December 1995), 4.

[19] E. M. Tansey and Rosemary C. E. Milligan, "The Early History of the Wellcome Research Laboratories, 1894–1914," in Liebenau, Higby, and Stroud, *Pill Peddlers*, pp. 92–5; Tansey, "Pills, Profits, and Propriety," pp. 4–6.

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because it helped lead to the establishment of the Wellcome Physiological Research Laboratories.

Had the American company Mulford been founded in New Jersey, it, too, would have faced difficulties based on antivivisection laws. That state passed an antivivisection law in 1880 that required authorization by the state board of health to conduct animal experiments.[20] But Mulford, like so many pharmaceutical firms in the United States, was established just beyond the reach of the New Jersey law, in Philadelphia.[21] Like Burroughs Wellcome, Mulford quickly adapted Roux's techniques for commercial production.

In 1894, Mulford president Milton Campbell (b. 1862) hired Joseph McFarland (1868–1945), a member of the Philadelphia Board of Health and the Medico-Chirurgical College, to produce diphtheria antitoxin and possibly other biologicals. This move "was the first direct effort on Campbell's part to enact a policy of active product development through laboratory science."[22] McFarland soon acquired the assistance of faculty members of the University of Pennsylvania Veterinary School to produce the drug, and Mulford arranged for the Laboratory of Hygiene at Pennsylvania to test the antitoxin. By 1900, Mulford was producing nearly a dozen different biologicals through these arrangements, including tetanus antitoxin, anti-streptococcus serum, and rabies vaccine.[23] In the United States, where foreign and domestic biologics producers had to be licensed by the federal government from 1903, the number of companies producing antitoxins, serums, and vaccines doubled from about a dozen in 1904 to two dozen four years later. The number of biological products manufactured by licensees also grew rapidly, from less than a dozen in 1904 to nearly 130 by 1921 (though many of these were ineffective).[24]

## POLITICAL AND LEGAL ELEMENTS

Laws and state policies have had a profound effect on the development of the pharmaceutical industry – or lack thereof. For example, nineteenth-century political efforts to strengthen Germany, principally under Otto von Bismarck, facilitated the growth of the pharmaceutical and other industries. In France and Italy, on the other hand, patent laws of 1844 and 1859, respectively,

[20] This law had a major impact on the conduct of research in one major U.S. firm. See John P. Swann, *Academic Scientists and the Pharmaceutical Industry: Cooperative Research in Twentieth-Century America* (Baltimore: Johns Hopkins University Press, 1988), pp. 43–6.

[21] This is not to suggest that the business was founded in Pennsylvania to escape the New Jersey law. In fact, it is probable that Mulford, like the firm that was affected by the law, Merck, was unaware of this statute.

[22] Jonathan Liebenau, *Medical Science and Medical Industry: The Formation of the American Pharmaceutical Industry* (Baltimore: Johns Hopkins University Press, 1987), p. 59.

[23] Liebenau, *Medical Science and Medical Industry*, pp. 58–62.

[24] *Annual Report of the Surgeon-General of the Public Health and Marine-Hospital Service of the United States*, 1904, p. 372; *Annual Report of the Surgeon-General*, 1908, p. 44; Kondratas, "Biologics Control Act of 1902," p. 18.

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prohibited the monopolization of medical products on ethical grounds; firms were entitled to little more than trade names to protect their proprietary interests. Still, they were able to turn to foreign patents to protect their products. In fact, the French pharmaceutical industry, driven largely by its export trade, thrives in the global market today.[25]

Tariff policy, as seen in the case of late imperial Russia, could significantly affect the development of a domestic pharmaceutical industry. Although policies favored domestic production until the late nineteenth century, subsequent tariff treaties contributed to the inability of the indigenous industry to supply some of the more important products, such as synthetic febrifuges and alkaloid preparations. Russian tariffs encouraged the export of raw materials and the import of finished products. Consequently, Western European firms bought from Russia raw commodities such as cinchona bark, salicylic acid, and crude opium, then sold Russia the quinine, modified salicylate, and morphine. For example, as documented by Mary Schaeffer Conroy, the tariff on salicylic acid was three times the duty on the corresponding amount of aspirin. In 1924, a pharmaceutical production specialist in the Soviet government "still railed about how illogical tsarist tariffs had retarded prewar pharmaceutical industry."[26]

## INDUSTRY VERSUS PROFESSIONAL PHARMACY

The development of the industry in many ways proceeded at the expense of an entrenched group of professionals – pharmacists. Industry and the profession of pharmacy have battled over the territoriality of drug distribution on many different fronts in most countries. In France, two laws in 1803 established the hegemony of pharmacists over competing groups, such as spicers, in the provision of medicines to the public. Although such competition was by no means unique, a characteristic system developed such that, even in the early twentieth century, perhaps half of the licensed French pharmacies were manufacturing one or two specialty items. Furthermore, a 1919 law required supervision by pharmacists over drug manufacturing operations.[27]

In the late nineteenth and early twentieth centuries, strong lobbying by pharmacists was in no small part responsible for legislation that, according to Conroy, effectively stifled development of the Russian pharmaceutical

[25] A. Soldi, "Scientific Research and Evolution of the Italian Pharmaceutical Industry," *Il Farmaco: Edizione Pratica*, 21 (June 1966), 293–312; Michael Robson, "The French Pharmaceutical Industry, 1919–1939," in Liebenau, Higby, and Stroud, *Pill Peddlers*, pp. 107–8.

[26] Mary Schaeffer Conroy, *In Health and in Sickness: Pharmacy, Pharmacists, and the Pharmaceutical Industry in Late Imperial, Early Soviet Russia* (Boulder, Colo.: East European Monographs, 1994), pp. 137–74 (quotation is on p. 166).

[27] Edward Kremers and George Urdang, *History of Pharmacy: A Guide and a Survey*, 1st ed. (Philadelphia: Lippincott, 1940), p. 64; Glenn Sonnedecker, *Kremers and Urdang's History of Pharmacy*, 4th ed. (Philadelphia: Lippincott, 1976), pp. 75–6; Robson, "French Pharmaceutical Industry," p. 108.

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industry.[28] Prior to the French law of 1919, Norway passed two laws, in 1904 and 1914, that required companies to place pharmacists in charge of all pharmaceutical procedures. And that the Norwegian industry sensed competition from the community of pharmacies was evidenced by "We Know How," a 1938 technological exhibit in Oslo in which Nyegaard and Company demonstrated its superiority over pharmacies in providing prepackaged medicines to the masses.[29]

In the United States around the time of the Civil War, the activity of a nascent pharmaceutical industry and the importation of prepackaged medicines had prompted concern among pharmacists. William Procter, Jr. (1817–1874), the leading spokesman for professional pharmacy at this time, was troubled by these developments for many reasons. First, they represented a direct assault on the traditional role of the scientifically trained pharmacist to produce medicines. If the pharmacist becomes a mere dispenser of medicines, Procter lamented, then "he relapses into a simple shopkeeper."[30] Second, Procter wondered if companies would let commercial motives supersede ethical considerations, resulting in substandard drugs. He questioned whether firms would be as willing as pharmacists to abide by the official methods as recommended by the United States Pharmacopoeia. Proctor was unsetteld by the vision of a multitude of firms using a variety of different procedures to produce what would likely be a very erratic product.[31]

Indeed, the pharmaceutical industry's rise in nineteenth-century America did lead to the demise of the pharmacy as a source for stock drug production. And compounding the stock ingredients according to the physician's prescription, the traditional basis of pharmacy practice, faced a similar fate in the twentieth century. In the United States, three in four prescriptions required compounding in the 1930s; two decades later, the proportion dropped to one in four. In 1960, merely one in twenty-five prescriptions called for compounding, and by 1970 the level reached a homeopathic one in a hundred.[32] Although the pharmacy no longer *manufactured* medicines in any sense of the word, the dispensing function grew as the industry cranked out and promoted a litany of new medications.

## WAR AS A CATALYST TO INDUSTRIAL DEVELOPMENT

As in so many other industries, wartime exigencies often stimulated growth in the pharmaceutical industry. For example, the pharmaceutical industry

[28] Conroy, *In Health and in Sickness*, pp. 168–73.

[29] Rolv Petter Amdam and Knut Sogner, *Wealth of Contrasts: Nyegaard & Co., a Norwegian Pharmaceutical Company, 1874–1985* (n.p.: Ad Notam Gyldendal, 1994), pp. 59, 62.

[30] Gregory J. Higby, "Evolution of Pharmacy," in *Remington's Pharmaceutical Sciences*, 18th ed., ed. Alfonso R. Gennaro (Easton, Pa.: Mack, 1990), p. 14.

[31] Gregory J. Higby, *In Service to American Pharmacy: The Professional Life of William Procter, Jr.* (Tuscaloosa: University of Alabama Press, 1992), pp. 49–51.

[32] Higby, "Evolution of Pharmacy," p. 15.

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in Russia grew significantly in the wake of the Crimean War.[33] Many firms struggled during the American Civil War, but E. R. Squibb, Rosengarten and Sons, Powers and Weightman, and John Wyeth and Brothers were key suppliers to the Union army. That side also initiated its own manufacturing operations in Philadelphia and on Long Island in 1864, in direct competition with these firms; but the military plants were dismantled after the war.[34] The Confederacy instituted pharmaceutical plants in over a dozen locations, and because alcohol, an important solvent and extractant, was in short supply, the South also opened several distilleries. The pharmaceutical firms produced needed medicines and analyzed smuggled drugs such as quinine and morphine. In addition, the state of Louisiana established pharmaceutical factories to fulfill some civilian needs. Toward the end of the war, the dearth of drugs was so severe that all available supplies had to be diverted to the army.[35]

The impact of Germany's dominance of the global pharmaceutical market became obvious during World War I. In France, a government study documented the shortage of both raw and finished products and the difficulty of providing the labor to deal with this situation. A controversial program of drug allocations followed; British imports helped fill the void, though these became a source of added hostility. Among postwar proposals to stimulate production were provisions for process patenting and limits on brand-name monopolies. By the 1930s, foreign firms still led in the production of pharmacopoeial products, but French firms controlled the market on proprietary drugs.[36]

The effect of shortages of intermediate and finished pharmaceutical products in the United States was evident in the dramatic wholesale price increases from 1913 to 1916 for popular febrifuge/analgesic drugs. Acetanilide prices increased from $0.21 to $2.75 per pound, Antipyrine grew from $2.35 to $60.00 per pound, and the per pound cost of Phenacetin ballooned fifty-fold.[37] The Office of the Alien Property Custodian seized the German-owned pharmaceutical patents under the amended Trading with the Enemy Act of 1917 and distributed them to U.S. firms. Because few U.S. firms at this time possessed the staff and know-how to produce many of these products, they turned to university scientists for assistance. Abbott Laboratories, for example, engaged University of Illinois chemist Roger Adams (1889–1971) in the

33 Conroy, *In Health and in Sickness*, pp. 141ff.

34 The best source on this subject is George Winston Smith, *Medicines for the Union Army: The United States Army Laboratories during the Civil War* (Madison, Wis.: American Institute of the History of Pharmacy, 1962).

35 Norman H. Francke, *Pharmaceutical Conditions and Drug Supply in the Confederacy*, Contributions from the History of Pharmacy Department of the School of Pharmacy, University of Wisconsin, No. 3 (Madison, Wis.: American Institute of the History of Pharmacy, 1955).

36 Robson, "French Pharmaceutical Industry," pp. 109–11.

37 W. Lee Lewis and F. W. Cassebeer, *Prices of Drugs and Pharmaceuticals*, War Industries Board Price Bulletin 54 (Washington, D.C.: U.S. Government Printing Office, 1919), pp. 6–7.

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manufacture of the sedative Veronal (barbital) and Novocaine. What began as a wartime emergency arrangement for Abbott turned into a collaboration with Adams that lasted six decades.[38]

World War II also had a major impact on the global pharmaceutical industry. In the first place, the balance of power in the industry was shifting away from Germany and toward the United States. The most likely reason for this transformation – besides the impact of the wars on German industry – was the rapid ability of the American industry to cultivate research as a recognized function of firms. Discussion of that development will follow.

World War II also witnessed an intense and abundant combination of private and public resources in the United States and United Kingdom toward therapeutic advances that would be advantageous to the war effort. Most of this activity, of course, stemmed from the discovery of penicillin's systemic chemotherapeutic effect by Howard Florey's (1898–1968) group at Oxford.[39] A huge effort also aimed to synthesize antimalarial agents because of the importance of malaria in the Pacific theater and the disruption of supplies of quinine.[40] These wartime projects had an impact on the growth of the pharmaceutical industry comparable with the coal tar dyes.

Scores of laboratories from academic, governmental, philanthropic, and industrial institutions in these two countries participated in programs initially conducted privately but later sponsored by the Committee on Medical Research of the Office of Scientific Research and Development in the United States and the Medical Research Council in Britain. Participants pooled the latest information on natural and synthetic production of penicillin, and data on syntheses and testing of quinine substitutes were shared in a similar fashion.[41]

Over two dozen U.S. and British pharmaceutical companies took part in these programs,[42] learning to manufacture penicillin in mass quantities by fermentation production and elucidating the chemistry of penicillin. These gains would serve industry well over the next decades in the race to improve penicillin and discover other antibiotics. By 1950, firms had screened thousands of specimens, mostly from the soil, to find another penicillin or

[38] Mann and Plummer, *Aspirin Wars*, pp. 44–6; Swann, *Academic Scientists and the Pharmaceutical Industry*, pp. 61–5.

[39] This story is exceedingly well documented. The core primary and secondary sources are appended to John Patrick Swann, "The Discovery and Early Development of Penicillin," *Medical Heritage*, 1, no. 5 (1985), 375–86. Omitted from that list is Gladys L. Hobby, *Penicillin: Meeting the Challenge* (New Haven, Conn.: Yale University Press, 1985).

[40] On why this became an issue at all during the war, see Norman Taylor, *Cinchona in Java: The Story of Quinine* (New York: Greenberg, 1945).

[41] On the organization of the penicillin work, see especially a study by someone who participated in the wartime program: John C. Sheehan, *The Enchanted Ring: The Untold Story of Penicillin* (Cambridge, Mass.: MIT Press, 1982). The best source on the antimalarial program is E. C. Andrus et al., *Advances in Military Medicine*, 2 vols. (Boston: Little Brown, 1948), vol. 2, pp. 665–716.

[42] For a list of participants in the various American wartime research programs, see Andrus et al., *Advances in Military Medicine*, vol. 2, pp. 831–82.

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streptomycin;[43] and indeed, these tedious screening programs yielded several useful and profitable pharmaceuticals.[44] Antibiotics had a sudden impact on the industry and on medical practice. Six years after the war ended, the proportion of U.S. prescriptions written for antibiotics climbed from nil to about 14 percent. Within ten years of the end of the war, antibiotics were responsible for up to about 40 percent of total sales for some well-established American firms.[45] But as some then feared and we now know, "antibiotic abandon" ensued – and concomitantly, antibiotic resistance.[46]

## INDUSTRIAL GROWTH AND THE ROLE OF RESEARCH

Progress in the institutionalization of research in the pharmaceutical industry has been a prerequisite for those new antibiotics, analgesics, oncologic drugs, cardiovascular agents, or almost any contribution to the therapeutic armamentarium. The early success of the German drug industry was largely due to its support of in-house research and/or cultivation of ties with academic scientists. Hoechst, for example, supported Ehrlich's work leading to the introduction of Salvarsan. But commercial pharmaceutical interests in late nineteenth-century Germany simply were following the precedent in chemistry from earlier in the century, in which academic–industrial ties had evolved to the point that firms were competing to align themselves with the best chemists and their students.[47] In Britain, Burroughs Wellcome's rise to prominence can be linked to its unique establishment of laboratories dedicated to chemical and physiological research in the 1890s, headed by two respected scientists, Frederick B. Power (1853–1927) and Henry H. Dale (1875–1968), respectively.[48]

From the later nineteenth century, selected firms in the United States pursued modest research activities, including Parke-Davis, Mulford, and Smith

[43] Walter Sneader, *Drug Prototypes and Their Exploitation* (Chichester: Wiley, 1996), p. 510, reports that Parke-Davis engaged Paul Burkholder (1903–1972), a botanist at Yale, to analyze soil samples for activity against six bacteria. Among the 7,000 samples Burkholder analyzed was an active microbe from which Parke-Davis workers isolated chloramphenicol; this turned out to be a blessing and a curse to therapeutics. The broad spectrum antibiotic turned out to cause fatal blood dyscrasias in a very small proportion of patients. The discovery of another broad-spectrum antibiotic, oxytetracycline (1950), reportedly involved more than 100,000 soil samples obtained, as was the case with chloramphenicol, from around the world. See John Parascandola, "The Introduction of Antibiotics into Therapeutics," in *History of Therapy*, ed. Yosio Kawakita et al. (Tokyo: Ishiyaku EuroAmerica, 1990), p. 274.

[44] For example, see Harry F. Dowling, *Fighting Infection* (Cambridge, Mass.: Harvard University Press, 1977), pp. 174–92.

[45] Parascandola, "Introduction of Antibiotics into Therapeutics," p. 277.

[46] James C. Whorton, "'Antibiotic Abandon': The Resurgence of Therapeutic Rationalism," in *The History of Antibiotics: A Symposium*, ed. John Parascandola (Madison, Wis.: American Institute of the History of Pharmacy, 1980), pp. 125–36.

[47] Swann, *Academic Scientists and the Pharmaceutical Industry*, p. 27.

[48] Tansey and Milligan, "Early History of the Wellcome Research Laboratories." Dale joined the Wellcome Physiological Research Laboratories in 1904 and became director two years later.

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Kline & French. But the U.S. drug manufacturing industry did not begin to approach the level of industrial research in Germany until the era between the two world wars, when research expenditures increased as a percentage of sales, research staffs grew quantitatively and qualitatively, and facilities dedicated to research emerged. Laboratories established by Merck, Abbott, and other firms were often launched with great fanfare: Research was good publicity as well as good business.

A 1971 U.S. National Science Foundation study determined that only two industries (aerospace and communications) spent a higher percentage of net sales on research than the pharmaceutical industry.[49] That was no doubt the case, but industry sources tend to gloss over the alleged research expense to move a drug from the lab bench to the medicine cabinet. Companies do not provide details about how such costs are determined – in such a way that a disinterested observer might be able to confirm the claims – but data supplied by the Health Care Financing Administration, the Office of Technology Assessment, and a pharmaceutical economist suggest that the proportion of research and development in the total cost of bringing a drug to market is much smaller – about 16 percent – than the industry's trade association would have the public believe.[50]

## REGULATING THE INDUSTRY

The pharmaceutical industry has been responsible for countless valuable additions to the drug compendia, but it has also given us products that assaulted the public health – drugs such as thalidomide, chloramphenicol, and clioquinol. Countries have responded quite differently, if at all, to the problem of unsafe, ineffective, and deceptive drugs in the marketplace. By 1928, Norway's Proprietary Medicines Act required that "specialty medicines" (any medicinal packaged or formulated in a distinguishable fashion) be approved by the government; a product's efficacy and its necessity to the materia medica were considered in the evaluation. Included in the National Institute of Public Hygiene of Hungary was a Section of Drug Control, established in 1925; for the most part, this section simply registered drugs. After the drug industry was nationalized in 1948, the section was succeeded by the National Institute of Pharmacy, which considerably extended drug regulation in Hungary. Eventually the institute authorized clinical studies, approved drugs on the basis of safety and efficacy, licensed

[49] John P. Swann, "Evolution of the American Pharmaceutical Industry," *Pharmacy in History*, 37 (1995), 79–82.

[50] Drake and Uhlman, *Making Medicine, Making Money*, p. 47. Now known as the Pharmaceutical Research and Manufacturers Association, this drug trade group had been known simply as the Pharmaceutical Manufacturers Association for almost forty years. See Sonnedecker, *Kremers and Urdang's History of Pharmacy*, p. 333.

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manufacturing facilities, and conducted postmarketing surveillance, among other functions.[51]

In the United States, regulation of biological medicines evolved differently from that of drugs. According to a law passed in 1902, production of so-called biologics had to be supervised by qualified staff, factories were inspected, manufacturers had to be licensed prior to marketing a regulated product, and the government sampled products on the open market for purity and potency. A different agency was charged with control over drugs of nonbiological origin under separate legislation four years later. Basically, the 1906 law addressed labeling of drugs, prohibited adulteration, and provided for factory inspections. An overhaul of the 1906 law in 1938 required government approval of new drugs on the basis of safety, and it mandated enhanced labeling for safe consumer use of a drug. In 1962, efficacy became a requirement for approving a new drug and all drugs introduced since 1938. The U.S. drug laws have been amended in many ways, but these were the essential changes during the twentieth century.[52]

Regulation of the pharmaceutical industry in many developing nations has ranged from corrupt to absent, as documented by Milton Silverman, Mia Lydecker, and Philip Lee. Originally these authors explored the extent to which some multinational pharmaceutical companies took advantage of these largely unregulated markets.[53] However, their later investigation revealed the culpability of the indigenous industry, from "licensed" commercial establishments to fly-by-night clandestine operations – and the lack of local or national statutes and staff to deal with them. In 1986, contaminated glycerine was the likely cause of fourteen unexpected deaths that occurred in a prominent Bombay hospital. A ten-month public hearing exposed the firm responsible, the corrupt hospital administration, the inept regional drug control authority, and the dereliction of office by the health minister. Reluctantly, the government responded by sacking the individuals involved.[54]

In 1992, Silverman and his coauthors reported a prescription for medical disaster in Brazil, where at least 20 percent of the drug supply outside

[51] Amdam and Sogner, *Wealth of Contrasts*, pp. 60–1; Karoly Zalai, "The Process of Development from Apothecary Activity into Pharmaceutical Industry in Hungary," in *Farmacia e Industrializacion: Libro Homenaje al Doctor Guillermo Folch Jou*, ed. F. Javier Puerto Sarmiento (Madrid: Sociedad Espanola de Historia de la Farmacia, 1985), pp. 165–8.

[52] James Harvey Young, "Federal Drug and Narcotic Legislation," *Pharmacy in History*, 37 (1995), 59–67.

[53] Milton Silverman, *The Drugging of the Americas: How Multinational Drug Companies Say One Thing about Their Products to Physicians in the United States, and Another Thing to Physicians in Latin America* (Berkeley: University of California Press, 1976); Milton Silverman, Philip R. Lee, and Mia Lydecker, *Prescriptions for Death: The Drugging of the Third World* (Berkeley: University of California Press, 1982).

[54] Milton Silverman, Mia Lydecker, and Philip R. Lee, *Bad Medicine: The Prescription Drug Industry in the Third World* (Stanford, Calif.: Stanford University Press, 1992), pp. 151–3. The authors do not indicate the fate of the firm that supplied the questionable glycerine.

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of hospital pharmacies was fraudulent. Included in this group were grossly subpotent counterfeit drugs for life-threatening conditions. Typically, these were sold directly to community pharmacies by the manufacturing miscreants. Both interests, according to the evidence, appeared to be bribing the undersalaried state pharmacy inspectors. In addition, the authors state that the responsibility for inspecting all manufacturers rested with just two individuals – who were inadequately trained. Political changes in Brazil during the 1980s apparently did not improve this state of affairs.[55] Regulated drug labeling was as evanescent as controlled drug distribution.[56]

So, it might not be surprising that Brazil is revisiting one of the darkest periods of twentieth-century therapeutics. That country has a large number of registered leprosy patients, approximately 78,000 at the beginning of the year 2000 – a figure that had dropped from about 106,000 in 1997.[57] Thalidomide, the sedative that caused thousands of birth defects in the late 1950s and early 1960s, has long been employed in the treatment of leprosy in Brazil (and elsewhere). In fact, in July 1998 the U.S. Food and Drug Administration, which did not approve thalidomide in its earlier life, approved this drug under extremely restricted access for a form of leprosy. But thalidomide has made its way into the hands of Brazilian women who do not suffer from leprosy and who are not apprised of the dangerousness of this drug. Consequently, since the mid-1960s, at least thirty-three cases of thalidomide-induced phocomelia have been reported from that country.[58]

## CONSOLIDATING THE INDUSTRY

Mergers have always been important in the evolution of the pharmaceutical industry. For example, the merger history of Merck Sharp and Dohme over the nineteenth and twentieth centuries involves many more companies than that name implies.[59] German dye manufacturers began consolidating in the first decade of the twentieth century; their efforts were refined and elaborated as participating firms shared patents and partitioned marketing territories, which they then defended vigorously. This system

[55] Silverman, Lydecker, and Lee, *Bad Medicine*, pp. 154–9.

[56] Ibid., pp. 247ff.

[57] Miriam Jordan, "Leprosy Remains a Foe in Country Winning the Fight Against AIDS," *Wall Street Journal*, August 20, 2001, at http://www.aegis.com/news/wsj/2001/WJ010805.html (accessed January 2, 2003); Anonymous, "Footballer Pele to be 'Ambassador' for Leprosy Elimination," World Health Organization Press Release WHO/57, July 18, 1997, at http://www.who.int/archives/inf-pr-1997/en/pr97–57.html (accessed January 2, 2003).

[58] E. E. Castilla et al., "Thalidomide, a Current Teratogen in South America," *Teratology: The Journal of Abnormal Development*, 54 (1996), 273–7; http://www.thalidomide.org/FfdN/Sydamer/SYDAMERI.html (accessed January 2, 2003).

[59] See [P. Roy Vagelos, Louis Galambos, Michael S. Brown, and Joseph L. Goldstein],*Values and Visions: A Merck Century* (Rahway, N.J.: Merck, 1991). If nothing else, company histories often do a good job of capturing the genealogy of a firm; see Higby and Stroud, *History of Pharmacy*, pp. 43–54.

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eventually resulted in the powerful post–World War I formation of I. G. Farben, the giant chemical and pharmaceutical cartel. The Swiss quickly responded with their own conglomeration of Sandoz, Ciba, and Geigy: Basler I. G.[60]

Mergers and acquisitions continued from time to time until the late 1980s, when this activity increased noticeably; the total value of pharmaceutical mergers for the brief period from 1988 to 1990 was $45 billion, which included such prominent unions as SmithKlineBeecham, Bristol-Myers Squibb, and Marion Merrell Dow (all of which formed in 1989).[61] The trend continued unabated in the 1990s, as Glaxo merged with SmithKlineBeecham to form GlaxoSmithKline, Novartis emerged from the union of Ciba-Geigy and Sandoz, Zeneca of Britain combined with Astra of Sweden as AstraZeneca, and Hoechst merged with Roussel, Marion Merrell Dow, and Rhone Poulenc Rorer from 1994 to 1999 to form Aventis Pharma.[62] Today, a comparatively small number of firms control most of the drug sales in the world, and the strategy for product development seems to be as much about acquisition as about the dedication of more funds to research and development.

A variety of circumstances, events, people, laws, institutions, and scientific developments have molded the international pharmaceutical industry. Like so many of the biomedical industries, it has come under increasing scrutiny by legislative authorities as the cost of health care has skyrocketed. The pharmaceutical industry can argue quite accurately that it has contributed importantly to the amelioration of disease, and rather economically at that – in spite of therapeutic disasters and charges of price manipulations. But industry officials, and especially public health policymakers, should never lose sight of the fact that practical results rest on a fundamental understanding of basic life and disease processes. Drug companies have contributed to that understanding, but the foremost estate of science in shepherding basic knowledge is and always was noncommercial. That fact should resonate in any policy discussion of public health or biomedicine.

[60] Ihde, *Development of Modern Chemistry*, pp. 671–4; Mann and Plummer, *Aspirin Wars*, pp. 53ff., 70ff.; Riedl, "Brief History of the Pharmaceutical Industry in Basel," p. 64.

[61] Robert Balance, Janos Progany, and Helmet Forstener, *The World's Pharmaceutical Industries: An International Perspective on Innovation, Competition and Policy* (Hants: Edward Elgar, 1992), pp. 183–4.

[62] Landau, Achilladelis, and Scriabine, *Pharmaceutical Innovation*, p. 139; Information Centre, Royal Pharmaceutical Society of Great Britain, "Mergers and Takeovers within the Pharmaceutical Industry," July 2002, at http://www.rpsgb.org.uk/pdfs/mergers.pdf (accessed January 3, 2003).

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# 9

# PUBLIC AND ENVIRONMENTAL HEALTH

*Michael Worboys*

The principles of modern public health have been loftily defined as "the protection and promotion of the health and welfare of its citizens by the state."[1] Governments have taken on these responsibilities in different ways, reflecting different political cultures, disease environments, and pressures from civil society. Public health measures have concentrated on four main areas: controlling hazards in the physical environment, ensuring the quality of food and water, preventing the transmission of infectious diseases, and providing vaccinations and other individual preventive services. In each sphere, professionals have developed disciplines and technologies that have historically focused on the prevention of disease more than the promotion of health, although health education became increasingly important in the twentieth century.

Understanding and managing the physical environment has required the use and development of the physical, biological, and engineering sciences, with interdisciplinary or multidisciplinary work a particular feature of public health activity. Ensuring the quality and quantity of food and water supplies also involved all the sciences. For example, a secure water supply has required knowledge of rainfall patterns from meteorology, water movements from geology and geography, extraction and storage techniques from civil engineering, processing and quality control from chemistry and biology, and physics to help deliver supplies to users. Preventing the spread of infectious diseases was a multidisciplinary enterprise involving the environmental, biological, human, and social sciences, and since the 1890s an increasing contribution from medical laboratory sciences, such as bacteriology and immunology. The development of modern preventive services began with smallpox vaccination programs and urban improvements, but in the twentieth century this approach burgeoned in Western industrialized countries to include the provision of personal health care services, medical surveillance, and health

[1] George Rosen, *A History of Public Health* (New York: MD Publications, 1958).



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education. Needless to say, the quality of services and their distribution has varied between countries, and at the start of the twentieth-first century many third world countries still lack basic water and sewerage provision, let alone medical and welfare services.

The history of modern public health can be divided into three periods during which new sites for professional activity were developed. In the period 1800–90, the main focus was on the *health of towns* as new methods of disease control were introduced that concentrated on the management of environmental and epidemic threats, and these became the basis for the institutionalization of public health. In the years 1890–1950, the major new concern was over *the health of nations*, especially economic and social efficiency, which was promoted by measures aimed at individuals and their behavior. Environmental approaches to public health were maintained, although they were increasingly routinized. Finally, after 1950, new attention was given to *world health*, particularly as a result of population growth, the impact of advanced industrial technologies, such as nuclear products and pesticides, on individuals and the biosphere, and the possibilities for the spread of infections through the increased speed and frequency of international travel.

## 1800–1890: THE HEALTH OF TOWNS

The origins of modern public health lay in the early nineteenth century and the responses of reformers and medical practitioners to the effects of urbanization and industrialization in Europe and North America.[2] In the Enlightened Absolutist states of continental Europe, these activities built on the tradition of medical police, the institutions through which the central state took an often authoritarian role in measuring its population and managing its health. In Britain and the United States, previous efforts to ameliorate conditions had come from private initiatives or local authorities. However, it was the overcrowding, pollution, and environmental degradation of early industrial towns, with their high morbidity and mortality rates and vulnerability to epidemics, that sparked public health movements. Initially, reformers implicated the atmosphere as the carrier of disease poisons, referred to as miasmas. From the 1840s to the 1880s, reformers and medical practitioners sought to reduce the dangers of urban and industrial conditions, mainly by imposing legally defined standards that sanitary engineers and other public health workers, such as public analysts and meat inspectors, strove to enforce. At the same time, public health doctors monitored the incidence of disease, administered vaccinations, and exhorted people to keep clean and behave in a hygienically responsible way.

[2] Dorothy Porter, *Health, Civilization and the State: A History of Public Health from the Ancients to Modern Times* (London: Routledge, 1998); Dorothy Porter, ed., *The History of Public Health and the Modern State* (Amsterdam: Rodopi, 1994).

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Public health at this time was built on two traditions, one focusing on the *environment* and the other on *people*. One implication drawn by many historians is that in the middle decades of the nineteenth century those activists whose approach was rooted in environmentalism tended to oppose contagionist models of the spread of disease, whereas the latter approach favored them. The environmental approach, with its roots in Hippocrates' *Airs, Waters and Places*, looked to physical and biological scientists to understand the external risks to health and to engineers to produce urban improvements. According to an influential study by Erwin Ackerknecht, this approach was predominant in liberal capitalist countries and was exemplified by antipathy to quarantines.[3] Approaches that were centered on people derived from the mercantilist and Absolutist assumption of the value of a healthy, populous country, codified in the doctrines of medical police. Medical police agencies were associated with strong regulatory states and paternalism, and their work aimed to promote health and wealth by ensuring population growth and trying to isolate citizens from epidemics and nuisances. Typical medical police activities were the supervision of quarantines, disease surveillance, and the regulation of medical and midwifery practice. Although this approach utilized the skills and knowledge of medical practitioners, it generated and depended much more on administrative and social disciplines, especially statistics. In many instances, the two traditions were complementary; for example, when cholera threatened in the early 1830s, all European governments intervened in some way, with most prudently adopting both quarantines and hygienic measures. Nonetheless, historians have continued to debate Ackerknecht's suggestion that political and economic factors shaped theories of disease and their adoption. There is now a consensus among historians that the medical profession was not split simply into "contagionists" and "anticontagionists." Rather, individual doctors took different views on different diseases, with many conditions being regarded as contingently contagious, though there were, of course, disagreements about the causal factors and the degree of contagion in different circumstances.[4] However, there is little dispute that economic and political interests did determine policy choices about quarantines, though not in direct or consistent ways. Peter Baldwin's rich comparative history of disease-control policies in Europe between 1830 and 1930 argued an important role for what he terms "geo-epidemiology" – the unique dynamics of an epidemic within a country and with other countries.[5] Intriguingly reversing the familiar argument that disease-control policies followed politics, he suggests that the ways in which different states responded to epidemics were major factors in overall state formation.

[3] Erwin Ackerknecht, "Anticontagionism between 1821 and 1861," *Bulletin of the History of Medicine*, 22 (1948), 561–93.

[4] Margaret Pelling, *Cholera, Fever and English Medicine, 1825–65* (Oxford: Oxford University Press, 1978).

[5] Peter Baldwin, *Contagion and the State in Europe, 1830–1930* (Cambridge: Cambridge University Press, 1999).

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The collection and collation of data on the incidence of disease and the progress of epidemics became a priority for governments and civil agencies. Enlightenment thinkers and propagandists in the eighteenth century had promoted the extension of numerical methods to all spheres of life as part of their project on the creation of a "science of man." The economic and political dimensions of this project were pursued through the discipline of statistics, a term coined in 1787. The promoters of this subject aimed to quantify the wealth of nations, beginning with censuses and the collection of other national data, which were then extended to recording births and deaths. Whereas the development of statistical knowledge was the responsibility of government agencies in the German states, in liberal Western states it was pursued by individuals and voluntary societies. In Belgium, Adolphe Quetelet (1796–1874) pioneered the use of averages and other methods to determine the physical and social geography of disease in the 1830s and 1840s. At the same time in France, Louis René Villermé (1782–1863) related changes in the economy to mortality and morbidity trends and was among the first to question the Hippocratic consensus on the overriding importance of the environmental determinants of health.[6] In Britain, statistical societies – highbrow "reform" clubs – were founded in Manchester in 1833 and London in 1834, presaging the appointment of William Farr (1807–1883) as Registrar General in 1837. Like Villermé in France, Farr became involved in the public health movement, providing reformers with data on the mortality consequences of overcrowding, industrial conditions, and local epidemics.[7] Edwin Chadwick (1800–1890), a British government insider with political interests to defend, marginalized the views of those, like the Scottish physician William Poulteney Alison (1790–1859), who maintained that economic and social conditions were major determinants of health.[8] Instead, Chadwick associated public health with the physical conditions of the urban environment and mobilized, among other evidence, the greater life expectancy of those in rural areas who lived in greater poverty. It is ironic that rural areas, where the majority of the population of Europe lived until well into the twentieth century, were often defined by epidemiologists and statisticians as "healthy districts" when it was well known that the condition of dwellings and lack of basic sanitation meant that most people in the countryside lived in unsanitary conditions.

The idea that public health was centrally about environmental management developed in the 1830s and 1840s in the analysis and propaganda of the

[6] Ann La Berge, *Mission and Method: The Early French Public Health Movement* (Cambridge: Cambridge University Press, 1992); William Coleman, *Death Is a Social Disease: Public Health and Political Economy in Early Industrial France* (Madison: University of Wisconsin Press, 1982).

[7] John M. Eyler, *Victorian Social Medicine: The Ideas and Methods of William Farr* (Baltimore: Johns Hopkins University Press, 1979).

[8] Christopher Hamlin, *Public Health and Social Reform in the Age of Chadwick: Britain, 1800–1854* (Cambridge: Cambridge University Press, 1998).

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sanitarians.[9] This group led the wider public movements that emerged in most European countries and in urban areas on other continents. Prompted by the high death rates reported by statisticians, the local and national crises associated with fever epidemics, and the wider political concerns about the condition (physical and moral) of the new urban working class, public health movements campaigned for measures to reduce urban mortality and morbidity. In Northern Europe and North America, they used a disease model that made "filth" and putrefaction the main causes of fevers. In turn, they identified the principal dangers to health as polluted air, nuisances – such as fly tips, poisoned and blocked watercourses, contaminated land and industrial waste, pig sties and town dairies – and, not least, the bodies of the Great Unwashed. The dominant explanation of fevers was the zymotic theory, which derived from Justus von Liebig's (1803–1873) assumption that the processes of fermentation and putrefaction were caused by the action of a "ferment," a chemical substance with particular catalytic properties. Zymotic processes arose in filth, and the poisons generated were assumed to spread in the air to vulnerable populations, causing their bodies to become "inflamed" and "infected," effects only too evident in fevers, skin eruptions, and debility. Although sanitarians recognized that disease ferments could be spread via water supplies, food, and to a limited extent by person-to-person contagion, they were most worried about the threat posed by the atmosphere. Poisoned air or miasmas, marked by their smell and other perhaps immaterial qualities, were seen as able to infiltrate anywhere and carry infection across classes and other social boundaries. As well as acting directly as exciting causes of fevers, miasmas were also believed to weaken bodies and predispose them to other afflictions. However, there were other traditions and analyses of the problem, including those that stressed contagion and poverty as predisposing causes of disease.[10]

The major intellectual weapon that reformers deployed against disease threats was sanitary science. The synthetic character of this discipline is nicely captured in Latour's description: "an accumulation of advice, precautions, recipes, opinions, statistics, remedies, regulations, anecdotes, case studies."[11] Sanitary science was seen to be both ancient and very modern. Hippocrates was cited as its founder, though its practitioners also claimed the mantle of modern science. They trusted that their analyses would reveal the (natural) laws of health and that these would guide expert actions and advice to the government and the public. A cornerstone of sanitary science was epidemiology,

[9] John Duffy, *The Sanitarians: A History of American Public Health* (Urbana: University of Illinois Press, 1990).

[10] John V. Pickstone, "Dearth, Dirt and Fever Epidemics: Rewriting the History of British 'Public Health', 1750–1850," in *Epidemics and Ideas: Essays on the Historical Perception of Pestilence*, ed. Terence Ranger and Paul Slack (Cambridge: Cambridge University Press, 1992), pp. 125–48.

[11] Bruno Latour, *The Pasteurization of France* (Cambridge, Mass.: Harvard University Press, 1988), p. 20.

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which promised to reveal the multiple causes of disease by locating it in geographical space, the social structure, and historical time. Sanitarians mainly targeted epidemic and occupational diseases, both of which seemed to have external exciting causes. They largely ignored constitutional and idiopathic afflictions, such as tuberculosis and rheumatism, whose origins were seen to be internal and spontaneous and hence nonpreventable. There seemed to be two main ways to attack external sources of disease: either to improve the environment so that they were not produced in the first place, or to prevent the exposure of individuals and communities when they arose locally or were imported. The dominant poisoning analogy for fevers led chemists to try and ascertain the nature of toxic substances, and when that proved difficult, to determine safe levels by measuring indicators, such as nitrogen and carbon levels. The analysis of water proved easier than that of air, so despite the importance of the atmosphere in sanitary ideology, there were fewer studies of air pollution or the nature of miasmas.[12]

From the mid-nineteenth century, scientists began to switch from chemical to biological explanations of fevers, and investigators began to look for living disease agents in the environment and in human bodies.[13] The ability of microscopists to show minute living organisms had grown steadily because of the technical improvement of their instruments, but the significance of so-called monads (as the simplest living organisms were termed) was open to dispute. Medical practitioners first portrayed them as signs of gross contamination, and sanitarians used the observations to attack the performance of water companies. From the 1860s, some doctors and biologists used parallels with known parasites, such as tapeworms and fungi, to suggest that monads and other "animalcules" could be pathogenic and act as "disease germs." Against this view, chemically inclined sanitarians argued that ingesting microorganisms was no different from eating fish, that such organisms might play a role in removing dangerous material from the body, and that their presence might be a good indicator of the quality of water.

Ideas of recycling and natural purification were often associated with concerns about filth and its dangers. Although human, animal, and other organic wastes were regarded as threats to health, they were also seen as potentially beneficial if collected and transported to rural areas to be spread on the land to help maintain its fertility. Agricultural practices were never far from the experiences of nineteenth-century urban life, and ideas of crop rotation and recycling exemplified the providential character of nature. Those who believed that disease ferments were biological rather than chemical agents saw putrefaction in teleological terms, as nature's way of preparing matter for reuse by organisms. The development by municipal engineers of

[12] Christopher Hamlin, *A Science of Impurity: Water Analysis in Nineteenth Century England* (Bristol: Adam Hilger, 1990), pp. 35–6.

[13] John Eyler, "The Conversion of Angus Smith: Chemistry to Biology," *Bulletin of the History of Medicine*, 56 (1980), 216–24.

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large-scale sewage systems to remove human and other waste from towns raised the problem of disposal to new levels. In towns near the coast, waste was dumped at sea, where dilution, marine life, and time would render it safe. However, in many inland towns, dumping was not an option, and hence it became important to ensure the safe collection and removal of wastes, plus their controlled decomposition, purification, and safe reuse. Different methods of waste management were developed, either "dry" systems as in night soil collections or "wet" as in the system of flush drains. Many techniques of waste treatment were developed, from physical methods such as filtration and settlement through to complex chemical and biological processes. As the enterprises grew, the knowledge and management skills became highly technical and specific, so that sanitary engineers were able to establish themselves as a separate professional body. Within medicine, public health doctoring was slow to emerge as a distinct activity, not least because specialization in medicine was not common, and few doctors sought a full-time career in an area that was neither secure, of high status, nor economically rewarding.

Many historians have argued that the etiological models provided by germ theories of disease were the key factor in the erosion of environmentalist thinking in public health. They maintain that as more and more fevers were shown to spread from person to person by the transmission of pathogenic bacteria, or via specific channels such as the water supply, food, or insect vectors, public health professionals began to attack pathogens directly or target specific points of passage. Against this, revisionist historians have argued that the impact of new bacterial ideas and practices was more complex, that the switch to germ theories of disease was protracted, and that public health doctors continued to implicate environmental factors in disease prevention.[14] In the last quarter of the nineteenth century, few doctors and scientists saw bacteria as all-powerful invaders; most understood their actions in terms of the metaphor of "seed and soil" – the germination of the "seeds" of disease requiring a vulnerable human "soil." For example, the antiseptic system of managing wound infections was based on the "panspermist" belief that the atmosphere was full of minute living organisms, but these only caused sepsis when they fell into dead or damaged tissue. Such views were congruent with the clinical and epidemiological experience of fevers, where some people were more open to infection than others, and where the same infection varied in intensity between individuals and communities.

Many researchers argued that disease germs might have to pass through developmental stages outside of the human body. The first accepted demonstration of a bacterial etiology, Robert Koch's (1843–1910) work on anthrax, revealed a disease spread by spores that could lay dormant in the soil for

[14] Nancy Tomes, *The Gospel of Germs: Men, Women and the Microbe in American Life* (Cambridge, Mass.: Harvard University Press, 1998), pp. 1–90; Michael Worboys, *Spreading Germs: Disease Theories and Medical Practice in Britain, 1865–1900* (Cambridge: Cambridge University Press, 2000).

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years. Cholera was the first major public health disease for which a specific germ was identified, again by Koch in 1883–4, although it took over a decade for a consensus to be reached that this agent was the essential cause. Nonetheless, bacterial germ theories gradually dominated medical thinking and were accommodated with older explanations of the origins of epidemics; for example, Max Pettenkofer's (1818–1901) theory that cholera was produced by rising groundwater was translated into the notion that the germs of cholera and typhoid fever were reactivated by dampness. The number of diseases, such as smallpox and measles, where transmission was by direct, unmediated contagion seemed to be quite small, and even here physical variables, such as winds and cold, were assumed to predispose the body to infection.

Public health authorities increasingly sought to manage infectious diseases and epidemics by vaccination, isolation, disinfection, and notification. The production and dissemination by state organizations of the cowpox vaccine that protected against smallpox remained a core public health activity in most states. However, the work of Louis Pasteur (1822–1895) in producing attenuated bacteria that also protected against specific infections held out the hope of "new vaccines" for all infectious diseases. In the 1870s and 1880s, the isolation of the sick shifted from the home to large special hospitals, where the state would cover the costs for the greater public good. Many of the new isolation hospitals were established for smallpox, but as epidemics of this disease waned, they were used for infections such as scarlet fever and diphtheria, quickly becoming children's hospitals. Many local authorities established disinfection stations, where the furniture and clothes of families suffering epidemic diseases could be sterilized. The use of disinfectants in the home was encouraged by doctors and, more importantly, through a whole new array of antigerm hygiene products marketed by local and national companies.[15] The notification of cases of disease was sought in order to allow doctors to map the origins and progress of infections and to trace the contacts of sufferers. Notification was a contested issue, as it touched upon the sensitive relations between the state and the private practitioner and upon doctor–patient confidentiality.

Although they question a determinative role for bacteriology, revisionist historians acknowledge that its ideas and practices were used to further medicalize public health. Bacteriological ideas supported the argument that the change from the "blunderbuss" of sanitary science to the "precision rifles" of preventive medicine also brought economies and efficiencies, not to mention better forms of surveillance. In most countries, disease notification legislation was tightened and the number of beds in isolation hospitals was massively increased. These approaches gave opportunities for public health doctors to use their clinical skills and for modernizers in medicine to promote the establishment of bacteriological laboratories to provide diagnostic

[15] Tomes, *Gospel of Germs*, pp. 48–112.

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and other services. However, the recasting of zymotic diseases as "bacterial" and "communicable" continued to be uneven. The microbiology of many common diseases, such as scarlet fever and smallpox, remained uncertain well into the twentieth century (when they were shown to be viral diseases). The rich resources of bacteriology were mobilized to support all manner of policies and ideals, and not just reductionist, laboratory-based, disease-centered approaches.[16] For example, in health education, the universally recommended practice of sleeping with one's bedroom window open was said to reduce the number of bacteria in the air, as well as producing a dry, high-oxygen environment that was unfavorable to germs.

Any switch by public health agencies away from general environmental improvements was protracted and partial. Indeed, one initial reaction to the identification of bacteria was to heighten fears about the power of the disease agents lurking in the environment, as in panspermism. Paul Starr's much quoted comment that bacteriology created a "new conception of dirt" is apposite: Germs were new but still identified with filth.[17] Even when the association of specific bacteria with particular infections led to the identification of an agent with a specific disease, this did not necessarily mean single-factor causation. Within medicine, bacteria were mostly regarded as exciting causes that only acted with other predisposing causes; for example, the *Tubercle bacillus* was more common and destructive among the poor and those whose lungs were already damaged from working in dusty indoor trades. Certain habits would increase risks of infection, and hence antituberculosis propaganda warned people to control spitting, to be careful with milk and meat, and to avoid dark, dank, and dirty places.[18] But other types of hygienic advice, such as avoiding alcohol, making homes more open and airy, and being careful who you married, were less about avoiding infection than about strengthening bodily constitutions.

Among medical practitioners in tropical colonies commitment to environmental influences in disease causation remained particularly strong until at least 1900.[19] In the nineteenth century, the assumptions of sanitary science had received powerful corroboration from the high mortalities suffered by Europeans in the tropical extremes of temperature, humidity, and sunshine. Doctors assumed that such latitudes gave familiar diseases a particular intensity as well as producing unique tropical fevers. The reduction of European deathrates in the tropics during the nineteenth century was largely achieved by the importation of the sanitary measures developed for towns in

[16] Barbara Rosencrantz, "Cart before the Horse: Theory, Practice and Professional Image in American Public Health, 1870–1920," *Journal of the History of Medicine*, 29 (1974), 55–73.

[17] Paul Starr, *The Social Transformation of American Medicine* (New York: Basic Books, 1982), pp. 189–90.

[18] Katherine Ott, *Fevered Lives: Tuberculosis in American Culture since 1870* (Cambridge, Mass.: Harvard University Press, 1996).

[19] Mark Harrison, *Public Health in India: Anglo-Indian Preventive Medicine, 1859–1914* (Cambridge: Cambridge University Press, 1994).

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Europe, plus the adoption of special measures such as quinine prophylaxis for malaria.[20] The concentration of Europeans in coastal towns and military bases allowed sanitary measures to be targeted on small areas and controlled populations. The effects of climate were dealt with by the careful "seasoning" of new arrivals, periodic leave, the use of hill stations, and personal hygiene. Sanitary engineering was also introduced into the towns and cities of new nations, such as Brazil, and modernizing older nations, such as Japan and China. However, rapid rates of urban growth, complex local politics, and the weak economic base for tax-raising meant that the sanitary infrastructure was often incomplete or functioned irregularly. Colonial settlements and major ports outside of Europe remained vulnerable to epidemics, particularly of cholera, yellow fever, and the plague. From the 1860s, governments were subject to pressure from a series of International Sanitary Conferences to institute quarantines during epidemics and to improve sanitation to remove the conditions in which epidemics could settle and spread. As in Europe and North America, so in colonies and new nations, there continued to be a divide within the public health professions between those who continued to favor general environmental improvements and those who favored specific measures targeted at particular disease agents or aimed at controlling diseased people. In the 1890s, these approaches were finely balanced, but after 1900 the latter began to attract more professional, political, and public attention.

## 1890–1950: THE HEALTH OF NATIONS

Contemporaries and historians have agreed that there was a major reorientation in public health around 1900. The accepted idea is that the focus switched from the physical environment to individual citizens, with a broadening of interest in national populations.[21] These changes were reflected in specialist formations, as the previously multidisciplinary "public health" split into preventive medicine, sanitary engineering, and a number of analytical sciences. The context of these changes was increased international economic competition, aggressive imperialism, new initiatives in social welfare, and falling mortality rates. Health concerns began to crystallize around the issue of physical and racial degeneration, with many new initiatives aiming to deliver medical services to improve the "quality" of people as individuals rather than to prevent disease in communities. This is not to say that other approaches were neglected. Indeed, alongside the new person-centered and disease-centered approaches, there were significant continuities. Water

[20] Philip D. Curtin, *Death by Migration: Europe's Encounter with the Tropical World in the Nineteenth Century* (Cambridge: Cambridge University Press, 1989).

[21] Elizabeth Fee and Dorothy Porter, "Public Health, Preventive Medicine, and Professionalisation in Britain and the United States," in *Medicine in Society*, ed. Andrew Wear (Cambridge: Cambridge University Press, 1992), pp. 249–75.

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supplies, drainage, sewerage, and pollution controls continued to be extended and key innovations, such as the activated sludge treatment of sewage and the chlorination of water supplies, proved cost-effective. Older approaches were made to serve new purposes; for example, the arrival of the inside flush toilet connected to sewer mains continued the campaign against environmental pollution while requiring and symbolizing new standards of domestic and personal hygiene.

Historians of public health have come to argue that the new person-centered approaches came from many sources. One crucial factor was the changing pattern of urban disease, with the decline of epidemics and so-called filth diseases and an awareness of the toll of endemic diseases, such as tuberculosis and syphilis, and of social diseases such as alcoholism and feeblemindedness. There was, and continues to be, considerable debate over the causes of the decline in communicable diseases, with a growing body of opinion maintaining that sanitation and public health measures were key factors.[22] This is a departure from the previous orthodoxy that followed Thomas McKeown's claim that the major cause of mortality decline was rising standards of living, especially improved diets.

Historians are also divided over the reasons for the development of new public health and personal health services. Was it because of "pressure from below," as working-class political groupings and the extension of the franchise led governments to institute more egalitarian and progressive welfare policies? Or were reformers always pushing at a part-open door, as political and business leaders recognized the value of healthy citizens in the struggle for shares of world output and trade, in averting social unrest, and in gaining loyalty in wartime? A third argument is that public health policy ceased to be a sociopolitical issue and became the domain of experts in sanitary engineering and preventive medicine, to be shaped principally by technical rationality, pragmatism, and professional politics.

The main expression of concern over the quality of Western peoples was the eugenics movement. Although the origins of the subject lay in Francis Galton's (1822–1911) notion of a science of "good breeding," eugenics never became a fully institutionalized human science. Institutes and university departments were founded in many countries, but research proved ethically and practically difficult. In the United States and Germany, eugenists had a significant influence on social policies and specific schemes to lower the birthrate of the "unfit" and promote that of the "fit," which in Germany became more racist and murderous under the Nazi regime.[23] In many

[22] Simon Szreter, "The Importance of Social Intervention in Britain's Mortality Decline, c. 1850–1914," *Social History of Medicine*, 1 (1988), 1–37; Anne Hardy, *The Epidemic Streets: Infectious Disease and the Rise of Preventive Medicine, 1856–1900* (Oxford: Clarendon Press, 1993).

[23] Daniel J. Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (New York: Knopf, 1985); Mark B. Adams, *The Wellborn Science: Eugenics in Germany, France, Brazil and Russia* (Oxford: Oxford University Press, 1990).

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countries, there was a clash of ideologies, if not policies, between eugenists and public health professionals. The former claimed that problems such as mental deficiency and alcoholism were the result of inherited traits and that vulnerable people ought to be segregated or perhaps sterilized to prevent them from passing on their characters. The latter maintained that such problems were the result of unsanitary conditions and public ignorance of the principles of hygiene and could be remedied by providing improvements and personal health services. On practical policy, the two sides came to have much in common, not least because environmental conditions were believed to influence the degree to which an inherited trait or susceptibility might express itself. For example, a propensity toward alcoholism would not be excited if the person became a teetotaler, and someone with an inherited tubercular diathesis was advised to avoid unventilated places to protect their vulnerable lungs.

Such views are congruent with the arguments of David Armstrong and Dorothy Porter that preventive medicine after 1900 was as much concerned with behavior and social interaction as it was with disease agents.[24] Indeed, bacteriological ideas were used to support and sustain the new interests. Laboratory research and preventive experience reversed the earlier idea of a germ-ridden environment and normally germ-free human body, pointing instead to an environment that was usually relatively pathogen-free and to human and animal bodies that carried many microorganisms.[25] The vulnerability of germs to sunlight, desiccation, temperature, and predators in the environment reaffirmed older ideas of the natural cleansing of the environment. In addition, the main problems with communicable diseases now concerned small-scale epidemics and childhood infections, in which people, animals, and their wastes were implicated as the main sources of contagion. Studies of infections, particularly of typhoid fever, showed that many healthy people carried pathogenic germs; this raised a particular problem in isolation hospitals over when to discharge patients who had recovered but still harbored disease germs.

The asymptomatic infected person, the so-called disease carrier, gained international notoriety through the career of "Typhoid Mary," a catering worker named Mary Mallon, who was shown to have spread typhoid fever over many years in the northeastern United States.[26] Typhoid Mary also represented wider fears about bacterial contamination of food, especially milk as a medium for the spread of tuberculosis from cows to humans and diarrheal germs to bottle-fed babies. These problems were tackled at various

[24] David Armstrong, *The Political Anatomy of the Body* (Cambridge: Cambridge University Press, 1983); Dorothy Porter, "Biologism, Environmentalism and Public Health in Edwardian England," *Victorian Studies*, 34 (1991), 159–78.

[25] J. Andrew Mendelsohn, "The Cultures of Bacteriology: Formation and Transformation of a Science in France and Germany, 1870–1914" (unpublished PhD diss., Princeton University, 1996).

[26] Judith W. Leavitt, *Typhoid Mary: Captive of the Public's Health* (Boston: Beacon Press, 1996).

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points along the food supply chain, but a priority was to make the public responsible and promote domestic hygiene standards to improve safety at the final stage of food preparation. As an Irish immigrant, Mary Mallon also symbolized fears about germ-carrying immigrants. In the United States, it was not just worries about who was arriving from Europe but also the threat posed by emancipated African Americans moving from the southern states. The federal government established the Ellis Island complex in New York Harbor to screen European immigrants, and this was the forerunner of the first national public health agency in the country. Many other states took measures to control immigrants, which they increasingly justified on fears about the introduction of "weaker" races as well as communicable diseases.[27]

From the 1880s, bacteriological laboratories, particularly the Pasteurian institutions in France, had promised to produce vaccines that would perhaps one day allow protection against all infections.[28] The initial successes of this work were with animal diseases, but its triumphant application to rabies in the mid-1880s attracted international medical and media attention. Few new vaccines for human infections were produced in the nineteenth century, and their effectiveness was disputed. Smallpox vaccination was recast as a bacteriological procedure, even though the specific identity of the germ eluded researchers; typhoid fever and tetanus vaccines were used with certain groups, especially the military; but the major practical impact of prophylactic vaccines was in the impetus it gave to the institutionalization of bacteriology and laboratory medicine. The Pasteur Institute in Paris, which opened in 1888, was built with public and private monies raised to further antirabies work, though the greatest change came in the early 1890s with the production of diphtheria antitoxin – a curative rather than preventive product. The isolation and commercial production of natural antibacterial substances was pioneered at the Pasteur Institute in Paris and by Emil von Behring (1854–1917), who worked at Koch's Institute for Infectious Diseases in Berlin. The rush to use diphtheria antitoxin and other products for prevention, diagnosis, and treatment led to the creation of research and service laboratories. Most countries established central research laboratories but left service provision to local government, entrepreneurial doctors, academics, or laypeople.

The tension between the old public health and the new disease-centered preventive medicine was most visible in military and colonial medicine because of the professional isolation and the persistent environmentalism of doctors based in the tropics. Yet military medical men, for example Alphonse Laveran (1845–1922), Ronald Ross (1857–1932), and Walter Reed (1851–1902), made important breakthroughs against tropical fevers using the new laboratory methods. The most notable work was on the etiology of malaria,

[27] Alan M. Kraut, *Silent Travelers: Germs, Genes and the "Immigrant Menace"* (New York: Basic Books, 1994).

[28] Gerald L. Geison, *The Private Science of Louis Pasteur* (Princeton, N.J.: Princeton University Press, 1995).

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which revealed not only the specific developmental stages of its causative protozoan parasite but also the role of mosquito vectors in its transmission.[29] Through the 1900s, the parasite-vector model was successfully applied to other tropical diseases, including sleeping sickness, yellow fever, leishmaniasis, and bilharzia, and this work was consolidated and developed in the new medical specialty of tropical medicine. These developments, which attracted international political and scientific attention because of imperial ambitions and rivalries, also spawned new biological specialties – parasitology and helminthology – and changed the institutional position of the previously amateur subject of entomology. The specter of parasite-carrying insects did much to popularize germ theories of disease and to suggest that the best way to control communicable diseases was to destroy disease agents or their carriers.

The new understanding of malaria opened up new possibilities for controlling the disease and securing the health of Europeans in tropical colonies. Colonial authorities had three main control options: to kill the parasite, to kill the vector, or to break the cycle of transmission by separating the parasite from its human and insect hosts.[30] Protozoan and helminth parasites were found to be vulnerable to a variety of quinine- and arsenic-based drugs, which became the basis for the wider development of chemotherapy.[31] Vector control and transmission-breaking were quite similar approaches and remained dominant for most of the twentieth century. They ran from individual protective measures, such as drug prophylaxis, to ecological management that required the complete reshaping of environments. Individuals were advised to avoid contact with flies by wearing protective clothing and using nets, changing their lifestyles, and living in settlements segregated from the local population, who were assumed to be reservoirs of infection. The direct assault on vectors with pesticides had only limited success before the 1940s because the chemicals used and methods of delivery were inefficient. The only viable approach, which also promised a once-and-for-all solution, was "species sanitation" – to change the landscape (e.g., deforestation) or land use (e.g., drainage) or to alter the local ecology of towns so as to deny particular insect vectors the habitats they required for breeding and feeding. This approach had its most spectacular success during the construction of the Panama Canal, when General William Gorgas (1845–1920) used his military authority to introduce engineering, sanitary, and ecological methods to control both yellow fever and malaria.[32]

[29] William F. Bynum and Bernardino Fantini, eds., *Malaria and Ecosystems: Historical Aspects* (Rome: Lombardo Editore, 1994).

[30] Michael Worboys, "The Comparative History of Sleeping Sickness in East and Central Africa, 1900–1914," *History of Science*, 32 (1994), 89–102.

[31] Miles Weatherall, *In Search of a Cure: A History of Pharmaceutical Discovery* (Oxford: Oxford University Press, 1990).

[32] Marie D. Gorgas and Burton J. Hendrick, *William Crawford Gorgas: His Life and Work* (Philadelphia: Lea and Febiger, 1924).

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Judged more widely, the track record of tropical hygiene policies was quite mixed, with success depending greatly on the power of governments and experts to manage the social as well as the physical environment. Economic and political priorities ensured that control measures were concentrated in European settlements, plantations, and mines, so to a large extent the new medical sciences were "tools of Empire."[33] Economics was also the reason for the priority given to the control of hookworm, a debilitating endemic disease, which was a problem on plantations in many tropical colonies as well as in the southern United States. Attempts to control this disease were supported by the Rockefeller Foundation, which became one of the leading agencies for research and policy in public health and tropical hygiene in the second quarter of the twentieth century.[34] The foundation began working on hookworm disease in the United States in the context of rural public health, which emerged as an issue in industrialized countries as the health problems of their "backward" regions were addressed. On the international scene, the Rockefeller Foundation has been portrayed as an agency of U.S. imperialism, and its experts were among the first to investigate and try to improve the health of the indigenes of colonies, especially through yellow fever control programs and the promotion of rural public health.

There was a growing recognition in the 1930s that the health of colonial populations was poor and deteriorating with closer contact with industrialized nations. From the management of special groups in colonies, a number of problems emerged that became national and international health issues. The special diets given to prisoners and other institutionalized groups, especially in Southeast Asia, allowed the study and recognition of dietary deficiency diseases.[35] The opportunities for comparative investigations of health and diet allowed colonial experts not only to study the effects of famine on local populations but also to reveal the problems of undernutrition and malnutrition.[36] The lung problems of migrant African workers in the South African goldfields, especially pneumonia, tuberculosis, and silicosis, paralleled investigations in Europe and North America. This work helped put occupational health back on the political and medical map and underlined the continuing close links between imperial peripheries and industrial metropoles.[37]

[33] Daniel Headrick, *The Tools of Empire: Technology and European Imperialism in the Nineteenth Century* (Oxford: Oxford University Press, 1981).

[34] R. B. Fosdick, *The Story of the Rockefeller Foundation* (New Brunswick, N.J.: Transaction Publishers, 1989); Marcos Cueto, *Missionaries of Science: The Rockefeller Foundation and Latin America* (Bloomington: Indiana University Press, 1994).

[35] Kenneth Carpenter, *Beriberi, White Rice and Vitamin B: A Disease, a Cause and a Cure* (Berkeley: University of California Press, 1999).

[36] Lenore Manderson, *Sickness and the State: Health and Illness in Colonial Malaya, 1870–1940* (Cambridge: Cambridge University Press, 1996).

[37] Randall Packard, *White Plague, Black Labor* (Pietermaritzburg: University of Natal Press, 1989); David Rosner and Gerald Markowitz, *Deadly Dust: Silicosis and the Politics of Occupational Disease in Twentieth Century America* (Princeton, N.J.: Princeton University Press, 1991).

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Occupational diseases had been known for centuries, and during the nineteenth century legislation was introduced in many countries to control specific risks. However, many statutes were permissive, and the inspectorates established to monitor the problems often lacked authority and expertise. It was only in the decades after 1900 that concerted attempts were made to study the problems systematically and to define and implement national standards. These were mostly orchestrated by a new cadre of experts in occupational health who worked with and between government agencies and labor unions. In many industrial sectors, reformers worried about the overall working environment, while preventive medicine professionals tended to focus on specific diseases; for example, the effects of chemicals (such as lead and phosphorus) and the risks of dust (e.g., byssinosis in textile trades and pneumoconiosis in the mining and grinding industries).[38] Yet, in industrialized countries, occupational medicine remained within the framework of workmen's compensation legislation and questions about the responsibility for the occurrence of specific conditions. In mining, the issue was often the extent to which a particular case of silicosis was caused by the work itself, particular mine and company hygiene policies, the worker's home environment, or a family or racial susceptibility. In the first half of the twentieth century, these issues were usually decided case by case in the courts, though formal compensation schemes increasingly were introduced, administered by new medical disciplines such as industrial hygiene and occupational health.

The medicalization of public health continued to be the dominant trend in the subject until the 1940s. However, it should not be forgotten that engineers and other experts continued to operate and develop the sanitary infrastructure while the environmental causes of ill health continued to be managed as local problems – for example, urban smogs and epidemics of communicable diseases. Public health was directly affected by the wider social and political changes in welfare policies. For example, housing was reconstituted as a matter of social welfare and amenity rather directly linked to health. This transition brought conflicts, notably in food policy over whether malnutrition could be combated simply by dietary advice and food supplements or whether it would only disappear with reforms that directly tackled poverty.[39] Public health activity was criticized by two main groups. First, mainly on the Left, were those who argued that the concentration on environmental improvements and preventive medical measures had failed to address the main preventable causes of ill health, namely poverty. The second group, mainly clinical doctors, thought the best way to promote "national health"

[38] Christopher C. Sellars, *Hazards of the Job: From Industrial Disease to Environmental Health Science* (Chapel Hill: University of North Carolina Press, 1997).

[39] David F. Smith and Jim Phillips, eds., *Food, Science, Policy and Regulation in the Twentieth Century: International and Comparative Perspectives* (London: Routledge, 2000); David Arnold, "The Discovery of Malnutrition and Diet in Colonial India," *Indian Economic and Social History Review*, 31 (1994), 1–26.

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was through the growth in curative medicine, where hospitals, clinics, and general practitioner services brought patients the latest products of science and technology.[40] This trend was most evident in the 1930s in British discussions on the creation of "national health services," which were almost wholly about the reorganization of clinical provision.

## 1950–2000: WORLD HEALTH

The growing attention to global health problems after 1945 was in part a consequence of the creation of the World Health Organisation (WHO), but international cooperation on public health had begun with the Sanitary Conference in 1866 and continued in 1907 with the creation of the Office International d'Hygiène Publique (OIHP). Both organizations coordinated information on the spread of epidemic diseases and tried to develop international agreements on disease control. The Health Division of the League of Nations worked alongside the OIHP during the 1920s and 1930s, promoting standardization in reporting as well as undertaking inquiries into specific problems.[41] The WHO, which was established in June 1948, maintained the surveillance and standardization activities of the Health Division, but its Assembly and expert committees, in line with the spirit of postwar reconstruction, also developed programs to try to improve the health of nations. However, the WHO suffered from the same problems as earlier international health organizations – a lack of resources and power.

In most fields, the WHO had to work through sovereign national and local agencies, using their institutions and resources. It has had very few independent powers to impose disease control measures. This weakness was compounded by the fact that the WHO was largely run by doctors and other technical experts, who tended to focus on the medical aspects of problems, favoring technical solutions over structural ones. This is not to say that the WHO was without influence. Its concentration on poor countries with undeveloped health services and the highest mortality rates, meaning colonial and then newly independent territories, ensured that its efforts were significant when compared with the poor quality of locally provided services. Programs in these areas were largely cast in terms of "technical assistance" from first to third world; they were paternalist and tended to foster dependence rather than independence. A new problem for WHO officials was that advances in curative medicine after 1945 had given greater cultural power to the hospital and the research laboratory, to the detriment of public health and preventive medicine. Thus, political elites in third world countries often gave priority

[40] Daniel M. Fox, *Health Policies–Health Politics: The British and American Experience, 1911–1965* (Princeton, N.J.: Princeton University Press, 1986).

[41] Paul Weindling, ed., *International Health Organizations and Movements, 1918–1939* (Cambridge: Cambridge University Press, 1995).

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to the building of first world type hospitals in cities rather than improving the sanitary infrastructure or building rural health centers.

The second reason for the new interest in world health follows from the convergence of disease experiences across countries as a result of the globalization of industry and trade, tourism, and the impact of widely diffused medical technologies. This is not to deny the huge differences between the mortality and morbidity levels of first and third world countries and the equally great differences in the provision and quality of health services. Rather, it points to the growing number of common problems caused by the spread of Western lifestyles; for example, urban air pollution from motor cars, bacterial resistance to antibiotic drugs, and smoking as a cause of lung cancer. In addition, the number of global health problems increased. Faster and cheaper international travel facilitated the spread of certain communicable diseases, most notably acquired immune deficiency syndrome (AIDS).[42] The atmospheric testing of nuclear weapons raised levels of radioactivity worldwide in the 1950s and 1960s, and the radioactive material that escaped from the Chernobyl nuclear power station in 1988 spread across much of Northern Europe.

Social and medical advances also changed the age structure of populations, albeit in different ways. A key variable was changing patterns of disease. In first world countries, chronic and degenerative diseases, especially heart disease, cancers, and strokes, became the major sources of morbidity and mortality. In third world countries, infectious diseases, remained important, though rather than epidemics it was the endemic problems of malaria, respiratory diseases, and childhood infections that posed the most serious problems. In first world countries, the number of elderly people increased and produced new demands on health care services, while in third world countries reductions in infant and child mortalities led to rapid increases in population.

The foundation of the WHO was coincident with the rapid diffusion of two technologies developed during the Second World War: antibiotics and synthetic pesticides. Antibiotics, such as penicillin and streptomycin, promised to aid the control of acute infections as well as endemic problems such as yaws and respiratory infections. Cheap and effective new insecticides, such as DDT, offered experts in tropical medicine the long-sought means to kill the vectors of parasitic diseases. The development of disease control programs for third world countries based on these innovations spawned a new international medical elite plus fieldworkers in new disciplines such as malariology and applied ecology. International medical policymakers mounted what they called a "war against disease." In fact, the influence of the military went beyond rhetoric when the WHO organized "campaigns" that

[42] Virginia Berridge and Paul Strong, eds., *AIDS and Contemporary History* (Cambridge: Cambridge University Press, 1993); George C. Bond, ed., *AIDS in Africa and the Caribbean* (Oxford: Westview, 1997).

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operated with command structures and sought to eliminate diseases from whole regions.[43] In this context, disease was seen to be not just a threat to individual health but a key factor inhibiting the economic and social development of the third world.[44] This raised again the question of whether ill health was a cause of poverty or poverty a cause of ill health. For WHO experts, who only had technical means at their disposal, tackling disease and providing medical services were often their only options. However, there were always experts who argued that there were severe limitations to what medical and public health schemes could achieve, especially in malnourished populations who relied on resource-starved health systems operating in areas dislocated by wars and migration.

Postwar scientific and technological optimism fed the WHO decision in 1955 to attempt the global eradication of malaria.[45] This became the paradigmatic "vertical" control program: dealing exclusively with a single disease, self-contained in personnel and resources, and reliant on advanced imported medical technologies. The principal technology of malaria eradication was DDT spraying, backed up by prophylactic antimalarial drugs and advice on the use of screens. But after initial local successes, when quite dramatic reductions in incidence were achieved, the disease gradually reestablished itself in cleared areas and by the 1970s the policy was abandoned. The project foundered in part because malarial parasites became drug-resistant and mosquitoes acquired resistance to DDT, but there were also organizational problems. The whole enterprise gave a low priority to informing or involving local people, so little was done to build infrastructures that could continue and maintain anti-malarial measures after the "vertical" program personnel had moved on. In 1966, when hopes were still high for the malaria program, the WHO announced that it would seek to eradicate smallpox. This program succeeded in 1977. It built on long-established vaccination programs and combated a disease that was perhaps in long-term decline. The WHO had similar though less ambitious "vertical" programs for childhood immunization and the control of other communicable diseases, such as bilharzia and yaws. The influence of this approach was still evident in the 1970s when the WHO and other technical aid agencies changed tack to promote "horizontal" schemes – primary health care (PHC) dealing with health problems across the board. However, schemes were often developed as "vertical" schemes, with experts debating whether the remit of PHCs should be comprehensive or restricted to certain diseases.

[43] John Farley, *Bilharzia: A History of Imperial Tropical Medicine* (Cambridge: Cambridge University Press, 1991).

[44] Randall Packard, "Post-war Visions of Post-war Health and Development and Their Impact on Public Health Interventions in the Developing World," in *International Development and the Social Sciences: Essays in the Politics and History of Knowledge*, ed. Frederick Cooper and Randall Packard (Berkeley: University of California Press, 1997), pp. 93–115.

[45] Gordon Harrison, *Mosquitoes, Malaria and Man: A History of Hostilities since 1880* (London: John Murray, 1978).

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Pesticides were widely used in first world agriculture as well as third world disease control programs. Through the 1950s, evidence emerged of the environmental damage caused by their residues, especially when they accumulated at the end of the food chain. In 1962, in her book *Silent Spring*, Rachel Carson spelled out the long-term impact of pesticides on local, regional, and global ecosystems and the direct and indirect threat this posed to human health.[46] Carson's book was seminal to the environmental movement of the 1960s, but in terms of global health a more immediate threat was radioactive fallout from nuclear weapons testing and its potential to increase the incidence of cancer. Medical and public fears focused on atmospheric testing, which distributed fallout globally, with particular fears about the levels of certain isotopes in milk and meat. Radiation experts claimed that exposures were low and carried no risk, but the memory of the atomic bombs dropped on Hiroshima and Nagasaki during World War II and growing public anxieties about cancer raised the problem to the top of the international political agenda. Nuclear radiation was also a danger locally, to people in the Pacific and Asia, where testing had occurred at ground level, and to those working with radioactive materials. Nonetheless, political attention focused on achieving an atmospheric test ban treaty, and although this was justified by fears about the effects of low-level radiation on children and babies, its passage was shaped by wider shifts in cold war relations between the United States and the Soviet Union.[47]

More widely, environmental problems became issues in their own right, with questions of amenity and quality of life becoming as important as health risks. Paradoxically, the WHO was slow to become involved in addressing the health consequences of pollution and development in third world countries and did not work that closely with the UN's Environment Programme (UNEP) or its Food and Agriculture Organisation (FAO).

The new global public health tended to be issue based: targeted at a particular disease or responding to a specific problem. This approach was also a feature of national and local public health in first world countries after 1950, for example, with birth control, smoking, and food hygiene. In many cases, the issues were identified and promoted by lay pressure groups, a fact that reflected the professional weakness of preventive medicine and uncertainties about its role in medical systems dominated by curative services. The rapid pace of innovations in therapeutics and the extension of health services in welfare reforms had continued to marginalize preventive services within medicine. In first world countries, the combination of effective vaccines and

[46] James Whorton, *Before "Silent Spring": Pesticides and Public Health in Pre-DDT America* (Princeton, N.J.: Princeton University Press, 1974); Gino J. Marco and Robert M. Hollingworth, eds., *Silent Spring Revisited* (Washington, D.C.: American Chemical Society, 1987).

[47] Robert A. Divine, *Blowing on the Wind: The Nuclear Test Ban Debate, 1954–1960* (Oxford: Oxford University Press, 1978); Harold K. Jacobson, *Diplomats, Scientists and Politicians: The United States and the Nuclear Test* (Ann Arbor: University of Michigan Press, 1966).

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antibiotics rapidly reduced morbidity and mortality rates from communicable diseases, robbing preventive medicine of two of its enduring functions from the nineteenth century: the monitoring of infectious diseases and the management of isolation hospitals. State and pharmaceutical laboratories continued to produce more effective and safer vaccines, and major new campaigns were mounted for childhood immunization against polio, tuberculosis, measles, mumps, and rubella. Increasingly, these programs were run through school medical services, hospitals, and general practitioners rather than public health services.

Birth control was typical of the new issue-based public health.[48] It became important in both first and third world countries, in the 1960s because of the introduction of the oral contraceptive pill. The medical profession had kept its distance from birth control, in part because of its earlier links with eugenics and in part because of the religious and moral questions with which it was associated. Birth control had been promoted in first world countries by individuals such as Marie Stopes and Margaret Sanger from the 1920s, but it became much more visible in the 1960s when the control of fertility became a political and rights issue for the women's movement. The introduction of the oral contraceptive pill, while offering women more effective control of their fertility, required medical supervision and dependence on the pharmaceutical industry. In some countries, administration of the Pill was through preventive medical agencies, although in most it was provided by family practitioners, voluntary agencies, or specialist services. In third world countries, birth control was also a political issue. National and international medical agencies promoted its practice to reduce family size and hence help ameliorate problems such as malnutrition, and threats to women's health, and even engineer the reduction of overcrowding in the rapidly growing cities of Africa, South and East Asia, and South America. However, the cultural dimensions of birth control meant that medical services often faced active and passive resistance at all levels.

The most prominent issue in first world public health from the 1950s was the link between smoking and health, which became a concern in third world countries in the 1990s as the consequences of the tobacco habit began to be seen worldwide.[49] In first world countries, lay and medical pressure groups slowly persuaded governments that most lung cancer deaths were caused by smoking and hence were preventable. This produced a gradual shift from measures based on persuasion through health education to those relying on pricing and prohibition, especially as the evidence of the effects of passive inhalation of cigarette smoke mounted. It is interesting that the issue of

[48] Carl Djerassi, *The Politics of Contraception* (Stanford, Calif.: Stanford Alumni Association, 1979); Lara Marks, *Sexual Chemistry: A History of the Contraceptive Pill* (New Haven, Conn.: Yale University Press, 2001).

[49] World Health Organisation, *Tobacco or Health: A Global Status Report* (Geneva: World Health Organisation, 1997).

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preventable cancers was not further exploited by lay groups and public health professionals; chemical carcinogens are implicated in many conditions, and there is generally strong public support for screening programs. Respiratory diseases were also at the fore of new concerns about the local urban environment. Smogs, first from domestic coal burning and later car emissions, most famously in Los Angeles and Delhi, have been linked to modern epidemics of bronchitis and childhood asthma. Both were seen as diseases of modern civilization, as have conditions such as Legionnaire's disease (spread by air-conditioning systems), Listeria (a consequence of chilled food), bacterial contamination of meat and eggs (mainly in intensively reared livestock), and allergies (to all manner of synthetic materials). However, these issues proved difficult to exploit politically, producing chronic illnesses rather than death and with those affected being dispersed and difficult to organize as a pressure group. Many of the effects recognized were long-term and insidious, as in the case of smoking, where vested interests were able to obfuscate the dangers and the public proved reluctant to make immediate changes in lifestyle for long-term statistical benefits.

In other areas, long-term changes in disease patterns were used as pointers to environmental changes; for example, the rise in skin cancer rates in southern latitudes was cited as the first of many consequences that may follow ozone depletion and global warming. Other scenarios painted by the ecoepidemiologists are of tropical diseases spreading north and south, the emergence of new pathogenic viruses as ecosystems change, and the loss of potential natural drugs as biodiversity declines.

## CONCLUSION

In 1981, the WHO adopted a policy entitled "Health for All by the Year 2000," which has been associated with something called the "new" public health or the "greening" of public health and indicated a linkage with the environmental movement. However, as its definition of public health shows, it was not that new:

> The term builds on the old (especially nineteenth century) public health that struggled to tackle health hazards in the physical environment (for example by building sewers). It now includes the socio-economic environment (for example, high unemployment). 'Public health' has sometimes been used to include publicly provided personal health services such as maternal and child care. The term new public health tends to be restricted to environmental concerns and to exclude personal health services, even preventive ones such as immunisation.[50]

[50] D. Nutbeam, "Health Promotion Glossory," *Health Promotion* (1986), 122.

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There are two significant features of this characterization: the exclusion of any role in health promotion for clinical medicine and the inclusion of economic and political factors. Thus, the advocates of the new public health set an ambitious and overtly political agenda for the twenty-first century. This promises to reverse one of the main trajectories of over 150 years of public health work, namely the tendency to pursue the "art of the soluble" (scientific and technical solutions for disease prevention and health promotion) and eschew the "art of the possible" (the economic and political determinants of ill health). How public health agencies will fare on the political stage locally, nationally, and internationally is uncertain, though a key factor will be the ability of those within medicine and outside it to mobilize interest and support for public health activities. Also, much will continue to depend on the economic and social consequences of old and new diseases, on the rates of environmental change, and, of course, on the impact of changes in health status, positive and negative, on the size and age structure of populations worldwide.

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*Part II*

# ANALYSIS AND EXPERIMENTATION

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# 10

# GEOLOGY

*Mott T. Greene*

Geology is the name arrived at in the 1820s for a specific approach to the scientific study of the earth's outer layers. This new science aimed to discover and date the natural history of this three-dimensional ensemble of layered rock, to learn the origins, variety, and provenance of the rock-forming minerals that composed these layers, and to uncover and understand the natural processes and laws that shaped them. The name "geology" came into general use when the new approach it denoted had already been under way for more than a century (as is almost always the case in science). Thus, while it was still an activity without a fixed name, "geology" had already encountered several robust and preexisting competing approaches to studying the earth, each with its own proprietary interest in the phenomenon. Much of the history of geology in the nineteenth and twentieth centuries is a story of conflict and accommodation with these antecedent approaches to the study of the surface of the planet. As a result, most writing on the history of geology – and especially that produced since about 1980 – has embraced the idea that geology emerged and grew as a science through a series of great controversies.[1]

For most of its history, geology has stood in clear and marked contrast to the approaches to the earth taken by astronomy and by physical cosmology and cosmogony. The earth of nineteenth-century astronomy and scientific cosmology was a gravitationally governed and rotating spheroid. It had no

[1] See, for instance, Anthony Hallam, *Great Geological Controversies*, 2nd ed. (Oxford: Oxford University Press, 1989); David R. Oldroyd, *Thinking about the Earth: A History of Ideas in Geology* (Cambridge, Mass.: Harvard University Press, 1996). Both cover the entire period discussed here and offer bibliographical guidance and a discussion of key terms. In addition to the specialized works listed herein, see also the collection of essays *Toward a History of Geology*, ed. Cecil J. Schneer (Cambridge, Mass.: MIT Press, 1969), and Mott T. Greene, *Geology in the Nineteenth Century: Changing Views of a Changing World* (Ithaca, N.Y.: Cornell University Press, 1982). Also of interest are several essays in *Images of the Earth: Essays in the History of the Environmental Sciences*, ed. Ludmilla Jordanova and Roy Porter (Chalfont St. Giles: British Society for the History of Science, 1979). Older surveys of interest include F. D. Adams, *The Birth and Development of the Geological Sciences* (New York: Dover, 1954, reprinted); Archibald Geikie, *The Founders of Geology* (New York: Dover, 1962, reprinted); Karl von Zittel, *History of Geology and Palaeontology* (Weinheim: J. Cramer, 1962, reprinted).



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history of note other than a steady thermodynamic course from a frozen (or fiery) origin in a distant but calculable past to a fiery (or frozen) endpoint in a distant but calculable future. The earth of astronomers and physicists had always been an object of bulk properties: its shape, structure, and relief interpreted as consequences of its mass, motion, thermal regime, and proximity to other astronomical bodies. Viewed from this standpoint, the earth of geology was little more than the study of transient epiphenomena, well below the threshold of scientific interest. For geology to exist and achieve scientific status, it somehow had to give importance, coherence, and meaning to a variety of materials, structures, and processes that held virtually no interest for astronomers and physicists.

Geology, in its formative decades, was thus pressed from one side by a study of the earth compounded only of gravitational and thermodynamic generalities and was also jostled roughly on the other side by a study concerned only with the earth's most local and pragmatic details. In the early nineteenth century, when one descended from the empyrean of cosmic and astronomical interest concerning the earth and its doings, one entered a realm of technical expertise and craft lore concerning individual rocks and minerals, a region inhabited by men minutely preoccupied with discrete, local, and uncoordinated knowledge of the "subastronomical" details of the earth.

Mineral prospecting and mining; the smelting of metallic ores and the production of implements and weapons of metallic alloy; the finding, classifying, polishing, and cutting of crystals and gemstones; the quarrying and working of a great variety of rocks with different uses and properties; and the employment of minerals and mineral extracts as dyes, catalysts, pharmaceuticals, and as craft and industrial feedstocks all went back to the fourth millennium before the current era. Mineral geography and cartography, trade in metals and stones, and methods of digging, shoring, and draining shaftwork mines and open quarries have left traces and treatises in every one of the great early civilizations. All of these complex technical, economic, and engineering activities, and the kinds of knowledge about the earth and its components they contain, were already part of vigorous practical and intellectual enterprises and had to be acquired by geologists from those miners, mineralogists, mineral chemists, and craft workers who already held them. They had to be made public where there was an economic interest in secrecy and made common and uniform where localism, habit, and craft practice held sway.

Put this way, it all sounds terribly Hegelian, with geology "waiting to be born" in a dialectical struggle with its predecessors. Something rather more concrete was actually the case. The style of explanation, or approach to the study of earth, we call "geology" amounts to an extension of late Enlightenment conceptions of natural philosophy and historical explanation to the understanding of the earth and its component phenomena. It is, with regard to the mineral and stony surfaces of the earth, the result of the

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"temporalization of the chain of being," to use the phrase of Arthur Lovejoy. Rather than arranging the phenomena of the world in some sort of order of ascending complexity and vitality – a chain of being from the most inert and homogeneous rocks up through all the variety of creation to mankind and the angels above – Enlightenment natural history in the eighteenth century increasingly moved to arrange things in terms of their sequential appearance in historical time and consequently portrayed the world and its life as an emerging and often-modified order and structure. The Comte de Buffon's *Epochs de la nature* (Epochs of Nature, 1778), for instance, gave an age to the earth of many tens of thousands of years and left biblical time, the Ark, and a static, perfected creation far behind and started down the road to a detailed natural history of the world. "Geology" means and has always meant to explain what the earth is by telling the detailed historical story of how it came to be structured and ordered in the way we see it and then interpreting the details of this history, passively or actively, in terms of the natural causes, laws, and processes that drive it.

Thus characterized, geology arrived on the intellectual scene of early modernism at the beginning of the nineteenth century along with strong preferences for the historical mode of explanation in understanding politics and arts, religions and sciences, cultures, nations, and states. Geology was pursued by scientists who had an interest in the details of material nature that physicists and astronomers found trivial and an interest in generalization and general principles foreign both to mining practice and craft mineralogy. Geology came into existence as a distinct intellectual and scientific force by producing, out of these intermediate interests, results that eventually became compelling and useful to both antecedent groups. Not only that, but the historical picture that geology produced of the evolution of earth and life became rapidly and pervasively influential outside the bounds of the natural sciences. This "worldview," in the most literal sense of that term, served as the evidentiary foundation for a new master narrative of human life, human nature, and human history. Geology has in the last two hundred years – perhaps more by its patient, empirical grinding than by any brilliance of conception – brought about a change in the way humans see themselves and their universe as great and profound as any transmitted to philosophy by fundamental physics.

That geology consisted in discovering and telling the historical details of the shaping of the earth and its component parts and inhabitants under the aegis of physical laws made this new program of study a clear competitor to yet another group of thinkers and doers with a prior vested interest: natural theologians and the authors of "sacred" histories of the earth. These historians viewed the earth as an object created by God within the last few thousand years to serve as an abode for man and as an arena for the drama of sin and redemption. The study of this sacred earth, with which the new approach called geology had to compete, aimed to uncover and document the empirical

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natural remains of the history told in the Hebrew scriptures, including such events as Noah's Flood. It also aimed, by a study of earth's surface processes, to exhibit evidence of continuing divine interventions, both benevolent and punishing, in various aspects of the order of nature. Geology was faced with the necessity of offering completely naturalistic explanations for phenomena already given a supernatural cause and purpose in a broad range of philosophical theologies; consequently it faced a vigorous and significant opposition from the exponents and defenders of these earlier histories.

Geology eventually made peace and even common cause with mining on the one hand and astronomy and cosmology on the other by linking the knowledge of both groups in a new pattern and on a new scale – the planetary surface in *all* its detail and dynamic relations – in a way that interested both groups without contradicting their schemes and practices. But the obvious historical and logical relations between the sacred and secular versions of earth history, with the latter progressively supplanting the former in substituting natural for supernatural causation in place after place and instance after instance, allowed no ready accommodation.

Within the scientific community of geological investigators in Europe and North America, the idea of a young earth, created almost instantaneously and fully formed, and inhabited from the start by its current denizens, was already passing rapidly away in the 1830s. The story of this great encounter between scripture and stratigraphy is compellingly presented in Charles C. Gillispie's *Genesis and Geology*, still the best work on the topic a half century after it was written.[2] Later commentators have had to recognize, however, that Gillispie and his contemporaries focused their attention on those aspects of the subject that most reflected the tension with religion, at the expense of other issues that had far greater significance for the development of geological science. This is most obvious in the case of the "uniformitarian–catastrophist" debate (discussed later), which was active in the English-speaking world but for which there was no real equivalent in continental Europe. Several modern studies have argued that disagreements over the rate of geological change did not necessarily have the major theoretical significance once attributed to them – however much they were highlighted by those seeking to attack or defend the view that the last catastrophe might have been Noah's Flood.

The eventual truce between revealed religion and geology within the bounds of the scientific community must not, however, be confused with a sudden or lasting victory for an agnostic or atheistic naturalism, with which it was by no means identical. This was especially true when the geological record of former life came to be considered in detail in the middle and latter parts of the nineteenth century. Moreover, though the marginalization of sacred history of the earth – especially with regard to life and the doctrine

[2] Charles C. Gillispie, *Genesis and Geology: A Study in the Relations of Scientific Thought, Natural Theology and Social Opinion in Great Britain, 1790–1850* (New York: Harper, 1959).

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of organic evolution – was largely complete *within* science by the end of the nineteenth century, the controversy at the level of popular understanding was still joined at the end of the twentieth. In North America, the remaining exponents of such a sacred history are still powerful enough to launch campaigns to return accounts of divine creation to public school curricula and to press to eliminate the study of geological and biological evolution from these same curricula.

The close study of the history of science, under way for more than a century, leads us to understand "science" not only as a series of empirical truths and theoretical explanations obtained by scientists studying nature (though it is that) but as the complex activity of scientists and sciences operating in larger philosophical, social, political, and economic contexts. This is true for geology in all periods whether we consider philosophy and religion, the economic importance of earth materials and processes, or the shaping effect of political conceptions of national interest and national defense on what governments will pay geologists to study. All of these phenomena are as surely a part of the history of geology as the rock hammer and the hand lens, or the microscope and the scintillation counter, and will play a role in the narrative that follows.

## STRATIGRAPHY: THE BASIC ACTIVITY OF GEOLOGY

From the beginnings of geology down to the very recent past, geologists have concentrated overwhelmingly on compiling a three-dimensional picture of the earth's continental surface features, expressed in detailed maps and accompanying explanatory texts. Using long-evolved and laboriously negotiated conventions of geological cartography, these maps depict and describe the successions of layers of sedimentary rock, or *strata*, of which the earth's visible surface and outer crust is largely composed; thus the name *stratigraphy* – literally, the drawing of strata. These strata, often vast in lateral extent and stacked in sequences tens of kilometers in thickness, are the fundamental subject matter of geology. This activity of geology has been to name and measure every stratum of every sequence on earth, to detail its component minerals, and to reconstruct the story of its formation, its existence, and in many cases its deformation and destruction. The ensemble of life histories of these layers has been compiled into a massive and total history of the earth's surface features and is a triumph of intellectual attention to singularity unequaled in the history of human thought.

There are classes of rocks that geologists study other than those that appear in stratigraphic layers. The stratigraphic rocks are composed of sand, mud, calcium carbonate, and other material – granular substances, coarse and fine, that sank (particle by particle) to the bottom of a sea or were carried by a stream or blown by wind to places where they could be buried and hardened

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and later exposed and eroded again. In addition to these *sedimentary* rocks, there are also *igneous* rocks, which owe their existence either to cooling from a molten state or to ejection as ash or cinder from a volcanic vent or fissure. There are also the *metamorphic* rocks, so altered by heat and pressure from their original state as to require new names. These are both of enormous importance and interest to geologists, but the principal activity of geology has still been to study sequences of strata.

The primacy of this stratigraphic activity is well documented in the history of geological work at the beginning of the century. One might well begin with Abraham Werner (1749–1817), a professor of mineralogy at a state mining academy in Freiberg, Germany, at the turn of the nineteenth century. Werner taught field technique and mineral identification to a generation of students who spread out all over the world to test, and later to radically modify, Werner's ideas of the sequence of rocks making up the earth's crust. One might also single out the French geologist and vertebrate paleontologist Georges Cuvier (1769–1832), who with his coworker Alexandre Brongniart (1778–1847) published the *Essay on the Mineralogical Geography of Paris* (1810), which documented the sequences of strata and their fossil contents in the great basin around Paris. In Britain, the great Scots geologist James Hutton (1726–1797) rescued a nearly extinct tradition of analysis of landforms and combined it with a Newtonian picture of a dynamic earth driven by the earth's internal heat, its surface built up and eroded away again and again over limitless spans of time. His emphasis on the primacy of the erosion cycle had a determinative influence on the practice of geology in the English-speaking world. William Smith (1769–1839), the pioneer British stratigrapher, was already producing stratigraphic maps of impressive accuracy before 1820. In short, the primary activity got under way at the same time in all the metropolitan high cultures and scholarly languages of Western Europe.[3]

The great controversies that dominated geology in Britain in the middle of the nineteenth century and markedly influenced the thinking of geologists everywhere in the world at this time were almost without exception about the extent and character of great sequences of rocks in England, Scotland, and Wales. Henry De la Beche (1796–1855), Roderick Murchison (1792–1871), Adam Sedgwick (1785–1873), Charles Lyell (1797–1875), and the other gentleman-scientists who founded the Geological Society of London, directed the government Geological Survey, and held the first professorships of geology in universities cooperated and competed with one another to map and name the great periods of earth history by documenting their sequences. The names they gave to the great groups of strata they mapped – Cambrian, Silurian, Devonian – remain in use today as abstract designations of rocks

[3] See Rachel Laudan, *From Mineralogy to Geology: The Foundations of a Science, 1650–1830* (Chicago: University of Chicago Press, 1987), and the early chapters of Greene, *Geology in the Nineteenth Century*. On Hutton, see Dennis R. Dean, *James Hutton and the History of Geology* (Ithaca, N.Y.: Cornell University Press, 1992).

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of a certain age all over the world, even where these have nothing to do with the Roman province (Cumbria), the Welsh tribe (Silurii), or British county (Devon) that gave them their names.

The mapping of the strata of Britain was carried on, as the preceding discussion suggests in a spirit of competition and controversy as well as cooperation. Science is after all a system of coordinated competition, with prizes and awards of money, fame, and position going to the most successful discoverers and inventors of things, and no part of modern science shows this with greater clarity than geology. Geologists come to have a proprietary interest in "their rocks" and take umbrage if others work, uninvited and unannounced, in their field areas. Roderick Murchison, the great student of the Silurian System, used imperial, military, and royal metaphors to describe his work – his Silurian "kingdom," his "battles and campaigns," his role as "king of Siluria." He fought with Henry De la Beche and Adam Sedgwick over priority of discovery and other matters. These debates have been well chronicled by James Secord, Martin Rudwick, and David Oldroyd.[4] These great Victorian controversies are a good indication of the "basic activity of geology."

The techniques were simple but the work exacting and arduous. Strata are rarely found uniformly exposed, and unraveling the stratigraphic history of a region means connecting together what one has seen in an outcrop here and an outcrop there, often many miles apart. One collected specimens of each stratum by hitting them with a rock hammer (in fact, a colloquial name for a field excursion was "to go hammering"). Back at home, the mineral and fossil contents could be minutely identified and used as criteria for still further correlation. One drew a sketch of the outcrop and labeled the individual strata. One tried to pinpoint the location, a task made easier as the geographic survey maps became more precise, and to determine the angle of dip of the strata with a clinometer and their orientation with a magnetic compass. A hand lens, a sample bag, and stout boots completed the scientific kit. From the results of many such excursions, a field report of local extent could be prepared to be integrated with a regional or larger report, where one existed.

The scientists were aided in this work by local residents knowledgeable about natural history and mineralogy, by farmers, quarrymen, and miners, and by professional fossil collectors. Charles Lyell, perhaps the best-known name in nineteenth-century geology, was nicknamed "the pump" for his

[4] James A. Secord, *Controversy in Victorian Geology: The Cambrian-Silurian Dispute* (Princeton, N.J.: Princeton University Press, 1986); Martin J. S. Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985); Martin J. S. Rudwick, *Worlds before Adam: The Reconstruction of Geohistory in the Age of Reform* (Chicago: University of Chicago Press, 2008); David R. Oldroyd, *The Highlands Controversy: Constructing Geological Knowledge through Fieldwork in Nineteenth-Century Britain* (Chicago: University of Chicago Press, 1990). See also Robert A. Stafford, *Scientist of Empire: Sir R. I. Murchison, Scientific Exploration and Victorian Imperialism* (Cambridge: Cambridge University Press, 1989).

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assiduous pursuit of what others knew, but his style of work was common to all the great stratigraphers, who were trying to amass and coordinate what was already known as well as discovering what was not known. We are reminded here as elsewhere in science not to put too much reliance on a famous name. Closer inspection generally reveals that the award of priority of discovery to an individual is at best an iconic representation for the work of a more or less extensive community, which reaches a summation of sorts in the work of a single name or group of names. We know this cascade of influences by citation conventions in scientific publications once a science is established, but these do not reveal (and these metropolitan scientists were often reluctant to admit) the extent to which these authors depended on others.

The development of the petrographic microscope in the 1860s and the subsequent study of rocks in thin section to determine their history by inference from mineral composition and crystalline structure opened a huge field of study whereby massive and crystalline rocks, volcanic rocks, and metamorphic rocks (characteristically altered by heat and pressure) could now be seen as a part of geology (rather than mineralogy) and brought into the master narrative of the history of the earth and melded with this stratigraphy.

## MOUNTAINS AND MOVEMENT

The pursuit of the basic activity of geology, stratigraphy, is difficult enough in its own right but is made even more difficult and complicated by the dynamic motion of the earth's crust. On short and long timescales, sections of the earth's crust rise and sink, and they also break, tear, and rift. On long timescales they also fold, extrude, thrust, and deform. This dynamic activity, in combination with the destructive action of running water and wind, increases the unevenness of the surface of the earth above the level of the sea – its *relief.* But this unevenness of the earth's surface also tells a tale concerning the *structure* of its outer layers, and nowhere more than in mountain ranges.[5]

The origin of mountain ranges has always been one of the great questions of geology and has been pursued continually from its origins. Why is it that mountains are not dotted randomly across the landscape like the stars in the sky? Why do they so often occur in "ranges" with long axes that may extend hundreds or thousands of miles? Why do they often have a core of crystalline rocks visible at the summit, flanked by sedimentary strata, that are sometimes symmetrical and even symmetrically folded? Why do some mountain ranges, such as the Appalachians, the Rockies, and the Andes, run parallel to the coast of a continent, whereas others, such as the Alps, run transversely across the

[5] For a detailed history of the topics in this section, see Greene, *Geology in the Nineteenth Century.* An older study of the British contributions is G. J. Davies, *The Earth in Decay: A History of British Geomorphology, 1578 to 1878* (New York: Science History Publications, 1969).

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middle of a continent or like the Highlands of Scotland disappear into the sea?

Whereas British geologists made great headway in mapping flat-lying or tilted sequences of strata, the French, the Swiss, the Austrians, the Germans, and the North Americans led the way in the study of mountains. They did so because they had to. Large areas within their national boundaries are dominated by high and complexly folded mountain ranges, with cores of crystalline rock, giving no easy answer to the question of their origin or age. Such eighteenth-century pioneer workers as Peter Simon Pallas (1741–1811) in the Urals of Russia and H. B. de Saussure (1740–1799) in the Swiss Alps were followed by others who devoted their lives to mapping the complex structure and unraveling the history of individual mountain ranges. Arnold Escher (1807–1872) in the Alps, Jules Thurmann (1804–1855) in the Jura, and William Rogers (1804–1882) and Henry Darwin Rogers (1808–1866) in the Appalachians are examples of such workers.

To study a mountain range, one walks up and over it again and again at right angles to the long axis of the chain, hammering, sampling, and mapping transverse sections at intervals along its length. In this way, one builds up a three-dimensional picture of the chain as a whole and tries to unravel from this picture a view of the area before the mountains were lifted up. This puzzle-solving activity may be imagined by analogy with a pile of richly patterned quilts that have been rumpled, wrinkled, and folded and then cut repeatedly with scissors to remove large sections. The puzzle is to discover, without being able to move or physically unfold the quilts, their original size and the details of their patterns before they were crumpled and cut. It is exhilarating, dangerous, isolated, and often very hot or very cold work, of very great particularity, and it has always been one of the principal attractions of going into geology as a field of scientific study.

By the middle of the nineteenth century, enough was known of a few dozen prominent mountain ranges that one could study them comparatively and divide them into fold mountains, (block) fault mountains, and a few other basic types. General theories of mountain uplift were numerous and varied. Leopold von Buch (1774–1853), for instance, studied the mountains of Italy, Germany, France, and Scandinavia and argued that mountains were created by extremely rapid and violent volcanic uplifts, creating either a "crater of elevation" (such as Vesuvius) or a mountain chain, with a volcanic rift along the long axis. Léonce Élie de Beaumont (1798–1874) thought that mountain ranges represented zones of structural weakness in the crust of the earth as it repeatedly collapsed around a cooling and shrinking interior; he believed that all mountain ranges that made the same angle with the equator were of the same age and that all mountains made up a series of sides of huge pentagons across the face of the earth. Like von Buch, he believed that the episodes of mountain building were catastrophic, presenting the greater violence of past events as a consequence of the gradual decline in the earth's central heat.

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The belief that past earth movements were on a catastrophic scale had also been supported by Georges Cuvier, who noted both the abrupt transitions between the fossil populations in successive strata and the evidence of unusual geological activity in the recent past. The latter – erratic boulders and deposits of "boulder clay" – were later attributed to the ice age but were at first thought of as evidence of a great flood. Some of Cuvier's English followers, including William Buckland (1784–1856), at first identified this last catastrophe with Noah's Flood. Charles Lyell challenged this whole "catastrophist" perspective in his *Principles of Geology* (1830–3), arguing that all earth movements were slow and gradual on the same scale as modern earthquakes. He linked this to the Huttonian vision of history, in which erosion and elevation were balanced in an eternal cycle. Lyell's arguments were methodological, based on the claim that only by concentrating on obervable causes could geology become truly scientific. The resulting "uniformitarian–catastrophist" debate has attracted much attention because it symbolizes the conflict between the new science and the old biblical tradition. Earlier histories tended to dismiss catastrophism as unscientific, but several studies have shown how it formed a coherent and sensible program, especially when linked by Élie de Beaumont and others into the prevailing vision of an earth that was cooling down and not, as Lyell claimed, in a steady state.[6] Later histories have tended to play down the significance of the debate. Many of the stratigraphical debates were conducted almost independently of the disagreement over the rate of change. More seriously, however, continental European geologists remained almost untouched by the Lyellian perspective, remaining wedded to a vision of the earth as a planet that had changed significantly as it cooled down and in which change was more likely to be episodic (if not actually catastrophic) rather than uniform. In the English-speaking world, however, Lyell did have an impact on the wider reading public because his emphasis on the vast extent of geological time brought home to everyone the need to rethink the old Mosaic vision of earth history. He also, of course, influenced Charles Darwin.

By the last quarter of the century, the outlines of a narrative began to emerge into which most of these efforts could be fitted to some degree. Most geologists were willing to see an earth history in which, over long periods, the continental surfaces were being eroded away. The erosion products were

[6] For the older interpretation, see Gillispie, *Genesis and Geology*. For a more positive view of catastrophism, see Martin J. S. Rudwick, "Uniformity and Progress: Reflections on the Structure of Geological Theory in the Age of Lyell," in *Perspective in the History of Science and Technology*, ed. Duane H. D. Roller (Norman: University of Oklahoma Press, 1971), pp. 209–27. See also Martin J. S. Rudwick, *Bursting the Limits of Time: The Reconstruction of Geohistory in the Age of Revolution* (Chicago: University of Chicago Press, 2005). On Buckland, see Nicolaas A. Rupke, *The Great Chain of History: William Buckland and the English School of Geology (1815–1849)* (Oxford: Oxford University Press, 1983). On Lyell, see Leonard G. Wilson, *Charles Lyell, The Years to 1841: The Revolution in Geology* (New Haven, Conn.: Yale University Press, 1971), and Rudwick's introduction to the reprint of Lyell's *Principles of Geology*, 3 vols. (Chicago: University of Chicago Press, 1990–1).

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deposited as sediments in offshore basins at the continental margins. As these marginal basins subsided, the thicknesses of strata grew and grew. Gradually, unless renewed by mountain-building activity, the continents would be worn down to a point where they could be inundated by the oceans. At that point, the uplift of marginal basins (by a variety of entirely hypothetical mechanisms) would create new mountain ranges that deformed and folded as they rose. These, in turn, eroded seaward to create even larger continents growing around a primeval core. It appeared also that there were distinct periods in earth history when mountain building took place worldwide and periods in which there was little such activity.

This theory, called the *geosynclinal* theory because of its emphasis on the downward inflection of the sedimentary basins, took many forms, but it served as a rough unifying principle from the 1870s through about 1960. It gave a plausible account of why there were marine fossils in high mountain ranges and in deep continental interiors hundreds of miles from the ocean. It acknowledged the stratigraphic primacy of erosion and sedimentation. It made room for cycles and periodic phenomena and gave a vocabulary that could be used on every continent. Its only serious challenger before the 1920s was the theory of the Austrian geologist Eduard Suess (1831–1914) put forth in his four-volume synthesis *The Face of the Earth* (1883–1909). Suess collected everything known geologically about the earth and gave it an integrated presentation in a single work. As a description of the earth, it has few equals in the history of geological literature, but it also unfolded as a cosmic drama with a tragic finale. It was a theory of sedimentary basins and of rising and falling sea levels, and therefore of alternation of land and sea, but it had the added wrinkle that the oceans were seen to be growing at the expense of the continents by the occasional and slow foundering of huge continental blocks; in a distant future, the earth would be a water planet covered by a "panthalassa," or worldwide ocean.

Most geological schemes at the end of the nineteenth century gave great play to the ability of large tracts of continental surface to crumple and shorten or to be thrust, without disintegrating, over scores of kilometers. The physical processes that might have caused these structures were difficult to imagine, but the geological evidence was overwhelming and convincing. Charles Lapworth (1842–1920) in the Highlands of Scotland and Albert Heim (1849–1937) in the Swiss Alps demonstrated such huge overthrusts. By 1903, the French geologist Pierre Termier (1859–1930) was able to announce the stunning discovery that the difference between the eastern and western parts of the Alps, always puzzling, was the result of the eastern Alps overthrusting the western – that there was a place in the eastern Alps where the entire thickness of the western Alps could be seen exposed in a "window."

The science of geology was certainly, at the end of the century, entering a triumphant phase. There was a near-universal sense that the mode of work, level of theoretical depth, and quality of results guaranteed the continued

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independence and growth, and not the mere survival, of the enterprise. The major outlines and relief of the earth's surface were verified and mapped, and geological mapping even of remote regions was under way on every continent. The phenomena of geology were being investigated at every level from the microscopic to the global.

## ICE AGES AND SECULAR COOLING OF THE EARTH

In the last quarter of the nineteenth century, geology acquired a number of additional subjects and divisions. Important among them were glacial geology and geomorphology, with the firm establishment by 1875 of the theory of the ice ages in both Europe and North America. Large tracts of the Northern Hemisphere above about 50° N latitude, but often much farther south, are covered with thick deposits of gravel, sand, clay, and loose rock. Large portions of Canada and Scandinavia are bare rock, with topsoil entirely scoured away and the rock deeply cut and striated. Across North America and the North European Plain, the landscape is littered with great erratic boulders, geologically unrelated to anything within hundreds of miles. Valleys are shaped like the letter "U" rather than the letter "V." Many hillsides have successions of large exposed terraces, as if of former lake shorelines. In sacred histories of the earth, of the kind geology battled early in the nineteenth century, these were taken to be the remnants of the Great Deluge of Noah. By mid-century, the favored explanation was that this loose material had been rafted by icebergs and then dropped in the last alternation of land and sea – an argument by analogy with the ability of alpine glaciers to carry rocks great distances and for icebergs calving off Greenland to do the same.

The Swiss naturalist Louis Agassiz (1807–1873) argued in 1840 and after that the ensemble of phenomena were best explained by the hypothesis that large tracts of the Northern Hemisphere had been covered – and not too long ago – by huge thicknesses of ice. This interpretation gained ground, championed by Scandinavian geologists such as Otto Torrell (1828–1900) and Gerard De Geer (1858–1943) and the Germans Albrecht Penck (1858–1945) and Eduard Brückner (1862–1927), among others. By the 1880s, decisive evidence was available for not just a single glaciation but repeated advances and retreats of the ice sheets, their borders mapped in detail by the terminal moraines of debris they left behind. By the 1880s, there was also significant evidence accumulating that large areas of South Africa, India, and even Australia had also, in a much earlier period, been covered by ice sheets.[7]

These findings were remarkable in themselves but had enormous implications for the relationship of geology to physics. For the great majority of

[7] See Hallam, *Great Geological Controversies*, chap. 4; Oldroyd, *Thinking about the Earth*, chap. 7; Davies, *Earth in Decay*, chap. 8.

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working geologists, this relationship was distant, diffident, and only moderately consequential. There were always a few theorists who tried to relate the larger questions of geology to some physical processes. These "dynamical geologists," to the very end of the nineteenth century, generally created narratives of earth history compatible with the thermodynamic picture of a globe cooling from an incandescent nebula. There was an alternative hypothesis, that of the American geologist T. C. Chamberlin (1843–1928), that the earth had formed by accretion of cold dark matter, but even this idea had the earth warming until it melted by gravitational contraction and then cooling slowly thereafter. This vision of a long-term and irreversible cooling of the earth got strong support from the stratigraphic record: the presence of reef limestones in high latitudes, evaporites (salt and gypsum), and massive sandstones indicated that through most of history the earth had been warmer than at present.

The evidence of successive glaciations in the Northern Hemisphere and the possibility of an ancient ice age in the Southern Hemisphere was not compatible with a slowly cooling earth. The oscillations of the climate had in fact been suggested by the theory of James Croll (1821–1890) based on astronomical variations influencing the earth's orbit around the sun.[8] But the fact that fieldwork had confirmed a theory incompatible with the physicists' model of the cooling earth created no panic among geologists. Rather, the reverse was true, and there was a growing sense that physics could not override the evidence of geology. This fed additional fuel to the already bright fire of scientific self-esteem geologists had begun to feel. Geology, by the patient accumulation of empirical data, was now capable of global theories of its own.

## AGE AND INTERNAL STRUCTURE OF THE EARTH

At the very moment of these triumphant declarations of independence and scientific maturity, geology was transformed in the first decade of the twentieth century by the emergence of three fields of study, appearing in rapid succession: radiometric dating, seismology, and gravimetric geodesy. All of these assumed great importance by about 1910, despite having been virtually unknown in 1900 outside small communities of subspecialists.

The discovery of radioactivity, and that radioactive substances were abundantly distributed in the crust of the earth had two immediate consequences. The first was to throw overboard all calculations of a cooling earth as a "motor" for earth history because the heat generated by radioactivity provided a constantly renewed antidote to long-term cooling. The second, and by far the

[8] See Christopher Hamlin, "James Geikie, James Croll and the Eventful Ice Age," *Annals of Science*, 39 (1982), 565–83.

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most consequential, was the discovery of the first means of giving reliable *absolute* dates to the earth and its strata by measuring the decay of uranium into lead.

It comes as something of a shock to realize that until almost the First World War, the age of the earth was not known at all and could only be estimated indirectly by assumptions about cooling or by measuring the rate of sedimentation in river deltas. The former technique was an astronomical deduction and the latter an extrapolation from current rates of sedimentation to the whole thickness of deposited sediment in the geological record. The result was a wild range of absolute dates, bridging more than two orders of magnitude. There were serious claims that the earth was less than 10 million years old, though most estimates were somewhere between 100 and 600 million years, and a few ranged above a billion years. That the answer was most certainly more than a billion years was stunning and provided a tremendous influence on cosmology – flowing from geology back to physics and astronomy. The story of this great struggle over the age of the earth and its implication for geology has been told by Joe Burchfield.[9]

For the next half-century after the discovery of radiometric dating, the age of the earth "grew" as more artful and exact techniques were applied, perhaps most notably by Arthur Holmes (1890–1965) and Clair Patterson (1922–1995).[10] The latter's 1953 date of 4.5 billion years is the generally accepted figure. Even very early in this field of study, it was possible to date the extent of the various stratigraphic periods, and this gave a sense of precision and clarity to what had been relative and vague. But much more importantly for the intellectual role of geology in general culture, it connected geology to humanity as *history* – as an unbroken and datable past. There had not just been a "Jurassic period," with dinosaurs and a variety of plants and animals, but a Jurassic period that had lasted for 69 million years, beginning 213 million years before the present and ending 144 million years ago. It could be globally subdivided into three epochs and further subdivided into eleven ages, each with different physical and climate conditions deduced from stratigraphy and paleontological remains. The age of the other known periods of stratigraphy could also be established, but they were now seen to comprise but a small fraction of the earth's overall history.

The development of seismology, the study of the transmission of wave-like disturbances (generated by earthquakes) through the body of the earth, had less popular impact outside geology but was as consequential within it. Seismology not only provided direct information on earthquake dynamics but gave a picture of the earth's deep interior. By analysis of the wave

9 Joe D. Burchfield, *Lord Kelvin and the Age of the Earth* (New York: Science History Publications, 1975) and Patrick Wyse-Jackson, *The Chronologers' Quest: The Search for the Age of the Earth* (Cambridge: Cambridge University Press, 2006).

10 On Holmes, see Cherry Lewis, *The Dating Game: One Man's Search for the Age of the Earth* (Cambridge: Cambridge University Press, 2000).

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forms, changes in velocity, and total travel times of earthquake waves from the originating earthquake to networks of recording instruments around the world, it became possible to "see" the deep interior of the earth and to draw a picture of its internal layering. Already by 1909, Andrija Mohorovicic (1857–1936) had established that there was a discontinuity between the earth's mantle and crust at a depth of a few tens of kilometers. Further work by Beno Gutenberg (1889–1960) and others showed deep boundaries between the mantle and a multilayered core, part solid and part fluid (see Oldroyd, Chapter 21, this volume).

Gravimetric geodesy, the mapping of the absolute value of gravity at various points on the earth's surface and its comparison with calculated values, gave another means to make inferences about the earth's interior. The American Clarence Dutton (1841–1912), who had helped map the Grand Canyon, became curious about why the earth, given its size and age, was not as smooth as a billiard ball. He wondered what preserved the elevation of portions of the earth against the wearing of erosion, which, cooperating with gravity, should long ago have rendered it flat and smooth. He decided that one answer would be that the crust of the earth might float on material below that possessed no strength – that it might actually be buoyant. There was some gravity data from the nineteenth century to support this view, but partly inspired by Dutton's conjecture, a great survey of the gravity field of the United States, completed in 1909, seemed to indicate that the crust was substantially lighter than the interior and floating on it. This led to great modifications in the theory of the earth's dynamic behavior over the next few decades: Along with radioactivity and seismology, this principle of *isostasy*, as Dutton had called it, played an important role in the theory of continental drift, proposed by Alfred Wegener (1880–1930) in 1912, and thereafter. Wegener, a young atmospheric physicist just out of graduate school, grasped that with the earth deprived of strength at so shallow a depth, and heated by radioactive decay, it was possible that much of geological activity could be seen as a consequence of the splitting and drifting apart of great continental fragments and many puzzling questions of geology thus answered (see Frankel, Chapter 20, this volume).

## ECONOMIC GEOLOGY

Radioactivity, seismology, and gravity measurement penetrated geology rapidly, at first for their theoretical interest. But the latter two were immediately recognized as powerful tools in "geophysical prospecting." Seismological recording of the reflection of waves generated by explosions was and is a powerful means of locating deposits of oil and natural gas. Gravity measurements allowed one to prospect for subterranean ore bodies by mapping local variations in absolute gravity. Long before the study of the earth's magnetic field played an important role in the theory of plate tectonics, prospecting for

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iron and nickel ore with sensitive magnetometers was a universal geological practice. This rapid and successful exploitation of these techniques (the best pendulum gravimeter before 1930 was invented by scientists working for the Gulf Oil Company) allows us to pause and reflect in general terms on the extent to which geology has been driven by economic considerations (see also Lucier, Chapter 7, this volume).

The worldwide search for economically exploitable deposits was the driving force behind much of the geological exploration at the end of the last century and behind one of the greatest and most geologically useful works of the twentieth century, albeit one rarely mentioned by historians of geology. It is the *Handbook of Regional Geology* (1905 to about 1920), a massive multiauthor, multinational enterprise, under German editorship, that surveyed the entire world. As an example of the sort of coverage it had, one might look at Max Blanckenhorn's *Syria, Arabia, Mesopotamia* (1914), appearing as Heft 17 (Volume 5, Part 4) of this series. Following the pattern for all the volumes, it began with a "morphological overview," then went to a stratigraphic history, a history of structural events and mountain building, a history of eruptive rocks, and then a survey of economically useful deposits. In 159 pages, one could read a summary of everything known geologically about this part of the world, including a bibliography right up to the year of publication. A similar volume appeared for every major continent and region, not excluding inner Asia, Greenland, and Antarctica, some of the last places to be visited and studied. Also in this category of work, inspired equally by scientific curiosity and the hope of economic gain, was Franz Lotze's *Rock Salt and Potassium Salt Geology* (1938), a very large tome appearing as Volume 3, Part 1 of the series Geology of the Non-Metallic Minerals and characteristic of a huge body of literature devoted to the location and extraction of mineral ores.

If economic geology and the pursuit of ores and petroleum products had a profound effect on the direction of much geological literature, it also influenced theoretical debate. The most famous symposium ever held on continental drift was organized in 1926 for the New York meeting of the American Association of Petroleum Geologists by a Dutch petroleum geologist who was a vice president of the Marland Oil Company in Tulsa, Oklahoma. He understood that if continental drift were a fact, one could locate offshore oil deposits by using continental reconstructions matching coastlines to link a known deposit on one continent to an as yet undiscovered one on another.

## GEOLOGY IN THE TWENTIETH CENTURY

In the nineteenth and twentieth centuries, geology developed a three-part structure of university and academic geology, economic and industrial geology, and the geology of state, national, and imperial geological surveys. In practice, most geologists wore more than one hat: An academic geologist

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might begin his career looking for oil, gypsum, gold, or any other economic mineral and only then pursue an advanced degree leading to a teaching job. Most geologists since the latter part of the nineteenth century have worked entirely outside academia; they went to work for mining and mineral firms and stayed there most or all of their careers. Government surveys did and do have career geologists in their service, but it has been common everywhere for there to be tremendous overlap between academic and survey employment.

Most sciences have something like this – there are academic, industrial, and government chemists, for instance. But the national geological surveys give it a special twist: It is entirely unremarkable to see a book entitled *Geology of Canada*, but it would be very strange to see a book on *Chemistry and Physics of Canada*. That geology is a science that pulls up sharply at political borders is an anomaly that has profoundly affected its development. The generous and cooperative spirit of the period before World War I was not reconstituted until after World War II. Interwar geology tended to be nationalist, inward looking, suspicious, and monoglot. Whereas German-language citations in U.S. geological literature had been as high as 50 percent before the First World War, by the late 1920s they were below 5 percent, and never rose above that level again. The breakup of the Austro-Hungarian Empire gave a boost to the geology of Poland, Hungary, and Austria but restricted the scope of the work and the impulse to correlate over long distances. The breakup of the great European empires and the loss of their holdings in Africa and Asia had a similar effect. The resulting lack of cooperation and exchange across language communities has had a tremendously retarding effect on general theory and to this day has left the science very sensitive to political disruption and ideological division. One may recall that the much vaunted "revolution in the earth sciences" of the early 1970s did not include any Russian or "Soviet Bloc" geologists (more than half the world community at that time), this group coming on board only as political developments allowed in the late 1980s.

The recent reconstitution of an international geological community has been advanced by the successes of the original research effort of the science: the mapping and description of the earth's outer layers. But since the late 1960s, the science has gone through a rapid and thorough change in its ruling theory based on new evidence and methods. The old picture of stable continents and ocean basins, of dynamic interplay centered on slow geosynclinal filling, and the advance and retreat of broad, shallow seas from the continents has given way to a theoretical edifice called plate tectonics. This theory, actually continental drift under a different name and driven by the spreading of the sea floors rather than the splitting and rafting of continental bergs, is now almost universally accepted – the only theory in the history of geology to have support this broad and deep. The demonstration of the theory took place largely by analysis of magnetic data from the ocean floors as well as the continental surfaces in conjunction with radiometric dating. Since the

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1970s, the science has been increasingly dominated by geophysical methods, even though field geology and paleontology provided immense collateral data in showing the motion of continents in the earth's past and in correlating "paleocontinents." Further details of these developments are given elsewhere in this volume (see Oldroyd, Chapter 21; Frankel, Chapter 20).

With the earth's strata largely mapped, most major classes of fossil organisms described, and a detailed chronology of geological time firmly in place, European and American states began in the 1980s and 1990s to disinvest and even dismantle those aspects of the state-sponsored geological surveys without direct "economic benefit." At about the same time, geological curricula began to drop mineralogy, historical geology, and paleontology as required subjects and devote greater attention to geophysics, remote sensing, and computer-based modeling of geodynamic processes.

The cumulative effect of such theoretical and practical successes using physical techniques and theory, rather than traditional geology, and the decisive impact of lunar and planetary exploration have made attractive a view of the earth as once again an astronomical object, an approach reinforced by the discovery that many great extinctions on earth may have been caused by the impact of asteroids and comets. We increasingly view the earth as one of a family of planets among which one counts not only such long-known and familiar siblings as Venus and Mars but also interesting cousins such as the Jovian satellites Ganymede, Callisto, and Europa. The possibility of life, or possible evidence of former life, on these planets as a subject of direct observation subtracts the last presumption of uniqueness from this planet and signals the permanent change from geology to "earth science," best seen as a subdivision of planetology concentrating in the future on global biogeochemical cycles and their relationship to the long-term dynamic behavior of our planet.

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# 11

# PALEONTOLOGY

*Ronald Rainger*

The study of paleontology has long provided a rich field for historical analysis. Throughout the nineteenth and twentieth centuries, geologists and paleontologists played prominent, often highly visible roles in science and society, and an earlier generation of scholars devoted considerable attention to such individuals. Biographers, principally scientists, produced laudatory studies of such figures as Georges Cuvier (1769–1832), Roderick Impey Murchison (1792–1871), Richard Owen (1804–1892), and Othniel Charles Marsh (1832–1899). With the development of the history of science as a field in the 1960s and 1970s, scholars devoted their attention to other aspects of the subject. Emphasizing the importance of conceptual and methodological developments in science, historians defined the role that paleontologists had played in documenting the occurrence of extinction, determining the relative age of the earth, and contributing to evolutionary theory.

In more recent years, the increasing interest in understanding science in its social and cultural context has resulted in new and important studies. Focusing on major individuals and developments in the nineteenth century, these contextualized studies challenge the interpretations of an older historiography. In addition to examining the emergence of scientific communities, these analyses illustrate the ways in which social, political, and cultural factors shaped scientific careers and interpretations. The recent interest in scientific practice has fostered analyses of fieldwork and specimen collections. In addition, paleontology has become increasingly important from the perspective of the institutional and disciplinary dimensions of the science. As a field that straddles both the biological and geological sciences, paleontology and its practitioners did not fit easily into the increasingly specialized scientific institutions and infrastructures that began to emerge in the nineteenth century. The importance of extensive fossil collections, which required substantial material resources, posed additional problems for the field. For the most part, paleontology developed as a museum-based science, often separate from the expanding university systems, and consequently it has



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attracted the attention of those interested in questions concerning the social and institutional topography of science. Recent historical studies have examined not only the disciplinary difficulties that paleontologists experienced within the university context but also the ways in which social, cultural, and political factors related to museum development influenced work within the science. Similarly, the interest in scientific popularization and the relationship between science and the public has had an impact on historical studies of the field. By examining a wide range of questions pertaining to the roles of scientists, specimens, and exhibits within museum contexts, historians have directed attention to paleontology's public dimension. A study of the history of paleontology offers insights into not only the new and important developments within that science but also the changing historiography of the history of science.

## CUVIER, EXTINCTION, AND STRATIGRAPHY

Prior to the nineteenth century, the concept of extinction generated considerable debate and discussion. For hundreds of years, naturalists had been discovering what we now recognize as fossils; however, the idea that such specimens constituted the remains of extinct organisms was not taken for granted. Extinction raised serious philosophical and theological questions, and even such avid naturalists as Thomas Jefferson (1743–1826) refused to accept the idea that mastodon bones or similar objects belonged to organisms that no longer existed.[1]

It was Georges Cuvier, a French zoologist and comparative anatomist, who first demonstrated the occurrence of extinction. After training in Stuttgart and undertaking additional study on his own, Cuvier in 1795 was appointed to the Muséum d'Histoire Naturelle in Paris. The following year, in a presentation entitled "Species of Living and Fossil Elephants," Cuvier used comparative anatomy to demonstrate that although mammoths and mastodons belonged to the same genus as modern elephants, they were different species that no longer existed. Some, including Jean Baptiste de Lamarck (1744–1829), did not accept that interpretation, but Cuvier's demonstration became the basis for all later work in vertebrate paleontology.[2]

Cuvier's paleontology rested on commitments to principles of taxonomy and comparative anatomy. Influenced by Antoine-Laurent de Jussieu, Cuvier combined a belief in a natural system of classification with an interest in

[1] Martin J. S. Rudwick, *The Meaning of Fossils: Episodes in the History of Palaeontology* (Chicago: University of Chicago Press, 1972), pp. 1–48; Thomas Jefferson, "Notes on The State of Virginia," in *The Portable Thomas Jefferson*, ed. Merrill D. Peterson (New York: Penguin, 1975), pp. 73–8.

[2] Rudwick, *Meaning of Fossils*, pp. 101–23; William Coleman, *Georges Cuvier, Zoologist: A Study in the History of Evolution Theory* (Cambridge, Mass.: Harvard University Press, 1964). See also Rudwick, *Bursting the Limits of Time: The Reconstruction of Geohistory in the Age of Revolution* (Chicago: University of Chicago Press, 2005).

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comparative anatomy as exemplified in the work of Felix Vicq d'Azyr and Louis-Jean-Marie Daubenton. Of prime importance was Cuvier's belief in the functional integrity of the organism: that only certain organs could exist and that every organism was a unique whole. God had created only those organs needed for specific conditions of existence, thus teleological functionalism characterized Cuvier's science. Cuvier also believed in the subordination and correlation of parts, that certain organs were more important than others, and that each part had a reciprocal relation to others. On that basis, he described, reconstructed, and classified dozens of families of fossil vertebrates. Cuvier became the leading natural historian in France and an important influence and resource for others. The British naturalists William Buckland (1784–1856) and William Conybeare (1787–1857) corresponded with and sent specimens to Cuvier, and Richard Owen and Louis Agassiz (1807–1873) launched their own careers by working with Cuvier.[3]

Cuvier's work also influenced developments in stratigraphy. Throughout the eighteenth century, many recognized that rocks and organic remains were found in strata. In the 1780s, the German mineralogist Abraham Werner (1749–1817) developed a system of geognosy that identified distinct formations and defined the relative ages of the earth's formations. Werner based his system on rocks and structure, not fossils, and it was William Smith (1769–1839) who first relied on organic remains to define strata and relative age. But Smith's work remained unpublished, and it was Cuvier and his colleague Alexandre Brongniart who in 1807 first described how fossils could be employed to define strata. Relying on the principle of superposition, that fossils in higher strata were of younger age than fossils lower down, they identified seven strata in the Paris basin and established that fossils could serve as a foundation for stratigraphy.[4]

Building on those studies, early nineteenth-century scientists developed a more refined and precise history of the earth. Fieldwork became normative practice, and geologists undertook extended excursions that enabled them to identify and define many of the most important features of earth history. Much of that work took place in Great Britain, where by mid-century Roderick Murchison and Adam Sedgwick (1785–1873) had identified the Cambrian, Silurian, and Devonian periods. Likewise John Phillips (1800–1874) proposed what are now recognized as the three principal eras of earth history: the Paleozoic, Mesozoic, and Cenozoic.[5]

[3] Rudwick, *Meaning of Fossils*, pp. 101–23; Coleman, *Georges Cuvier, Zoologist*; Toby A. Appel, *The Cuvier-Geoffroy Debate: French Biology in the Decades before Darwin* (New York: Oxford University Press, 1987), pp. 40–68; Nicolaas A. Rupke, *Richard Owen: Victorian Naturalist* (New Haven, Conn.: Yale University Press, 1994), pp. 23–4; Edward Lurie, *Louis Agassiz: A Life in Science* (Cambridge, Mass.: Harvard University Press, 1960), pp. 53–71.

[4] Rudwick, *Meaning of Fossils*, pp. 124–30; Rachel Laudan, *From Mineralogy to Geology: The Foundations of a Science, 1650–1830* (Chicago: University of Chicago Press, 1987).

[5] Martin J. S. Rudwick, *The Great Devonian Controversy: The Shaping of Science among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985), pp. 17–60; James A. Secord, *Controversy in Victorian Geology: The Cambrian-Silurian Dispute* (Princeton, N.J.: Princeton University Press, 1986), pp. 14–143.

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Traditional historical studies explained such development in positivistic terms, as a consequence of more data, improved methods, and a commitment to empiricism. More recently, Martin Rudwick, James Secord, and David Oldroyd have developed new and important interpretations of the history of British geology. Focusing on controversies that accompanied the identification of those systems, these authors explain the construction of the geological timescale within the context of the social, political, and cultural world of nineteenth-century British science. Rudwick explores the establishment of the Silurian and Devonian not merely as a geological dispute between Murchison and Sedgwick, but as a process of controversy, struggle, and negotiation that entailed issues of location, power, and status among a wide range of scientists and specialists both within London and beyond. Secord's study of Murchison's and Sedgwick's roles in the Cambrian–Silurian controversy examines the cultural as well as social and scientific factors that characterized the work of the principal figures. Oldroyd notes that Charles Lapworth's (1842–1920) delineation of fossil zones led him to question Murchison's effort to extend the Silurian to the Highlands of Scotland, but only by the early twentieth century, after the death of Murchison's protegé Archibald Geikie, did Lapworth's identification of the Ordovician gain support. Similar debates occurred in the United States, where scientists disagreed over the age and identification of the Taconic System. Although John Diemer is critical of such studies, the analyses by Rudwick, Secord, and Oldroyd demonstrate that scientists cannot be understood outside their social, cultural, and political contexts. The conceptual and methodological tools they employ should be adopted by other scholars to examine other scientific communities and activities.[6]

## PALEONTOLOGY AND PROGRESS

Although Cuvier laid the foundations for stratigraphy, he was reluctant to interpret stratigraphic succession as indicating a direction for the history of life. For Cuvier, the fossil sequences in the Paris basin were not indicative of progression; rather they defined a cycle of alternating marine and freshwater conditions: sudden diluvial catastrophes followed by the introduction of new fauna. Older studies associated Cuvier's catastrophism with overt religious views, however, more recent work offers fuller and more subtle interpretations. Rudwick defines Cuvier's catastrophism in terms of the regularities of a Newtonian universe. Dorinda Outram, who explores the personal, social,

[6] Rudwick, *Great Devonian Controversy*; Secord, *Controversy in Victorian Geology*; David R. Oldroyd, *The Highlands Controversy: Constructing Geological Knowledge through Fieldwork in Nineteenth-Century Britain* (Chicago: University of Chicago Press, 1990); Cecil J. Schneer, "The Great Taconic Controversy," *Isis*, 69 (1978), 173–91; John Diemer and Michael Collie, "Murchison in Moray: A Geologist on Home Ground. With the Correspondence of Roderick Impey Murchison and the Rev. Dr. George Gordon of Birnie," *Transactions of the American Philosophical Society*, 85, pt. 3 (1995), 1–263.

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and political contexts within which Cuvier operated, argues that he sought to detach himself and his science from scriptural geology. Cuvier did not explain extinction or earth history in religious terms, and by separating paleontology from theology worked to establish an entire new field of knowledge. Toby Appel, although noting that Cuvier was a religious man, attributes avoidance of religion in his scientific writings to his commitment to an empirical science and fear that unbridled speculation would yield unsettling social and political consequences.[7]

Cuvier's hesitations notwithstanding, naturalists in Great Britain interpreted the increasing data from the fossil record as evidence of catastrophes, followed by new creations, that demonstrated design and progress. The prevailing physical theory of a cooling earth bolstered a directionalist interpretation. For those working with the tradition of British natural theology, the fossil record demonstrated a series of miraculous creations, culminating in the appearance of humans. James Parkinson defined the history of the fossil record in scripturalist terms, while William Buckland identified the last catastrophe with the biblical Flood. William Conybeare and Adam Sedgwick did not embrace such strict religious interpretations but still believed in a progressive history of the earth. By the mid-nineteenth century, however, some had abandoned the linear succession from reptiles to mammals to humans in favor of multilinear systems.[8]

Not all accepted a belief in progress, however. Charles Lyell (1797–1875), author of *Principles of Geology* (1830–3), rejected catastrophism in favor of a uniformitarian interpretation that emphasized actualism, gradualism, and belief in a steady-state system. That commitment, coupled with Lyell's reluctance to define humans as the highest animal on a linear scale, resulted in outspoken opposition to progressivism. A number of recent studies have also described Thomas Henry Huxley's (1825–1895) denial of progress in the fossil record. As Mario Di Gregorio notes, Huxley remained committed to a typological concept of species into the 1860s and emphasized the persistence of primitive forms. Adrian Desmond attributes Huxley's position on progress to his views on geographical distribution and his opposition to Richard Owen, one of the chief proponents of progression. Both authors indicate that it was only in the late 1860s, after having read the work of Ernst Haeckel, that Huxley began to interpret the fossil record in evolutionary terms, but he never abandoned his interest in the persistence of primitive organisms.[9]

[7] Rudwick, *Meaning of Fossils*, pp. 130–1; Peter J. Bowler, *Fossils and Progress: Paleontology and the Idea of Progressive Evolution in the Nineteenth Century* (New York: Science History Publications, 1976), pp. 1–22; Appel, *Cuvier-Geoffroy Debate*, pp. 46–59; Dorinda Outram, *Georges Cuvier: Vocation, Science and Authority in Post-Revolutionary France* (Manchester: Manchester University Press, 1984).

[8] Rudwick, *Meaning of Fossils*, pp. 131–49, 164–217; Bowler, *Fossils and Progress*, pp. 93–115.

[9] Rudwick, *Meaning of Fossils*, pp. 187–91; Bowler, *Fossils and Progress*, pp. 67–79; Mario Di Gregorio, *T. H. Huxley's Place in Nature* (New Haven, Conn.: Yale University Press, 1984), pp. 53–126; Adrian Desmond, *Archetypes and Ancestors: Palaeontology in Victorian London, 1850–1875* (Chicago: University of Chicago Press, 1982), pp. 84–112; Adrian Desmond, *Huxley: From Devil's Disciple to Evolution's High Priest* (Reading, Mass.: Addison-Wesley, 1997), pp. 193–4, 204–5, 255–9, 293–4, 354–60.

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## PALEONTOLOGY AND EVOLUTION

Although many early nineteenth-century geologists and paleontologists believed that the fossil record demonstrated progress, the question of whether progress entailed evolution was a much more controversial matter. Many opposed evolution, and none more forcefully than Cuvier. Although Cuvier allowed for minor modification, once an organ changed, all organs had to change to maintain the functional integrity of the organism. Yet that was not possible because intermediate forms could not function or survive. Thus, there were no links in the fossil record, and fossils were not the ancestors of recent organisms. Cuvier defined earth history in terms of catastrophes that killed all organisms, followed by migrations or creations that yielded new forms, rather than evolution. In 1800, Cuvier opposed Lamarck's evolutionary theory, and he later rejected the evolutionary ideas of Etienne Geoffroy Saint-Hilaire (1772–1844). In contrast to Cuvier's teleological functionalism, Geoffroy, also a curator at the Paris museum, concentrated on identifying homologies that indicated transformations in structure and function among organisms. Originally identifying such changes among vertebrates, Geoffroy later extended his philosophical anatomy to emphasize unity of composition among all animals. Drawing on studies in teratology, Geoffroy by the late 1820s claimed that the environment could act on a developing fetus in such a way as to produce evolution. Applying that interpretation to the fossil record, he maintained that a recently discovered specimen of an extinct crocodile constituted a link in a progressive series from reptiles to mammals. This was anathema to Cuvier, and in 1830 he denounced Geoffroy's views before the French Academy of Sciences.[10]

Traditionally, scholars defined the Cuvier–Geoffroy debate in scientific terms, pitting teleological functionalism (Cuvier) against morphology (Geoffroy). Such interpretations emphasized Cuvier's triumph over Geoffroy and defined opposition to evolution as a hallmark of nineteenth-century French biology and paleontology. Appel, however, has offered a different interpretation with important historiographic implications. Her work indicates that for Cuvier the debate concerned more than different approaches to comparative anatomy. Cuvier contrasted his strict empiricism with Geoffroy's belief that analogy and speculation could play a role in science. Cuvier's position within the Paris museum, where Geoffroy also had supporters, and concern over scientific and political threats arising in the 1820s, contributed to Cuvier's effort to vanquish his rival. Most important, Appel indicates that although Cuvier got the better of Geoffroy in the debate, philosophical anatomy did not die; on the contrary, it gained in popularity.[11]

[10] Appel, *Cuvier-Geoffroy Debate*, pp. 40–174.

[11] Franck Bourdier, "Geoffroy Saint-Hilaire versus Cuvier: The Campaign for Paleontological Evolution (1825–1838)," in *Toward a History of Geology*, ed. Cecil J. Schneer (Cambridge, Mass.: MIT Press,

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Recent historical studies have likewise significantly changed the understanding of the status of evolution in early nineteenth-century Great Britain. Based on studies of some of the most prominent geologists and naturalists, a previous generation of historians accepted the view that Charles Darwin (1809–1882) was virtually alone in espousing evolution. Dov Ospovat and Philip Rehbock were among the first to note the influence of philosophical anatomy in England, but the work of Desmond especially has opened important new perspectives on the subject. Desmond's work examining a broad range of naturalists and physicians in the 1820s and 1830s has indicated that many rejected a science supported by conservative social and religious underpinnings. Among the disenfranchised and disaffected, the views of Lamarck and particularly Geoffroy had widespread scientific and social appeal. Many associated evolution with the potential for advancement and improvement, but many also embraced a morphology based on natural laws in place of a functionalism tied to teleology. The increasing acceptance of Karl Ernst von Baer's embryology, which denied recapitulation in favor of embryonic divergence from an initial germ, reinforced that trend. By the 1840s, traditional views were being challenged, and no one played a more interesting role in that regard than Richard Owen.[12]

Throughout the first half of the nineteenth century, Owen was the leading biologist and paleontologist in Great Britain. As superintendent of the specimens in the Hunterian Museum of the Royal College of Surgeons, Owen cataloged, described, and increased the number of fossils at that institution. Owen also acquired specimens from Britain's far-flung empire, and the later establishment of the British Museum of Natural History was one of his major achievements. Owen followed Cuvier in emphasizing form in relation to function, as evidenced in his study of the pearly nautilus. Nicolaas Rupke defines Owen's early work as part of a natural theology tradition associated with Buckland at Oxford, whereas for Desmond, Owen in the 1830s was influenced by the conservative philosophy associated with the Hunterian Museum and sought to undermine support for Lamarck and Geoffroy among radicals. Among his more notable efforts were analyses of Mesozoic mammals from Stonesfield and British fossil reptiles, including dinosaurs, that contradicted Robert Grant's (1793–1874) evolutionary interpretations.[13]

1969), pp. 33–61;" Appel, *Cuvier-Geoffroy Debate*; Pietro Corsi, *The Age of Lamarck: Evolutionary Theories in France, 1790–1830* (Berkeley: University of California Press, 1988).

[12] Dov Ospovat, *The Development of Darwin's Theory: Natural History, Natural Theology, and Natural Selection, 1838–1859* (Cambridge: Cambridge University Press, 1981); Philip F. Rehbock, *The Philosophical Naturalists: Themes in Early Nineteenth-Century British Biology* (Madison: University of Wisconsin Press, 1983); Adrian Desmond, *The Politics of Evolution: Morphology, Medicine and Reform in Radical London* (Chicago: University of Chicago Press, 1989). On the influence of von Baer, see Dov Ospovat, "The Influence of Karl Ernst von Baer's Embryology, 1828–1859: A Reappraisal in Light of Richard Owen's and William B. Carpenter's 'Palaeontological Application of von Baer's Law'," *Journal of the History of Biology*, 9 (1976), 1–28.

[13] Desmond, *Politics of Evolution*, pp. 236–344; Rupke, *Richard Owen*.

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Yet Owen soon abandoned Cuvierian functionalism and by the late 1840s was interpreting the history of life in terms similar to those of Geoffroy. Although denying Geoffroy's common plan for all organisms, Owen had accepted the concept of a vertebrate archetype. His essay *On the Nature of Limbs*, published in 1849, offered the fullest exposition of his views. Building on the work of Geoffroy and Carl Gustav Carus, Owen coined the term "homology" to define morphological similarities among different organisms. Based on such similarities, vertebrates could be traced back to an idealized, primitive archetype, little more than a series of vertebrae. Organic change, according to Owen, constituted a divergence from that archetype that was caused by two forces: a polarizing force that produced repetition of similar structures and a specialized organizing force that enabled organisms to adapt to new and different conditions. The interaction of those forces yielded change, eventually resulting in the appearance of humans. Owen had not discarded teleology, but by the 1850s he was interpreting the history of life in terms of secondary laws that produced adaptation, divergence, and specialization from a generalized archetype. In contrast to an older interpretation, most scholars now maintain that Owen accepted some form of evolution, albeit not Darwin's theory of evolution by natural selection. Owen did not explain evolution in materialist terms, and although recognizing and documenting the divergence and complexity of the fossil record, he understood the history of life on earth as progressive and ultimately under the direction of the Creator. On those and other grounds, he opposed Darwin's theory of evolution, but that did not keep him from interpreting the fossil record in evolutionary terms. In the late 1850s, he referred to *Archegosaurus* as a bridge between fishes and reptiles, and he later defined specimens from South Africa as the link between mammals and reptiles. Owen began to lay the foundation for an evolutionary interpretation of the fossil record in his book *Palaeontology* (1860), and in some respects his views were difficult to distinguish from Darwin's.[14]

Yet it was Darwin's theory, not Owen's, that influenced much of the paleontological research in the late nineteenth century. In part that was because of Darwin's supporters, who promoted his theory while undermining Owen's work and reputation. No one played a more important role in that regard than T. H. Huxley. Although he did not accept crucial features of Darwin's theory, Huxley quickly emerged as Darwin's most outspoken proponent. And, even though he had done virtually no work in paleontology before the late 1850s, it was in that field that Huxley challenged Owen. Desmond explains this in terms of Huxley's social and scientific ambitions. A generation younger than Darwin and Owen, Huxley rebelled against a system that

[14] Desmond, *Archetypes and Ancestors*, pp. 19–83; Desmond, *Politics of Evolution*, pp. 335–72; Rupke, *Richard Owen*, pp. 106–258.

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offered few professional opportunities to men of his age and socioeconomic standing. To Huxley, Owen represented the worst of an older order based on favoritism rather than merit. Huxley first criticized Owen's concept of the archetype and commitment to progress, but, according to Desmond, he soon realized the polemical importance of fossils and began work in paleontology. Owen's claim that the lack of the hippocampus minor bone distinguished humans from other primates roused Huxley's anger, and the two waged a nasty public debate over the issue. Huxley's *Man's Place in Nature* (1863) was not distinguished for its evolutionary interpretation or extended analysis of fossil human specimens. Yet it did signal a triumph over Owen, and while it was several years before Huxley would use fossils to construct phylogenies, he had played an important role in removing one of his and Darwin's major opponents in the field. Recent biographies provide extensive new information on the scientific activities and controversies of both men. Although Rupke examines Owen's work in much greater detail than previous studies, it is Desmond's contextualized analysis of Huxley that illustrates the fruitfulness of social history for biography.[15]

Equally important was the stimulus provided by Darwin's work. Darwin himself had done little work in paleontology; his only extended research was on fossil barnacles, and *On The Origin of Species* offered only meager evidence from the fossil record to support evolution. Yet *Origin of Species* had considerable popularity, and it provided a framework for future investigations. Beginning in the 1860s, many scientists took up morphological research: studies in embryology, comparative anatomy, and paleontology that emphasized the search for connections that would demonstrate the occurrence of evolution. Within paleontology, scientists sought intermediate forms, "missing links," to document evolution at the generic or species level or to establish connections among higher categories. The Swiss naturalist Ludwig Rütimeyer was one of the first to describe evolution among fossil mammals, and Melchior Neumayr, Franz Hilgendorf, and Wilhelm Waagen did much the same for fossil invertebrates. Naturalists had long known of the occurrence of fossil horses, and in 1866 the French scientist Albert Gaudry uncovered several new specimens and produced the first phylogeny of that family. More sophisticated studies of the topic came from a Russian scientist, Vladimir Kovalevskii (1842–1883). Confining his research to specimens in major museums, Kovalevskii's anatomical analyses enabled him to define Cuvier's *Anchitherium* as a transitional form between *Paleotherium* and horses. Kovalevskii was also a Darwinian, and in addition to documenting the existence of transitional forms, he explained modifications in structure in terms of their functional, adaptive value and in relation to changing external conditions. Few fully accepted Kovalevskii's interpretation, and in Russia his work met with a hostile reception. Yet many

15 Desmond, *Huxley*, pp. 251–335; Rupke, *Richard Owen*, pp. 259–322.

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valued his factual contributions, and his paleobiological approach influenced the work of Louis Dollo and Othenio Abel.[16]

Equally important were studies by paleontologists in the United States. Although Agassiz had rejected Darwin's theory, his students, including Alpheus Hyatt (1839–1902), took an interest in evolution. Convinced that the development of living nautiloids constituted a recapitulation of the evolutionary history of their fossil ancestors, the ammonites, Hyatt spent a lifetime documenting the evolution of that group. His work influenced several younger paleontologists: James Perrin Smith extended Hyatt's work on the evolution of ammonites, and Charles Emerson Beecher and Robert Tracey Jackson charted the evolution of brachiopods and pelycopods, respectively.[17]

More well known were the efforts of Americans working on fossil vertebrates. In the 1840s and 1850s, naturalists associated with expeditions to the American West sent hundreds of specimens to Joseph Leidy (1823–1891), a Philadelphia physician. Leidy's studies of fossil horses, oreodonts, and other extinct vertebrates focused on empirical problems: identification, description, and classification. Leidy recognized connections between older and more recent remains, and in the 1860s he accepted evolution but made virtually no attempts to explain that process or to construct phylogenies. Two of Leidy's younger colleagues, Edward Drinker Cope (1840–1897) and Othniel Charles Marsh (1831–1899), had no such hesitations. Both participated in government-sponsored explorations of the American West but relied primarily on inherited wealth to undertake their own expeditions. Several studies have documented their intense rivalry, their possessive, even rapacious, efforts to control fossil specimens, collecting sites, and collectors. Their competition led to priority disputes over discovering, naming, and describing new specimens, and as Ronald Rainger indicates, Marsh sought to lay down rules for doing work in paleontology and systematics. Yet each also made significant contributions. Together, Cope and Marsh discovered over 1,500 new fossil specimens, many of them representing genera and families previously unknown. Although Cope discovered more new specimens than his rival, it was Marsh's work that excited other paleontologists. Research in the Kansas Cretaceous in the early 1870s led to discoveries of birds with teeth, providing documentary evidence of an evolutionary relationship between birds and reptiles. The dinosaurs Marsh discovered, including the gigantic *Brontosaurus* (*Apatosaurus*) and *Diplodocus*, dwarfed the specimens previously found in

[16] Rudwick, *Meaning of Fossils*, pp. 218–71; Ronald Rainger, "The Understanding of the Fossil Past: Paleontology and Evolution Theory, 1850–1910" (PhD diss., Indiana University, 1982), pp. 83–156. On Kovalevsky, see Daniel P. Todes, "V. O. Kovalevskii: The Genesis, Content, and Reception of His Paleontological Work," *Studies in History of Biology*, 2 (1978), 99–165.

[17] Peter J. Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolutionary Theories in the Decades around 1900* (Baltimore: Johns Hopkins University Press, 1983); Ronald Rainger, "The Continuation of the Morphological Tradition: American Paleontology, 1880–1910," *Journal of the History of Biology*, 14 (1981), 129–58.

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Europe. Perhaps most impressive, his work in the American Midwest yielded horse specimens from virtually every epoch of the Cenozoic era, providing the most complete phyletic history of that family. On tour in the United States in 1876, Huxley expressed amazement at the fossils Marsh showed him, and Darwin referred to Marsh's work as the most important documentary evidence for evolution.[18]

Although the personal antipathy between Cope and Marsh had deleterious consequences, it did not keep the next generation from contributing to paleontology. Marsh had virtually no students, but several of his collectors, including John Bell Hatcher and Samuel Wendell Williston, made significant discoveries of fossil reptiles and mammals. So, too, did two other vertebrate paleontologists, William Berryman Scott (1858–1947) and Henry Fairfield Osborn (1857–1935). At Princeton, Scott conducted work in both the classroom and the field, and his close friend Osborn created a much larger and more ambitious program for vertebrate paleontology at Columbia University and New York's American Museum of Natural History. Rainger describes how, with financial support from wealthy patrons, Osborn sent collectors not only into the American West but eventually to Canada, Africa, and Asia in search of fossil vertebrates. Their efforts resulted in discoveries of thousands of fossil mammals and reptiles and gave the American Museum of Natural History one of the premier collections in the world. Osborn and his principal associates William Diller Matthew (1871–1930) and William King Gregory (1876–1970) produced new, sophisticated evolutionary histories that surpassed the work of the previous generation. Their research, particularly studies on the functional morphology of fossil vertebrates conducted by Gregory and his students Charles Camp and Alfred Sherwood Romer, provided new interpretations of the transition of animals from water to land, the origin of flight, the origin of bipedalism, and other morphological problems. Americans were not the only ones contributing to that tradition. Peter Bowler has indicated that paleontologists in Europe and elsewhere continued to compile fossil evidence in support of evolution and explore questions concerning the history of specific structures, functions, and behaviors, as well as the origin and evolution of major categories. Bowler emphasizes the continued intellectual activity within the morphological tradition, but Rainger's study of American paleontologists and Lynn Nyhart's analysis of morphology in the German universities suggest that, despite ongoing research, a

[18] Elizabeth Noble Shor, *The Fossil Feud between E. D. Cope and O. C. Marsh* (Hicksville, N.Y.: Exposition, 1974). On the scientific work of Cope and Marsh, see Ronald Rainger, *An Agenda for Antiquity: Henry Fairfield Osborn and Vertebrate Paleontology at the American Museum of Natural History, 1890–1935* (Tuscaloosa: University of Alabama Press, 1991), pp. 7–23; Ronald Rainger, "The Rise and Decline of a Science: Vertebrate Paleontology at Philadelphia's Academy of Natural Sciences, 1820–1900," *Proceedings of the American Philosophical Society*, 136 (1992), 1–32; Desmond, *Huxley*, pp. 471–82; Charles Schuchert and Clara Mae LeVene, *O. C. Marsh: Pioneer in Paleontology* (New Haven, Conn.: Yale University Press, 1940), pp. 246–7.

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variety of social and institutional indicators point to a decline in that tradition. Additional studies of other contexts, particularly studies like Nyhart's that combine conceptual analysis with social and institutional analysis of the problem, are needed.[19]

Although many paleontologists studied evolution, few embraced Darwin's theory of evolution by natural selection. From the 1860s through the 1930s, most paleontologists who examined questions pertaining to the mechanisms and patterns of evolution adopted neo-Lamarckian or orthogenetic interpretations. Here, too, as Bowler and Rainger indicate, American paleontologists were among the most prolific and outspoken. In the 1860s, Cope and Hyatt, unlike Darwin, claimed that the fossil record indicated linear, cumulative patterns of change. Both accepted the doctrine of recapitulation, and both identified a law of acceleration, by which the speeding up of individual development enabled organisms to add on new characters at the end of an inherited ontogeny, as the mechanism for linear evolutionary change. Originally, Cope explained evolution in theistic terms, but by the 1870s he had identified the organism's response to the environment as the trigger for acceleration and evolution. On some topics, notably the evolution of mammalian tooth and foot structure, he emphasized adaptation and the use or disuse of parts. Yet his commitment to the inheritance of acquired characters led Cope to define most fossil sequences in linear terms. Hyatt, too, identified adaptive response to the environment as explaining acceleration and evolution. But wedded to an embryological model in which evolution had to end in racial senility and degeneration, he, too, emphasized nonadaptive trends. Cope and Hyatt were influential in the United States, but as Bowler has demonstrated, the belief in recapitulation, the inheritance of acquired characters, and the prevalence of nonadaptive trends in the fossil record was commonplace among paleontologists of the time.[20]

Not all paleontologists, however, accepted neo-Lamarckian interpretations. Hyatt's emphasis on evolution as an ongoing path toward extinction smacked of orthogenesis. Osborn and Scott, who had originally accepted Cope's views, abandoned neo-Lamarckism in favor of orthogenesis. Attempting to incorporate new work on inheritance, especially August Weismann's challenge to neo-Lamarckism, Osborn in the 1890s developed a theory according to which environmental changes would trigger an ancestral germ plasm, which in turn would produce gradual, cumulative evolutionary change over time. Rejecting Darwin's theory, Osborn published massive tomes defining the history of elephants, rhinoceroses, and titanotheres in strictly linear, nonrandom terms. Many other paleontologists, including Othenio Abel

19 Rainger, *Agenda for Antiquity*; Peter J. Bowler, *Life's Splendid Drama: Evolutionary Biology and the Reconstruction of Life's Ancestry, 1860–1940* (Chicago: University of Chicago Press, 1996); Lynn K. Nyhart, *Biology Takes Form: Animal Morphology and the German Universities, 1800–1900* (Chicago: University of Chicago Press, 1995).

20 Bowler, *Eclipse of Darwinism*, pp. 121–35; Rainger, "Understanding of the Fossil Past," pp. 196–242.

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(1875–1946) and Rudolf Wedekind (1883–1961), proposed orthogenetic theories that, although somewhat different from Osborn's, nonetheless explained evolution as being caused by factors other than the natural selection of random variations and described linear patterns of change that seemed to lead almost inexorably to the extinction of a particular family or class.[21]

## PALEONTOLOGY AND MODERN DARWINISM

The new Mendelian genetics found few adherents among early twentieth-century paleontologists. The rediscovery of Mendel's work in 1900, coupled with the emergence of new, laboratory-based experimental programs, promoted much experimentation in genetics, particularly in the United States. Yet T. H. Morgan's new chromosomal theory of inheritance was not readily embraced by paleontologists in the United States or elsewhere. Rainger, while noting the continued belief in the inheritance of acquired characters, has argued that the prevalence of paleontologists in museums and geology, not biology programs, contributed to the lack of acceptance of genetics in the United States. Jonathan Harwood has defined the social structure as well as the cultural commitments within the German academic community as reasons for opposition to Mendelian genetics and Darwinian evolutionary theory in that country.[22]

By the 1920s and 1930s, however, biologists and paleontologists were challenging older interpretations. While many experimental biologists ignored the findings of paleontology, Julian Huxley employed statistical tools to challenge Osborn's orthogenetic interpretations. Even more important was the work of vertebrate paleontologist George Gaylord Simpson (1902–1984). Ronald Rainger and Marc Swetlitz have indicated that Simpson's American Museum of Natural History colleagues, Matthew and Gregory, influenced Simpson's rejection of orthogenesis and adoption of Darwinian evolutionary theory. Léo Laporte's studies examine how Simpson's statistical analyses of evolutionary rates and trends, coupled with his understanding of population genetics, made his book *Tempo and Mode in Evolution* a major contribution to the evolutionary synthesis. According to Simpson, the same genetic factors that account for the evolution of species likewise explained the origin and evolution of higher categories.[23]

[21] Bowler, *Eclipse of Darwinism*, pp. 173–7; Rainger, *Agenda for Antiquity*, pp. 37–44, 123–51; Wolf-Ernst Reif, "The Search for a Macroevolutionary Theory in German Paleontology," *Journal of the History of Biology*, 19 (1986), 79–130.

[22] Rainger, *Agenda for Antiquity*, pp. 133–45; Jonathan Harwood, *Styles of Scientific Thought: The German Genetics Community, 1900–1933* (Chicago: University of Chicago Press, 1993).

[23] Rainger, *Agenda for Antiquity*, pp. 182–248; Marc Swetlitz, "Julian Huxley, George Gaylord Simpson, and the Idea of Progress in Twentieth-Century Evolutionary Biology" (PhD diss., University of Chicago, 1991), pp. 53–91, 164–99; George Gaylord Simpson, *Tempo and Mode in Evolution* (New York: Columbia University Press, 1944); Léo F. Laporte, "Simpson's *Tempo and Mode in Evolution* Revisited," *Proceedings of the American Philosophical Society*, 127 (1983), 365–416.

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Whereas biologists embraced Simpson's work, the reaction among paleontologists was mixed. Most American paleontologists ignored Simpson's work and continued to publish descriptive morphologic and systematic papers. Some, such as Everett C. Olson (1910–1993), expressed dissatisfaction with the idea that microevolutionary processes could explain the evolution of higher categories. Olson never presented an alternative to the modern synthesis, but as Wolf-Ernst Reif has shown, many German paleontologists did. Although neo-Lamarckian and orthogenetic theories remained popular, Otto Schindewolf's (1896–1971) typostrophic theory, which distinguished species evolution from the evolution of higher categories and emphasized sudden and cyclical evolutionary change, was particularly influential. As the leading paleontologist in Germany, Schindewolf's views wielded considerable influence into the 1970s.[24]

Yet Simpson's work and the evolutionary synthesis were not without influence. Following World War II, a growing interest in evolutionary problems emerged from an unlikely source: American invertebrate paleontologists. In contrast to Europe, where students of fossil invertebrates maintained a continuous tradition of interest in evolution, invertebrate paleontology in the United States served the petroleum industry, and fossils were understood as little more than stratigraphic markers. By the late 1940s, some invertebrate paleontologists were dissatisfied with that emphasis and eager to examine fossils from a biological perspective. Norman Newell (1909–2005), an invertebrate paleontologist at Columbia University and the American Museum of Natural History who worked with Simpson, recognized the importance of understanding population genetics, adopting a population concept of species, and employing statistical techniques to study evolutionary rates. By the 1960s, Newell and others were referring to their work as paleobiology, a term that emphasized the importance of ecological and evolutionary questions rather than the stratigraphic, descriptive objectives that had characterized invertebrate paleontology.[25]

In 1971, two of Newell's former students, Niles Eldredge and Stephen Jay Gould, published a powerful criticism of the evolutionary synthesis. Rejecting the neo-Darwinian emphasis on phyletic gradualism, Eldredge and Gould defined evolution not as a slow, continuous process but rather as a series of rapid bursts of change followed by periods of stasis, which they termed

[24] Léo F. Laporte, "George G. Simpson, Paleontology, and the Expansion of Biology," in *The Expansion of American Biology*, ed. Keith R. Benson, Jane Maienschein, and Ronald Rainger (New Brunswick, N.J.: Rutgers University Press, 1991), pp. 92–100; Ronald Rainger, "Everett C. Olson and the Development of Vertebrate Paleoecology and Taphonomy," *Archives of Natural History*, 24 (1997), 373–96; Reif, "Search for a Macroevolutionary Theory in German Paleontology," pp. 117–22.

[25] J. Marvin Weller, "Relations of the Invertebrate Paleontologist to Geology," *Journal of Paleontology*, 21 (1947), 570–5; Norman D. Newell and Edwin H. Colbert, "Paleontologist – Biologist or Geologist?" *Journal of Paleontology*, 22 (1948), 264–7; Norman D. Newell, "Infraspecific Categories in Invertebrate Paleontology," *Evolution*, 1 (1947), 163–71; Norman D. Newell, "Toward a More Ample Invertebrate Paleontology," *Bulletin of the Museum of Comparative Zoology*, 112 (1954), 93–7; Norman D. Newell, "Paleobiology's Golden Age," *Palaios*, 2 (1987), 305–9.

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"punctuated equilibrium." Their hypothesis sent paleontologists into the field, and from the outset there were conflicting reports. Whereas Steven Stanley found evidence for punctuated equilibrium among fossil invertebrates, Philip Gingerich claimed that his studies of fossil mammals discredited the hypothesis. Examining the history of *Kosmoceras*, David Raup and R. E. Crick maintained that they could neither confirm nor disprove the hypothesis. Subsequently, Eldredge and Gould, who had originally defined punctuated equilibrium as consistent with neo-Darwinism, began to speak of it as a new theory of evolution. Equating speciation with macromutations and claiming that adaptation and natural selection could not explain speciation, they decoupled macroevolution from microevolution. Debate still persists over the validity of the interpretation and on issues of hierarchy, macroevolution, and species selection associated with punctuated equilibrium.[26]

The recent emphasis on catastrophism and mass extinctions also poses challenges for neo-Darwinism. Lyell's doctrine of uniformitarianism, which for over a century had served as a fundamental tenet of paleontology and evolutionary biology, met with some criticism in the 1960s. Still, most geologists and paleontologists remained committed to the Darwinian view that extinction, like evolution, was a gradual process resulting from competition, adaptation, and natural selection. That changed in the late 1970s, when scientists led by Luis Alvarez (1911–1988) and Walter Alvarez (b. 1962) posited an extraterrestrial cause for mass extinction at the Cretaceous/Tertiary (K/T) boundary. Having discovered a concentration of iridium within a layer of clay formed 65 million years ago, the time of the dinosaur extinctions, the Alvarez team proposed that the iridium had resulted from the impact of a meteorite. Their additional claim that the meteorite had produced a dust cloud that killed the dinosaurs ignited tremendous debate within the scientific community. Additional discoveries of iridium concentrations at other K/T boundary sites, and evidence from shock crystals, diamonds, and impact craters, led most geochemists, planetary geologists, and impact scientists to accept the hypothesis.[27]

[26] Niles Eldredge and Stephen Jay Gould, "Punctuated Equilibria: An Alternative to Phyletic Gradualism," in *Models in Paleobiology*, ed. T. J. M. Schopf (San Francisco: Freeman, Cooper, 1972), pp. 82–115; Stephen Jay Gould, "Is a New and General Theory of Evolution Emerging?" *Paleobiology*, 6 (1980), 119–30; Steven W. Stanley, "A Theory of Evolution Above the Species Level," *Proceedings of the National Academy of Sciences USA*, 72 (1975), 646–50; Philip D. Gingerich, "Paleontology and Phylogeny: Patterns of Evolution at the Species Level in Early Tertiary Mammals," *American Journal of Science*, 276 (1976), 1–28; David M. Raup and R. E. Crick, "Evolution of Single Characters in the Jurassic Ammonite *Kosmoceras*," *Paleobiology*, 7 (1981), 200–15. On the continuing debate, see Albert Somit and Steven A. Peterson, eds., *The Dynamics of Evolution: The Punctuated Equilibrium Debate in the Natural and Social Sciences* (Ithaca, N.Y.: Cornell University Press, 1992).

[27] Stephen Jay Gould, "Is Uniformitarianism Necessary?" *American Journal of Science*, 263 (1965), 223–8; M. King Hubbert, "Critique of the Principle of Uniformity," *Geological Society of America Special Papers*, 89 (1976), 1–33; L. W. Alvarez, W. Alvarez, F. Asaro, and H. V. Michel, "Extraterrestrial Cause for the Cretaceous-Tertiary Extinction," *Science*, 208 (1980), 1095–1108; William Glen, "What the Impact/Volcanism/Mass Extinction Debates Are About" and "How Science Works in the Mass-Extinction Debates," both in *Mass Extinction Debates: How Science Works in a Crisis*, ed. William Glen (Stanford, Calif.: Stanford University Press, 1994), pp. 7–38, 39–91, respectively.

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Paleontologists, however, were divided over the issue. Many micropaleontologists accepted impact, as did prominent invertebrate paleontologists. David Jablonski presented evidence that mass extinctions differed from normal, background extinctions, and David Raup and J. J. Sepkoski relied on statistical analysis of 3,500 families of marine organisms to claim that mass extinctions had occurred every 26 million years. Their results stimulated additional efforts to explain periodic extinctions, and Raup drew on the impact hypothesis to argue for a neo-catastrophism that would supplant Darwinism and uniformitarianism. Others criticized such claims. Anthony Hallam accepted the occurrence of mass extinctions but explained them as the result of sea level changes or massive volcanism. Anthony Hoffman rejected the evidence for periodicity and extraterrestrial impacts and denied that the hypothesis constituted a legitimate challenge to neo-Darwinism. Vertebrate paleontologists likewise remained skeptical. William Clemens refined the scale of his geological fieldwork and developed new means of analyzing the fossil record, but did not accept impact. Other vertebrate paleontologists challenged the hypothesis on the grounds that dinosaurs were going extinct, meaning that even before the impact event, dinosaur extinction and iridium enrichment were not contemporaneous, and that many families of organisms lived on into the Cretaceous. William Glen explored the historical, philosophical, and sociological questions arising from the mass extinctions debate, all of which offer ample opportunity for further study.[28]

## PALEONTOLOGY AND BIOGEOGRAPHY

Paleontologists have long had an interest in the spatial relationships among organisms. Agassiz believed in centers of creation, zoological provinces that gave rise to specific types. In the 1860s, Philip Lutley Sclater emphasized the importance of geographical regions, an approach that reinforced typological thinking. By contrast, Darwin and his followers adopted a historical interpretation of biogeography, claiming that each species had originated in and dispersed from a single locality. Rejecting extended land bridges and sunken continents, Darwin suggested a biogeography based on migration, a subject that Alfred Russel Wallace (1823–1913) examined in his *Geographical*

[28] David Jablonski, "Background and Mass Extinctions: The Alternation of Macroevolutionary Regimes," *Science*, 231 (1986), 129–33; David M. Raup and J. J. Sepkoski, Jr., "Periodicity of Mass Extinctions in the Geological Past," *Proceedings of the National Academy of Sciences USA*, 81 (1984), 801–5; David M. Raup, "The Extinction Debates: A View from the Trenches," in Glen, *Mass Extinction Debates*, pp. 145–51; Anthony Hallam, "End-Cretaceous Mass Extinction Event: Argument for Terrestrial Causation," *Science*, 238 (1987), 1237–42; Anthony Hoffman, "Mass Extinctions: The View of a Sceptic," *Journal of the Geological Society, London*, 146 (1989), 21–35; William Glen, "On the Mass-Extinction Debates: An Interview with William A. Clemens," in Glen, *Mass Extinction Debates*, pp. 237–52; R. E. Sloan, J. K. Rigby, L. M. Van Valen, and D. Gabriel, "Gradual Dinosaur Extinction and Simultaneous Ungulate Radiation in the Hell Creek Formation," *Science*, 232 (1986), 629–33.

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*Distribution of Animals* (1876). Wallace believed that most families of mammals had originated in a northern, Holarctic region and maintained that minor changes in physical geography and known means of migration could explain their subsequent geographical distribution.[29]

Wallace's work created interest in biogeography; however, many attacked his interpretation on issues pertaining to southern continents and organisms. The prevailing geological theory of a cooling earth suggested that organisms had arisen at both poles; thus, the south and north had served as centers for geographical distribution. The presence of peculiar animals – edentates, sloths, and marsupials – reinforced the idea of southern origins. Arnold Ortmann and Charles Hedley claimed that land bridges had once connected Antarctica to Australia, South Africa, and Latin America, and Hermann von Ihering posited additional land bridges connecting Brazil and West Africa. Using the evidence of fossil vertebrates, the Argentinian paleontologist Florentino Ameghino (1854–1911) turned Wallace's interpretation on its head. Claiming that mammalian horizons and faunas of Latin America antedated those of the Northern Hemisphere, Ameghino identified Argentina as the center for the origin, evolution, and distribution of vertebrates. In 1912, the German meteorologist Alfred Wegener (1880–1930) coupled the idea of an extended southern land mass with evidence of similarities between fossil remains in Africa and South America to propose a theory of continental drift.[30]

Proponents of land bridges and southern origins ran into opposition from William Diller Matthew. A specialist in fossil mammals, and one of the few Darwinian paleontologists, Matthew maintained that continental land masses and ocean basins were permanent. He supported Wallace's interpretation, and his seminal work "Climate and Evolution" (1915) was an extended argument for the northern origin of all vertebrates. Opposing Wegener's continental drift on the lack of a *vera causa*, Matthew drew on his understanding of the fossil record and the intricacies of correlation to attack the interpretations of von Ihering, R. F. Scharff, and others. Charles Schuchert and Thomas Barbour criticized Matthew's views, but his work remained influential into the 1950s. Bowler, Rainger, and Laporte have examined these developments; however, analysis of individuals and theories within their social and political contexts awaits further study.[31]

## MUSEUMS AND PALEONTOLOGY

As science became more professionalized in the nineteenth century, paleontologists were able to locate themselves in a variety of niches. Some worked

[29] Bowler, *Life's Splendid Drama*, pp. 371–418; Rainger, *Agenda for Antiquity*, pp. 191–202.
[30] Ibid.
[31] Ibid.; Léo F. Laporte, "Wrong for the Right Reasons: George Gaylord Simpson and Continental Drift," *Geological Society of America Centennial Special Volume*, 1 (1985), 273–85.

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in geological surveys, where their work was of particular value for stratigraphy – although some surveys were willing to support the study of fossils in their own right. Some universities hired paleontologists, although that source of support became problematic as experimental biology gained ground in the early twentieth century. Museums were, and remained, the principal locus of paleontological activity. Throughout the nineteenth century, they served as important intellectual, educational, and social resources. Buckland and Agassiz relished the opportunity to examine Cuvier's specimens in Paris. Owen, eager to establish a British Museum of Natural History, sought valuable fossils from throughout the empire, while his counterparts in the colonies relied on the sale of specimens to develop their own museums. Marsh ran the Peabody Museum as his private domain, and Huxley and Owen made full use of their rare opportunities to view his fossil vertebrate collections. Fossil collections at college and university museums served important pedagogical purposes for scientists and students alike. By the 1920s and 1930s, however, natural history museums, at least in the United States, had become increasingly isolated. Studies by Ronald Rainger and Mary P. Winsor maintain that although museum scientists continued to teach, undertake expeditions, and conduct research, the emphasis on systematics and comparative anatomy was irrelevant to the new and quite different scientific work taking place in universities. Following World War II, new cooperative relationships were established between museums and universities, and by the 1960s, with debates over systematics and evolutionary theory, museums once again became vigorous research centers.[32]

Museums also served as centers for the development of collections and scientific careers. Outram and Appel illustrate how developments at the Paris museum had an important impact on Cuvier's life and work. Rupke notes that museum-building, not evolution, dominated Owen's interests and activities. Osborn, according to Rainger, drew on networks of social and political connections to promote his career and program at the American Museum of Natural History. Although scholars have devoted attention to career construction, the role of collections requires further study. Susan Leigh Star and James R. Griesemer indicate how a focus on specimen collections provides insight into different perspectives and social worlds within a museum. Recent studies on fieldwork suggest new opportunities for studying

[32] Susan Sheets-Pyenson, *Cathedrals of Science: The Development of Colonial Natural History Museums in the Late Nineteenth Century* (Montreal: McGill–Queens University Press, 1989); Sally Gregory Kohlstedt, "Museums on Campus: A Tradition of Inquiry and Teaching," in *The American Development of Biology*, ed. Ronald Rainger, Keith R. Benson, and Jane Maienschein (Philadelphia: University of Pennsylvania Press, 1988), pp. 15–47; Rainger, *Agenda for Antiquity*; Mary P. Winsor, *Reading the Shape of Nature: Comparative Zoology at the Agassiz Museum* (Chicago: University of Chicago Press, 1991); Ronald Rainger, "Biology, Geology or Neither or Both: Vertebrate Paleontology at the University of Chicago, 1892–1950," *Perspectives on Science*, 1 (1993), 478–519.

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how and why fossil collections were developed and what purposes they served.[33]

As centers for fossil displays, museums have also captured the attention of the public and historians. In 1803, Charles Willson Peale's mastodon exhibit generated public interest at his Philadelphia museum and abroad. Fossils were often displayed at shows, as evidenced by the dinosaurs constructed for the Crystal Palace exhibition, and became a standard feature at major public museums built in the late nineteenth century. Designed to provide scientific and educational instruction, these exhibits also served as a form of entertainment, featuring displays of large, bizarre, and ferocious animals.[34]

Museums and their displays languished for much of the twentieth century, but the situation has changed dramatically since the 1980s. Paleontology, particularly dinosaur paleontology, has been at the forefront of that development. In the 1960s and 1970s, renewed attention to dinosaur anatomy and physiology had important consequences. Claims that dinosaurs were hot blooded provoked controversy. Discoveries of new species and genera and new interpretations of dinosaur stance, locomotion, and social behavior emerged. The impact hypothesis, and its association with dinosaur extinction, increased popular interest in dinosaurs, particularly among children. The construction of a new dinosaur exhibit at the Academy of Natural Sciences of Philadelphia in the mid-1980s caused public attendance to soar, and other museums soon followed suit. Scientists, curators, and exhibitors throughout the world have since redesigned and remounted their displays, and many major museums now include laboratory exhibits describing how paleontologists work.[35]

The transformation of museums, coupled with new approaches in museology and the history of science, has resulted in much scholarly attention to those institutions. Studies by Sally Gregory Kohlstedt, Joel J. Orocz, Susan Sheets-Pyenson, and Mary P. Winsor attest to an increased historical interest in museums. Debates over the social, political, and scientific aspects of museum work have yielded new, provocative interpretations that suggest that museums are more than expressions of civic virtue designed to promote public education. Some studies examine museum construction and object collection as statements of power and authority, and others explore decisions

33 Appel, *Cuvier-Geoffroy Debate*; Outram, *Georges Cuvier*; Rupke, *Richard Owen*, pp. 12–105; Rainger, *Agenda for Antiquity*; Susan Leigh Star and James R. Griesemer, "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907–39," *Social Studies of Science*, 19 (1989), 387–420; Robert E. Kohler and Henrika Kuklick, eds., "Science in the Field," *Osiris* (2nd ser.), 11 (1996), 1–265.

34 Charles Coleman Sellers, *Mr. Peale's Museum: Charles Willson Peale and the First Popular Museum of Natural Science and Art* (New York: Norton, 1980); Adrian Desmond, "Designing the Dinosaur: Richard Owen's Response to Robert Edward Grant," *Isis*, 70 (1979), 224–34; Rainger, *Agenda for Antiquity*, pp. 152–81.

35 Elisabeth S. Clemens, "The Impact Hypothesis and Popular Science: Conditions and Consequences of Interdisciplinary Debate," in Glen, *Mass Extinction Debates*, pp. 92–120.

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about what objects to display, and how to display them, within the context of economic, curatorial, and social factors. Donna Haraway has argued that museum displays are not constructed in isolation but reflect the ideas and values of the individuals and cultures that placed such objects on display, and other historians have examined paleontological exhibits from that perspective. Desmond defines the dinosaurs displayed at the Crystal Palace as embodying Owen's interest in undermining Grant's Lamarckian views. Rainger has argued that the paleontological exhibits constructed at the American Museum of Natural History reflected not only Osborn's evolutionary interpretations but his interest in preserving an established social, political, and scientific order. These studies examine museums and displays from the perspective of scientists and administrators, and more work is needed on public perception and reaction. With increasing popular and academic interest in museums, the study of paleontology and its public role offers many new opportunities for historical analysis.[36]

[36] Winsor, *Reading the Shape of Nature*; Sheets-Pyenson, *Cathedrals of Science*; Sally Gregory Kohlstedt, ed., *The Origins of Natural Science in America: The Essays of George Brown Goode* (Washington, D.C.: Smithsonian Institution Press, 1991); Joel J. Orocz, *Curators and Culture: The Museum Movement in America, 1740–1870* (Tuscaloosa: University of Alabama Press, 1990); I. Karp and S. D. Lavine, eds., *Exhibiting Cultures: The Poetics and Politics of Museum Display* (Washington, D.C.: Smithsonian Institution Press, 1991); Peter Vergo, ed., *The New Museology* (London: Reaktion, 1991); Donna Haraway, *Primate Visions: Gender, Race and Nature in the World of Modern Science* (New York: Routledge, 1989), pp. 26–58; Desmond, "Designing the Dinosaur"; Rainger, *Agenda for Antiquity*, pp. 152–81.

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# 12

# ZOOLOGY

*Mario A. Di Gregorio*

Zoology, the study of the animal kingdom, is no longer seen as a coherent branch of science. The specialization of the twentieth century has seen zoology's territory divided among a host of separate disciplines. But in the nineteenth century that specialization was only beginning, and many naturalists would still have called themselves "zoologists," their primary concern being to gain an understanding of the animal kingdom as a whole, its diversity of structure and function, and the ways in which its component species were related.

Exploration and the description of new species continued to drive home the sheer diversity of nature: Zoologists searched for the "natural system" of relationships but disagreed over how to uncover it. Philosophical naturalists started from a priori assumptions and abstract principles, searching for unity and symmetry in the array of natural forms. Many were influenced by various forms of idealist philosophy proclaiming that nature was the manifestation of a rational Mind. Others adopted a more empirical approach, starting from the study of particular cases; these naturalists were more likely to include information on the habits, distribution, and ecological relationships of species. There were constant disagreements over the relative significance of "form" (internal biological constraints) and "function" (adaptation to the environment) in determining the structure of individual species. The advent of evolutionism transformed biologists' ideas on the nature of the relationships between species, although the theory's impact on practice is less easy to define. By the end of the nineteenth century, the attempt to create a zoological paradigm based on the reconstruction of evolutionary relationships had foundered. Research began to focus more narrowly on physiology, anatomy, embryology, and eventually on ecology and genetics, making it harder to treat zoology as a coherent whole.

At the same time, the backgrounds of the naturalists involved had become transformed. At the start of the century, many were still gentleman-amateurs, often (at least in Britain) clergyman-naturalists with a vested interest in seeing nature as a divine creation. Darwin himself owed a great deal to this tradition,



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supplemented by the growing enthusiasm for collecting in exotic locations. Zoologists from such a background would continue to make contributions – Alfred Russel Wallace (the codiscoverer of natural selection) pioneered a wave of enthusiasm for biogeography in the 1870s – but zoology was increasingly transformed into a professional discipline located in museums and universities. Morphology (the study of form or structure) became king: Comparative anatomy and embryology were used to elucidate relationships in both the pre- and post-Darwinian eras, and increasingly these were centered in the laboratory. From the Muséum d'Histoire Naturelle in Paris, which housed Jean-Baptiste Lamarck and Georges Cuvier, to the great museums that eventually graced most European capitals, professional scientists began to take over the task of description and classification using new techniques based on the microscopic study of internal structure. In Britain, Thomas Henry Huxley and his disciples used the new biology to help create the social niche occupied by professional science in the modern world. Their model was the German university system – although recent studies have shown the fragility of the situation of zoologists forced to straddle the gap between anatomy's traditional locations in medicine and science. The problem with morphology was – as its critics noted – that it dealt only with the description of dead animals. The fragmentation of zoology came about because laboratory biologists increasingly wanted to use the experimental method to study organic processes (thus transforming embryology and the study of heredity), while a new generation of field naturalists – now with their own professional identification – created disciplines such as ecology.

Historians have not treated all these developments with equal weight. The emergence of new disciplines and research programs has attracted much attention, and many of these are treated separately in this volume. The origin and impact of Darwinism has also been widely discussed as a separate issue. But to some extent the popularity of the "Darwinian revolution" has distorted the study of the history of biology. Debates that can be seen as precursors or consequences of that revolution have been given more than their fair share of attention. There has been a tendency to assume, rather uncritically, that Darwin's theory must have transformed zoological research along modern lines. In many areas, it can be argued that evolutionism merely modified existing ideas and techniques. The more revolutionary implications of Darwinism did not develop until the twentieth century. This chapter will focus on the central theoretical issues as perceived by zoologists when the field was still accepted as a coherent focus of research, including some that have been marginalized in conventional historical treatments.

## THE NATURAL SYSTEM AND NATURAL THEOLOGY

In the conventional image of the Darwinian revolution, natural history in early nineteenth-century Britain was dominated by clergyman-naturalists

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whose sole interest was to describe species as illustrations of the Creator's power and benevolence.[1] This image is by no means completely inaccurate, but it conceals the extent to which these gentlemanly specialists could make serious contributions to scientific debates. The belief that species were divinely created did not rule out a concern for the study of the relationships between species: Description related to classification, and it was still possible to explore the implications of how naturalists might set about reconstructing the divine plan of creation.

The classification system of Carl Linnaeus (1707–1778) set the pace for what was to come but posed more problems than it actually solved. The goal was to discover the natural system of relationships between species, and here some zoologists sided with Linnaeus, whereas others criticized him. What was perceived to be the main theoretical difference between the Linnaeans and their opposition was explained by John Fleming (1785–1857), an influential non-Linnaean.[2] Instead of studying internal organs, the Linnaean school referred to external characters, a useful technical device but one unable to detect the actual relationships that connected organisms; their system was not based on *real affinities*. Fleming posed the following questions: Can we discover the true affinities of animals and plants to reconstruct their real relationships and through them the order of creation established by God? Can the natural system be detected by man, and if so, what are its foundations? The champions of the natural system hoped to uncover the *essential* characters of animals beneath what were considered the more "utilitarian" characters privileged by Linnaeus. Hoping to group organisms according to the sum of their organizational properties, naturalists searched for the system that would take them beyond the apparently random differences among animals to the real essence of the ideas that guided God in making the world.

The arguments between the Linnaeans and their opponents implied a subtle theoretical difference: The Linnaeans represented a more empirical, almost "phenomenological" concept of science, reminiscent of Aristotle, in which individuals were concrete representatives of divine ideas, or, in zoology, animal types. The non-Linnaeans tended to see "natural" and "real" as synonyms and were influenced by Platonism, individuals being for them only the copies of God's ideas. Both schools, however, were convinced of the existence of finalism in nature and believed that the task of naturalists was to discover the design of divine creation. To this extent, they could see their work as compatible with the influential school of thought that took its name from clergyman William Paley's (1743–1805) *Natural Theology* (1802).

The natural theologians, including Anglican ministers such as William Kirby (1759–1850) and Darwin's teacher John Henslow (1796–1861), thought that nature showed the ends of the Creator, and hence finality pervaded

[1] See Charles C. Gillispie, *Genesis and Geology* (New York: Harper, 1959).

[2] John Fleming, *History of British Animals* (Edinburgh: Duncan and Malcolm, 1828). See Mario A. Di Gregorio, "In Search of the Natural System: Problems of Zoological Classification in Victorian Britain," *History and Philosophy of the Life Sciences*, 4 (1982), 225–54.

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nature. All natural phenomena served purposes concerning the economy of nature. There was general harmony among living things, and the purpose of all that existed in nature was *perfect* adaptation to the environment of each organism. Nature was a benevolent mechanism in which even apparently negative aspects such as death and destruction had to be interpreted positively. Each organism had its place and purpose, and our task was to discover it. Naturalists should describe all of nature's manifestations and understand their place in the design. Through detailed observation of living creatures, we may arrive at general propositions on their place in nature – this was the essence of systematics and required the discovery of the natural system.

The natural theologians privileged function over structure because they believed biological explanation was based on purpose – in this they agreed with the French naturalist Georges Cuvier.[3] But they tended to study the relationships between organisms in nature, and their best results were in the study of animal and plant habits and adaptations. Rather than in the ponderous *Bridgewater Treatises*, expected to be the great monument to the school, their achievements are to be found in short but fascinating articles on topics such as the instincts of wasps, the movements of plants, and pollination of flowers by insects. Some of these topics were later taken up by Darwin and illustrate the extent to which his concern for the interaction between the organism and its environment was inspired by this school of thought – however much he transformed its views on how those adaptations were brought about.

## THE PHILOSOPHICAL NATURALISTS

Darwin's solution to the problem of how species were related may have been the most radical, but he was by no means the only British naturalist to wish for a more philosophical approach. Inspired in part by new movements in France and Germany, a new generation sought to replace the assumption that each species was designed with only adaptation in mind. The most speculative innovations were inspired by the German movement known as *Naturphilosophie*, which encouraged a Romantic or idealist vision of nature. But working naturalists were influenced by the new spirit and attempted to synthesize traditional taxonomic concerns with the new search for underlying regularities in nature.

Perhaps the most striking manifestation of this new spirit was the brief but intense spell of popularity enjoyed by the circular, or quinarian, system of classification devised by William S. MacLeay (1792–1865). In this system, animals were classified into five groups arranged in five circles connected by

[3] See Dov Ospovat, *The Development of Darwin's Theory* (Cambridge: Cambridge University Press, 1981).

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intermediate, or osculant, groups.[4] The quinarians thought nature expressed a circular disposition and that classification should take account of such a circularity by using circles to express the affinities of animals. The numbers derived more from mathematical considerations of symmetry and harmony than from empirical considerations, on the assumption that the Creator respected mathematical rules.

Hugh Edwin Strickland (1811–1853), one of the most original zoologists in the first half of the century, was very critical of both the excessive metaphysics of *Naturphilosophie* and the artificial symmetry of quinarianism. He defined affinity, the more important relationship for a philosophical zoologist, as "the relation which subsists between two or more members of a natural group, or in other words an agreement in essential characters."[5] This proper definition of affinity would allow naturalists to reach the natural system, for which Strickland proposed a geometrical but not symmetrical image. As he wrote, "The natural system is the arrangement in which the distance from each species to every other is in exact proportion to the degree in which the essential characters of the respective species agree."[6] Strickland thought of using maps to describe affinities, after making sure they would not reflect any artificial regularity. Species had affinities with other species through ramifications in many directions rather than in a straight line or circles. In 1843, Strickland provided a map of the natural affinities of birds based on such principles.[7]

Another of Strickland's activities was his contribution to a committee set up by the Council of the British Association on zoological nomenclature, on which Darwin also worked.[8] The need to rationalize the naming of zoological groups was deeply felt at the time, and Strickland was the main inspiration for the report that recommended the rule of priority as the main criterion for zoological reformers in a field hitherto ridden by excessive numbers of synonyms and hence great confusion. The report established the grounds for all zoological classification throughout the century.

[4] W. S. Macleay, *Horae Entomologicae* (London: S. Bagster, 1819). See Philip F. Rehbock, *The Philosophical Naturalists* (Madison: University of Wisconsin Press, 1983).

[5] H. E. Strickland, "Observations upon the Affinities and Analogies of Organized Beings," *Magazine of Natural History*, 4 (1840), 219–26, at p. 221. See William Jardine, *Memoirs of the Late Hugh Edwin Strickland* (London: Van Voorst, 1858); Gordon R. McOuat, "Species, Rules and Meaning: The Politics of Language and the Ends of Definitions in 19th-Century Natural History," *Studies in the History and Philosophy of Science,* 27 (1996), 473–519; Robert J. O'Hara, "Representations of the Natural System in the 19th Century," in *Picturing Knowledge*, ed. Brian S. Baigrie (Toronto: University of Toronto Press, 1996), pp. 164–83; M. A. Di Gregorio, "Hugh Edwin Strickland (1811–53) on Affinities and Analogies: or, The Case of the Missing Key," *Ideas and Production*, 7 (1987), 35–50.

[6] H. E. Strickland, "On the Method of Discovering the Natural System in Zoology and Botany," *Annals and Magazine of Natural History*, 6 (1840–1), 184–94, at p. 184.

[7] H. E. Strickland, "Description of a Chart of the Natural Affinities of the Insessorial Order of Birds," *Report of the British Association for the Advancement of Science* (1843), 69.

[8] H. E. Strickland, "Report of a Committee Appointed to Consider the Rules by Which the Nomenclature of Zoology May Be Established on a Uniform and Permanent Basis," *Report of the British Association for the Advancement of Science* (1842), 105–21; F. Burkhardt and S. Smith, eds., *The Correspondence of Charles Darwin* (Cambridge: Cambridge University Press, 1986), vol. 2, pp. 311, 320.

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The British move toward a more "philosophical" approach reflected an awareness of initiatives taking place on the Continent. In France, the newly reorganized Paris museum became a center of both research and controversy, well represented by the debates between Georges Cuvier (1769–1832) and his two rivals Jean-Baptiste Lamarck (1744–1829) and Etienne Geoffroy Saint-Hilaire (1772–1844). Lamarck's evolutionism is now known to have had more influence in the pre-Darwinian era than historians once imagined. Although it promoted a natural explanation of adaptation, it was based on traditional ideas and included a serial progression in the history of life on earth. Radical political thinkers stressed what they perceived to be its materialistic implications, as in the case of the comparative anatomist Robert E. Grant (1793–1874), who was eventually marginalized within the British scientific scene.[9]

The philosophical anatomy of Geoffroy proclaimed that structure determined function and that all living things had been formed according to one structural plan, of which all animals were variations. An organ could vary in different forms but never transposed from its natural position; thus, if we could discover the correct connection of various organs (the "law of connection"), we would be able to outline the abstract ideal type in which each organ existed in the highest stage of its intrinsic characteristics. That type would be the scheme of all possible transformations of each organ. If we compared vertebrates with crustaceans, we would see how each part of a vertebrate corresponded to one of a crustacean, as if vertebrates and crustaceans were variations of a single ideal animal.[10]

Georges Cuvier rejected both Lamarck's transformism and Geoffroy's search for unity. Cuvier's view of anatomy was diametrically opposed to that of Geoffroy because he insisted on the primacy of function. Function determined structure, so that from a function we could infer the structure that fulfilled that function ("the principle of correlation"). From the observation of the real conditions of existence of organisms, we could reach general conclusions on their characteristics and relationships. A good classification had to focus on subordination of characters – structures and properties more influential for the existence of organisms should be the dominant features of classification. For Cuvier, these were the brain and nervous system and the heart and circulatory system. On such grounds, four completely separate types (*émbranchements*) could be detected: vertebrates, molluscs, articulates, and radiates. Each animal belonged to one of these types, each type presenting all possible variations allowed by the limits established by the conditions of

[9] Pietro Corsi, *The Age of Lamarck: Evolutionary Theories in France, 1790–1830* (Berkeley: University of California Press, 1988); Adrian Desmond, *The Politics of Evolution* (Chicago: University of Chicago Press, 1989).

[10] Toby A. Appel, *The Cuvier-Geoffroy Debate: French Biology in the Decades before Darwin* (Oxford: Oxford University Press, 1987); E. S. Russell, *Form and Function* (London: John Murray, 1916), pp. 52–78.

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existence. Whereas Geoffroy emphasized the unity of nature, Cuvier granted greater scope to variety, although he held that individual species were completely fixed.[11]

In Germany, philosophical considerations led a whole generation of naturalists to search for underlying patterns in nature under the banner of *Naturphilosophie*. Although *Naturphilosophie* was widely dismissed as mere nature mysticism, historians have shown that it was a more complex movement.[12] Its less metaphysical wing was influenced by Immanuel Kant (1724–1804) and included Karl Ernst von Baer (1792–1876), Johannes Mueller (1801–1858), and J. F. Blumenbach (1752–1840). The most aggressive and controversial school was influenced by the idealist philosophy of F. W. J. von Schelling (1775–1854) and included Lorenz Oken (1777–1851). In spite of these theoretical differences, *Naturphilosophie* was perceived as an antiempirical, idealistic, and Romantic approach to natural science.

The supporters of *Naturphilosophie* were convinced that science could be deduced from abstract a priori concepts. Life was the constant manifestation of an internal principle through outward forms. *Naturphilosophie* insisted on the symmetry of nature, and the perfect being was conceived as a sphere, from which real beings departed to a greater or lesser extent. There was a hidden bond that exhibited the highest relationships of unity: Animals and plants came from an egg and then developed, and thus embryology provided the unity of living things. There was continuity from plants to animals, a point particularly reinforced by the study of infusoria, organisms thought to be intermediate between animals and plants, on which C. G. Ehrenberg (1795–1876) was the acknowledged authority.

## THE TRIUMPH OF TYPOLOGY

The aspect of *Naturphilosophie* that was judged most useful by the following generations of naturalists was the role accorded to embryology. After Cuvier, it was clear that in order to understand the whole plan of creation and therefore to outline the foundations of the natural system, the zoologist must know the type of organization to which an animal could be referred. Whereas Cuvier had based his four types of organizations on anatomical grounds, Karl Ernst von Baer had inherited from his Naturphilosophical background the view that it was embryological development that provided the best means to understand the characteristics of the four types and to obtain correct classifications, thus

11 Russell, *Form and Function*, pp. 31–44; William Coleman, *Georges Cuvier, Zoologist* (Cambridge, Mass.: Harvard University Press, 1964); Michel Foucault, *The Order of Things* (New York: Pantheon, 1970).

12 Timothy Lenoir, *The Strategy of Life* (Chicago: University of Chicago Press, 1982); D. von Engelhardt, *Historisches Bewusstsein in der Naturwisssenschaft von der Aufklaerung bis zum Positivismus* (Freiburg: Alber, 1979).

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establishing embryological typology. The use of embryology to understand structure and affinity promoted the trend – already started by comparative anatomy – to move zoology from the field to the laboratory. Zoologists still collected specimens, but their aim was dissection and the analysis of structure rather than the study of the species in its natural environment. The museum, and increasingly the university, became the locus of zoological research.

Typical of the movement to apply embryology to zoological classification was the work of Henri Milne-Edwards (1800–1885) in France. He argued that because embryos resembled each other more than the subsequent adult forms, it was embryology that indicated affinities and revealed what pure comparative anatomy could not: that affinities in adults were often obscured by adaptive modifications, striking in appearance but unimportant to establishing relationships.[13] Like von Baer, Milne-Edwards thought that development consisted in departure from a common type. On these principles, he outlined classifications of vertebrates, especially mammals. He conceived nature as the result of degrees of perfection: An increase in the perfection of function would lead to the perfection of animal organization through the division of labor as organs became more differentiated.

In Germany, Johannes Mueller linked the study of organic form (morphology) with physiology under the influence of a finalistic view of nature with strong religious and Romantic overtones.[14] Mueller gave great impetus to marine invertebrate zoology, and his expeditions to the seaside inspired the founding of marine zoological stations, where animals would be observed in their environment and then studied in laboratories. He discovered the larval forms of echinoderms and molluscs, thus reinforcing to a decisive extent the role of embryology in zoology. His study of fishes helped him to understand the morphological boundaries of animal classes, a milestone in his program of research that he hoped would show that it was in the great systematic groups that one could find the essence of animal organization. Mueller was sympathetic to the cell theory of his disciple Theodor Schwann (1810–1882).[15]

In Britain, Richard Owen (1804–1892) synthesized elements from *Naturphilosophie*, Geoffroy's transcendental morphology, and Cuvier's comparative anatomy.[16] What the natural theologians had called affinity, he redefined as "homology": "Homologue – the same organ in different animals under every variety of form and function."[17] Homology represented resemblances of structures caused by a similarity in the plan of organization of animal forms.

[13] H. Milne-Edwards, "Considérations sur quelques principes relatifs à la classification naturelle des animaux," *Annales des sciences naturelles*, 3 (1844), 65–99.

[14] W. Haberling, *Johannes Mueller: Das Leben des rheinischen Naturforschers* (Leipzig: Akademische Verlagsgesellschaft, 1924).

[15] B. Lohff, "Johannes Muellers Rezeption der Zellenlehre in seinem 'Handbuch der Physiologie der Menschen'," *Medizinhistorisches Journal*, 13 (1978), 248–58.

[16] Russell, *Form and Function*, pp. 102–12. On Owen and von Baer, see Dov Ospovat, "The Influence of Karl Ernst von Baer's Embryology, 1828–1859," *Journal of the History of Biology*, 9 (1976), 1–28.

[17] Richard Owen, *Lectures on Invertebrate Animals* (London: Longmans, 1843), p. 379.

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The underlying type based on such homologies Owen called the "archetype." This he endeavored to outline especially in his studies of vertebrates. He thought that vertebrate homologies led zoologists to discern an ideal vertebrate archetype, based on constancy of characters, to which all variation had to be referred. Vertebrates as we know them had to be considered as derivations from the archetype. The fish was a relatively uncomplex vertebrate that departed from the archetype to a lesser extent than other vertebrates; therefore it was a useful form in which to study the vertebrate type. Owen knew of Baer's embryology but used it mainly as mere support for his anatomical work. Originally his archetype was conceived in Aristotelian terms, but later, possibly under pressure from his conservative associates in England, he turned to a more Platonic concept that enabled him to present the new morphology as compatible with belief in a rational Designer.[18] Owen was a typical museum-based zoologist with strong links to the medical tradition of comparative anatomy, beginning his career at the Hunterian Museum of the Royal College of Surgeons and later playing a major role in the creation of the modern Natural History Museum in London.[19]

Another leading typologist was Owen's lifelong rival Thomas Henry Huxley (1825–1895). Huxley gained his reputation by describing and classifying the species collected on the voyage of HMS *Rattlesnake*. He endorsed von Baer's views (he translated part of von Baer's major book) and employed embryological typology in his work on invertebrate zoology. In his studies of cephalopods, ascidians, and jellyfish, he applied embryological methods in order to discover their homologies. He interpreted von Baer's types in a radically discontinuous manner, a view he maintained throughout his career. Huxley tried to apply the type concept as a mere practical device, as devoid as possible of its idealistic presuppositions but rather like a useful tool summarizing and embodying all characters of animals that could be grouped together.[20] Huxley's assault on Owen's Platonic archetype has been interpreted as part of his campaign to establish science as a new source of authority in British culture.[21]

[18] Nicolaas A. Rupke, "Richard Owen's Vertebrate Archetype," *Isis*, 84 (1993), 231–51; Nicolaas A. Rupke, *Richard Owen: Victorian Naturalist* (New Haven, Conn.: Yale University Press, 1994); J. W. Gruber and J. C. Thackaray, *Richard Owen Commemoration* (London: Natural History Museum, 1992); Philip R. Sloan, ed., *Richard Owen: The Hunterian Lectures in Comparative Anatomy* (London: Natural History Museum, 1992).

[19] W. T. Stearn, *The Natural History Museum at South Kensington* (London: Heinemann, 1981); Adrian Desmond, *Archetypes and Ancestors* (London: Blond and Briggs, 1982).

[20] T. H. Huxley, "On the Morphology of the Cephalous Mollusca" (1853), reprinted in T. H. Huxley, *Scientific Memoirs* (London: Macmillan, 1898–1902), vol. 1, pp. 152–93; T. H. Huxley, *The Oceanic Hydrozoa* (London, 1859); T. H. Huxley, "Fragments Relating to Philosophical Zoology, Selected from the Works of K. E. von Baer," *Taylor's Scientific Memoirs, Natural History*, 3 (1853), 176–238. See M. A. Di Gregorio, *T. H. Huxley's Place in Natural Science* (New Haven, Conn.: Yale University Press, 1984); Mary P. Winsor, *Starfish, Jellyfish, and the Order of Life* (New Haven, Conn.: Yale University Press, 1976).

[21] Desmond, *Archetypes and Ancestors*. See also Adrian Desmond, *Huxley: The Devil's Disciple* (London: Michael Joseph, 1994).

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Swiss-born zoologist Louis Agassiz (1807–1873) worked for a while in Munich, where he came across Schelling's *Naturphilosophie*; he later emigrated to the United States to become the leading nonevolutionary zoologist of his time and founder of the influential Museum of Comparative Zoology at Harvard.[22] Agassiz applied the results of embryology to paleontology; the fish of the Old Red Sandstone represented the embryological stage of the fish type, showing that the type followed the same creative pattern in the development of the individual and in the history of life on earth. He maintained this approach when he attempted a great theoretical work, the *Essay on Classification* (1859), which was perceived by many, including the young Ernst Haeckel, as the main theoretical alternative to Darwin's *On the Origin of Species* (1859). For Agassiz, a radical idealist, the creative idea that he saw running through the animal world guaranteed that species and higher taxonomic groups existed as ideal categories of the Supreme Intelligence.

## FROM DARWIN TO EVOLUTIONARY TYPOLOGY

Although the theory of evolution proposed by Charles Darwin (1809–1882) was to have an immense impact on the new scientific zoology, it included elements derived from the older tradition of field studies, which were difficult for the laboratory-based biologists to assimilate. The theory of common descent transformed the morphologists' search for the underlying source of unity within groups, but Darwin's interest in local adaptation and the effects of geographical distribution were of more interest to collectors working within the old natural history tradition. The details of how Darwin developed his theory are given elsewhere (see Hodge, Chapter 14, this volume); what follows is an overview of how the theory influenced the zoology of the late nineteenth century.

Darwin worked under the supervision of the Lamarckian evolutionist Robert Grant at Edinburgh, and this had great influence on his early zoological work on the bryozoan *Flustra*.[23] At Cambridge, he was introduced to the natural theology tradition by Henslow and others, while the *Beagle* voyage focused his attention on biogeography and the adaptation of species to their environment. On his return to England, his specimens were inspected by the leading naturalists of the time, including Owen, and the *Zoology of the Beagle* helped to make his name among his colleagues.[24]

22 M. P. Winsor, *Reading the Shape of Nature* (Chicago: University of Chicago Press, 1991); Edward Lurie, *Louis Agassiz: A Life in Science* (Chicago: University of Chicago Press, 1960).

23 Philip R. Sloan, "Darwin's Invertebrate Program, 1826–1836: Preconditions for Transformism," in *The Darwinian Heritage*, ed. David Kohn (Princeton, N.J.: Princeton University Press, 1985), pp. 71–120. On Darwin's early career, see Janet Browne, *Charles Darwin: Voyaging* (London: Jonathan Cape, 1995).

24 Charles Darwin, ed., *The Zoology of the Voyage of H.M.S. Beagle*, 5 pts. (London: Smith, Elder, 1838–43).

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From the late 1830s, Darwin began to explain zoological problems in terms of evolutionary theory. This was especially clear in his long and detailed work on cirripedes (or barnacles), his individually most distinguished contribution to zoology.[25] This research allowed Darwin to improve his understanding of scientific nomenclature, which he had recently approached in his collaboration with Strickland's committee. From there he could move to theoretical problems and test his views on the species question. By then, Darwin had reached some fundamental conclusions on classification that the barnacles helped to clarify: Homology revealed true genetic relationships rather than similarities of structures caused by a common basic type of organization.

Embryology, which Darwin had particularly appreciated in Milne-Edwards's work, helped him to reinterpret the archetype as the historical ancestor of living forms – the archetypal cirripede was the ancestral cirripede. Moreover, the barnacles illustrated the loss of useless organs and the abortion of parts in nature, and the transformation organs visible in barnacles suggested the occurrence of the change of functions of organs in evolution, a concept of vital importance in Darwin's theory. All of this was used in *Origin of Species*, in which he made clear that the natural system was founded on descent with modifications. All true classification was genealogical, representing an abridged version of the course of evolution.

Darwin's later studies, such as his work on earthworms, retained the natural theologians' interest in animal instincts, habits, and adaptations.[26] The influence of Charles Lyell (1797–1875) and Alexander von Humboldt (1769–1859) had focused his attention on geographical distribution as a key to approach the origin of species.[27] The study of the geography of living forms – biogeography, as it came to be called – also formed a central aspect of the research of the codiscoverer of natural selection, Alfred Russel Wallace (1823–1913). Wallace, like Darwin, realized how the struggle for existence was related to the distribution of species and, more broadly, to the balance of nature. He then studied how geographical barriers were related to speciation and drew a line – still called Wallace's line – across Indonesia to divide the Asian from the Australian faunas.[28] Following the publication of Wallace's book *The Geographical Distribution of Animals* (1876), the reconstruction of migrations from centers of origin became a major research program.[29]

[25] C. Darwin, *Monograph of the Sub-class Cirripedia* (London: Ray Society, 1851); Burkhardt and Smith, *Correspondence of Charles Darwin*, vol. 4, 1988, pp. 388–409. See M. T. Ghiselin, *The Triumph of the Darwinian Method* (Berkeley: University of California Press, 1969).

[26] C. Darwin, *The Formation of Vegetable Mould, Through the Action of Worms, with Observations on their Habits* (London: John Murray, 1881).

[27] M. J. S. Hodge, *Origins and Species* (New York: Garland, 1991).

[28] Janet Browne, *The Secular Ark: Studies in the History of Biogeography* (New Haven, Conn.: Yale University Press, 1983).

[29] P. J. Bowler, *Life's Splendid Drama: Evolutionary Biology and the Reconstruction of Life's Ancestry, 1860–1940* (Chicago: University of Chicago Press, 1996), chap. 8.

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In the years following the publication of Darwin's *Origin of Species*, a number of zoologists, including Huxley, Haeckel, and Anton Dohrn (1840–1909), claimed to have been either converted to or inspired by Darwin's theory of species. Peter Bowler and other historians have challenged the traditional view of Darwin's influence on nineteenth-century natural science and have claimed that in the actual *scientific* work of many zoologists, the influence of Darwin's theory was less visible than usually thought. Michael Bartholomew began a revisionist historiography of Huxley, and Jacques Roger has pointed out the pre-Darwinian elements in Haeckel's worldview. Robert J. Richards, on the other hand, insists on a community of views between Darwin and Haeckel. In fact, natural selection does not seem to have been widely applied by most so-called Darwinians – hence Bowler's term "pseudo-Darwinians."[30]

These tensions can be seen in the school of evolutionary morphology founded by the anatomist Carl Gegenbaur (1826–1903) and popularized by Ernst Haeckel (1834–1919).[31] Gegenbaur intended to turn idealistic morphology into a more modern discipline, and although to do this he eventually turned to evolution theory, his primary interests remained centered on the type concept and its implications for homology. Morphology explored how forms arose and developed and the character of their mutual relations. It could therefore reach general theories based on the empirical study of form in its dynamic context as revealed by embryology. Morphology could make sense of the order of nature because it was based on the results of the philosophically sound method of comparison. Thanks to comparative anatomy and embryology, Gegenbaur was sure he could reform Owen's concept of homology. To do this, he needed some input from a more broadly based zoology and asked the young Haeckel to join him at Jena. Together they created an influential research program – although we now know that their position in the German university system was by no means as comfortable as envious foreigners (such as Huxley) imagined.[32]

Just before moving to Jena, Haeckel had produced, while he was working by the shores of the Mediterranean, a ponderous monograph on radiolarians that followed Mueller's methodology. Then both he and Gegenbaur read the German translation of *Origin of Species* and realized that their reform of morphology must accommodate evolution. In 1870, Gegenbaur revised his

[30] P. J. Bowler, *The Non-Darwinian Revolution* (Baltimore: Johns Hopkins University Press, 1988); Michael Bartholomew, "Huxley's Defence of Darwinism," *Annals of Science*, 32 (1975), 525–35; Jacques Roger, "Darwin, Haeckel et les francais," in *De Darwin au darwinisme: Science et idéologie*, ed. Yvette Conry (Paris: J. Vrin, 1983), pp. 149–65; Robert J. Richards, *The Meaning of Evolution* (Chicago: University of Chicago Press, 1991).

[31] M. A. Di Gregorio, "A Wolf in Sheep's Clothing: Carl Gegenbaur, Ernst Haeckel, the Vertebral Theory of the Skull, and the Survival of Richard Owen," *Journal of the History of Biology*, 28 (1995), 247–80.

[32] E. Krausse, *Ernst Haeckel* (Leipzig: Teubner, 1987); G. Uschmann, *Geschichte der Zoologie und der zoologischen Anstalten in Jena, 1779–1919* (Jena: Gustav Fischer, 1959); Lynn K. Nyhart, *Biology Takes Form: Animal Morphology and the German Universities, 1800–1900* (Chicago: University of Chicago Press, 1995).

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textbook of comparative anatomy, the first edition of which – published a few months before *Origin of Species* – had been conceived in the tradition of idealistic morphology. He now turned the old archetypal patterns into the reconstruction of evolutionary genealogies.[33] The key to the order of nature had been found in the development of form through time. The types developed historically, so the systems of Oken and Owen became historically genetic, and the comparative method connected changes of form through the concept of homology. The natural system was a typology based on descent theory but preserving von Baer's embryological interpretation of the types.

Haeckel made a vital contribution to Gegenbaur's program: His concept of "phylogeny" linked the traditional concerns of morphology (homology and the type) to the new notion of descent by prioritizing the concept of "the evolutionary history of a group." The companion term "ontogeny" denoted the process of individual development, and the formula "ontogeny recapitulates phylogeny" – the "biogenetic law" – connected two poles of the new conceptual apparatus in the thesis that in the formal aspects of its development the organism passed through successive transformations that constituted the history of its type, revealing its own phylogenetic descent.[34] Thus the concept of "phylogeny" asserted that descent theory should primarily study the evolution of form and should do this through study of the formal aspects of development. According to the developmentalist tradition, the adult form of the organism developed from the first cells of the embryo by an inexorable process of multiplication, differentiation, and maturation, governed by "laws of growth." A new form could arise only by an addition to the established growth pattern. Evolutionary change then took place by natural selection between such forms. For Haeckel, natural selection did take place, but among types rather than among individuals. This program did not seem to correspond to Darwin's main preoccupations in *Origin of Species*. There the dominant images were those of ubiquitous mutability and insensible gradation, which were not obviously "type-friendly" notions. Many historians see natural selection as threatening the concept of inexorability of development, although this view is not shared by Richards.[35] Both with the radiolarians and in his evolutionary publications, Haeckel had presented a view of the order of nature based on geometrical symmetry, certainly not a Darwinian concept. In his classification of siphonophores, he produced not a sample of Darwinian methodology, as he claimed, but a reinforcement of earlier views of animal relations, especially Karl Leuckart's (1822–1898) view of polyformism,

33 Carl Gegenbaur, *Grundzuege der vergleichenden Anatomie*, 1st ed. (Leipzig: Wilhelm Engelman, 1859), 2nd ed. (Leipzig: Wilhelm Engelman, 1870). See William Coleman, "Morphology between Type Concept and Descent Theory," *Journal of the History of Medicine*, 31 (1976), 149–75.

34 M. A. Di Gregorio, *From Here to Eternity: Ernst Haeckel and Scientific Faith* (Goettingen: Van den hoek and Ruprecht, 2005); Bowler, *Life's Splendid Drama*.

35 Richards, *Meaning of Evolution*.

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in which individuals of colonial animals were modified according to their different roles in their colony on the principle of division of labor.[36]

No evolutionary typology would have existed, however, without the decisive intervention of Darwin's concept of descent and even of natural selection. This provided the causal explanation of evolution, avoiding Mueller's teleology but allowing Haeckel to develop his concept of phylogeny. Perhaps, rather than "Darwinians" or "pseudo-Darwinians," Haeckel and Gegenbaur should be defined as "semi-Darwinians." The concept of phylogeny provided a significant reinterpretation of idealist morphology, forcing its exponents to think in terms of real transformations. Inspired by this movement, a generation of morphologists sought to create a scientific evolutionism. Gegenbaur's disciple Max Fuerbringer (1846–1920) enlarged the program to obtain morphological relations between fossil, embryological, and adult forms in his ornithological work. Another member of Gegenbaur's school, Hans Gadow (1855–1928), emigrated to Britain and worked on a morphological interpretation of biogeography.[37]

The typological approach was still prominent in the zoology that T. H. Huxley used to transform the teaching of biology in Britain. After 1859, Huxley sided with Darwin in public debates on the species theory, but it was only in the late 1860s, possibly influenced by Haeckel, that he used evolutionary thinking in his zoological work, especially on the origin and development of birds and crocodiles. He applied the descent theory but made no use of natural selection.[38] Huxley always maintained the type concept, especially in his teaching, although it was defused of the idealist metaphysic. He took examples of a few types of animals to be studied as illustrations of the animal kingdom, so that the analysis of a crayfish, as representative of the crustacean type, could be treated as typical of all crustaceans.[39] Evolutionary theorizing was still too speculative for the students.

## TENSIONS WITHIN EVOLUTIONISM

Phylogenetic research seemed to offer a new foundation for zoology, transforming ideas about structural relationships and classification. But the project foundered, partly because the reconstruction turned out to be impossible for

[36] M. P. Winsor, "A Historical Consideration of the Siphonophores," *Proceedings of the Royal Society, Series B*, 73 (1971–2), 315–23.

[37] Hans Gadow, *A Classification of Vertebrates, Recent and Extinct* (London: A. and C. Black, 1875). See Bowler, *Life's Splendid Drama*.

[38] M. A. Di Gregorio, "The Dinosaur Connection: A Reinterpretation of T. H. Huxley's Evolutionary View," *Journal of the History of Biology*, 15 (1982), 397–418; Di Gregorio, *T. H. Huxley's Place in Natural Science*.

[39] T. H. Huxley, *The Crayfish: An Introduction to the Study of Zoology* (London: Kegan Paul, 1879). The limited extent to which evolutionism was used in Huxley's educational program is noted in Adrian Desmond, *Huxley: Evolution's High Priest* (London: Michael Joseph, 1997).

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technical reasons and partly because there were factors directing biologists toward other interests. Reinterpreting homology along phylogenetic grounds proved difficult because adaptive pressures can sometimes produce similar structures in different branches of evolution. The evolutionary morphologists' lack of interest in those same adaptive pressures was seen by some as a betrayal of the key Darwinian insight. And the link with physiology, repudiated by Gegenbaur but of interest to many laboratory-based zoologists, pushed many toward new questions such as the mechanical causes of embryological differentiation.

Several German zoologists followed an evolutionary approach to their discipline but were critical of Gegenbaur's program. Karl Semper (1832–1893) disagreed with the subordination of zoology to morphology and held a chair of Comparative Anatomy and Zoology at Wuerzburg, thus emphasizing the equal status of both disciplines. He insisted that a result of Darwin's doctrine was to make zoology a scientific discipline in its own right. For Semper, comparative anatomy had no right to speak for scientific zoology or to determine genealogical connections. Haeckel should not have accepted the subordination of his wider zoological interests to Gegenbaur's program. Semper's interest in physiology led him to study the effects of the environment on the organism in a book that played a role in the eventual founding of ecology.[40] Carl Claus (1835–1899), professor at Vienna, criticized Haeckel for not basing his taxonomy on objective grounds. He conceded a major role for morphology but refused to accept what he considered Haeckel's fanciful phylogenies.[41]

Anton Dohrn studied with Gegenbaur and Haeckel at Jena but soon clashed with them both on personal and scientific grounds.[42] After reading Friedrich Albert Lange's *Geschichte der Materialismus*, he concluded that the theoretical background of Haeckel's research was unsound. He criticized the view proposed by Alexander Kovalevsky (1840–1901) and supported by Haeckel and Gegenbaur that the vertebrates had originated from ascidians, claiming instead that they had descended from annelid worms. Dohrn arrived at these conclusions by starting from the highly metaphysical views of Geoffroy, who, contrary to Cuvier and von Baer, had referred to one general plan of organization of all animals, of which different plans were the derivations. Thus he turned Geoffroy's atemporal derivation into evolutionary descent.

Dohrn had started with a theory of the descent of insects from crustaceans. This was unsuccessful but provided good evidence for gradations and intermediate forms and placed the cirripedes in a central position – both Darwin-friendly concepts. It was in his attempt to prove his annelid theory, however, that Dohrn provided Darwin with useful ammunition. Dohrn claimed that

40 Karl Semper, *Der Haeckelismus in der Zoologie* (Hamburg, 1876); Karl Semper, *The Natural Conditions of Existence as They Affect Animal Life* (London: Kegan Paul, 1881).
41 Carl Claus, *Grundzuege der Zoologie* (Marburg: Elwertsche, 1868).
42 Theodor Heuss, *Anton Dohrn: A Life for Science* (New York: Springer, 1991).

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the passage from the annelid to the vertebrate form had been made possible by a change of function: In the course of descent, each organ had not only its principal function but also other functions that worked when conditions required them. In changed conditions, the secondary function could become primary, explaining how natural selection would not destroy forms in their intermediate stages of descent.[43] This was crucial to Darwin's argument that natural selection was not merely a destructive force, which Darwin had used in reply to St. George Mivart's (1827–1899) criticism on that point.[44]

Dohrn's greatest contribution to the progress of zoology was the foundation of the zoological station at Naples, where generations of zoologists had the opportunity to study marine animals – the realization of Mueller's project.[45] The work done at Naples, however, showed how difficult it was for zoology to survive as an independent discipline. Rather than moving toward morphology, the trend was toward a physiologically inclined program. Huxley, albeit a morphologist, encouraged a physiology as performed in the laboratory of his disciple Michael Foster (1836–1907).[46] Other students of Huxley carried on the morphological tradition, and one, Francis Balfour (1851–1882), was inspired by Gegenbaur and the Naples station to produce a synthesis between the physiological and morphological approaches. He saw how embryology could be used to reconstruct evolutionary descent but was aware of how the physiological requirements of the developmental process could obscure the evidence. Balfour died too young to complete his program, and many of his followers turned away from morphology.

Huxley's other distinguished disciple, Edwin Ray Lankester (1847–1929), may be seen as the last zoologist in the old sense of the term.[47] He was convinced that embryology was the key to the interpretation of natural science and rejected Owen's idealism in favor of more Darwinian views. He proposed that Owen's "homology" should be replaced by two terms, "homogeny" and "homoplasy" – the latter covering the production of similar structures in separate lines by convergent evolution.[48] Recognition of the widespread occurrence of homoplasy eventually undermined the project to reconstruct the genealogical relations of animals. Lankester supported the view of natural classification as a genealogical tree based principally on the

[43] Anton Dohrn, trans. M. T. Ghiselin, "The Origin of Vertebrates and the Principle of Succession of Functions," *History and Philosophy of the Life Sciences*, 16 (1993), 1–98. See Bowler, *Life's Splendid Drama*.

[44] St. G. Mivart, *On the Genesis of Species* (London: Macmillan, 1871); Darwin, *On the Origin of Species*, 6th ed. (London, 1872), chap. 6.

[45] I. Mueller, *Die Geschichte der zoologischen Stazion in Neapel* (PhD diss., Duesseldorf, 1976); Christiane Groeben et al., "The Naples Zoological Station," *Biological Bulletin* (*Supplementary volume*), 168 (1985).

[46] G. L. Geison, *Michael Foster and the Cambridge School of Physiology* (Princeton, N.J.: Princeton University Press, 1978).

[47] J. Lester and P. J. Bowler, *E. Ray Lankester and the Making of Modern British Biology* (Stanford in the Vale: British Society for the History of Science, 1995).

[48] E. R. Lankester, "On the Use of the Term Homology in Modern Zoology, and the Distinction between Homogenic and Homoplastic," *Annals and Magazine of Natural History*, 6 (1870), 34–43.

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phylogenetic law by producing an evolutionary version of embryological typology. Animals went through a series of stages during each of which they resembled one of their ancestors. Thus embryology was the resumé of evolution, and genealogical classification had to be based on it. Embryology was decisive in showing that there was an intermediate group, the ascidians, of great evolutionary significance, between invertebrates and vertebrates. Lankester understood the dominant role of physiology for contemporary biology and had studied in Leipzig with Karl Ludwig (1816–1895), but he remained faithful to morphology. He believed the chemical properties of life would provide the ultimate explanation of organisms but played no part in the emergence of molecular biology. In his later career, he was a prominent supporter of natural selection, although his lack of interest in the newly emerging genetics limited his impact on the development of twentieth-century Darwinism.

Lankester had founded an influential research school at University College London, and later became the director of the Natural History Museum. The crowning project of his scientific career was to be the *Treatise on Zoology*, which he edited. The first volume appeared in 1900, but the project was interrupted after eight volumes, as if the morphological zoology it presented had exhausted its strength. The *Treatise* concluded the epoch opened by Linnaeus's search for a natural system of relationships; in principle, it could now be seen that the natural system was genealogical, based on embryological typology, although in practice the system was difficult to reconstruct, and many biologists were losing interest in it.

## INTO THE TWENTIETH CENTURY

The nineteenth-century tradition of zoology reached its zenith with evolutionary morphology and the disciplines associated with it. This tradition survived into the twentieth century but was rapidly eclipsed by the emergence of new approaches in the life sciences that made "zoology" a less relevant category. The rise of experimentalism, and the consolidation of new areas such as microbiology and ecology, made the division between the studies of the animal and plant kingdoms seem somewhat artificial. Nevertheless, the discipline of zoology retained a place in the academic curriculum and the scientific community much longer than one might have expected. Ecologists and geneticists still worked within departments of zoology at many universities, and museums, too, retained the traditional distinctions based on the animal, plant, and mineral kingdoms. Only in the late twentieth century did zoology completely lose its role as a significant category of biology.

Morphology, which is more a method of work than a specific discipline, survived in the twentieth century and is still practiced, but lost its central position in the life sciences. Gegenbaur's school reverted to the idealism that he had tried to transform by replacing the geometrical transformations of Owen

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and Geoffroy with real historical descent.[49] The physiological approach to zoology favored at the Naples station won the day over pure morphology, and Haeckel's influence faded. Lankester's disciple Edwin S. Goodrich (1868–1946) continued to promote morphology at Oxford and made some efforts to come to terms with the newly emerging Darwinian synthesis, but in general the use of embryos as clues to ancestry was marginalized within evolutionary studies.[50]

Embryology now moved toward the study of the processes at work in development (see Hopwood, Chapter 16, this volume). Several morphologists turned from evolutionary studies to heredity and played a role in the founding of genetics (see Burian and Zallen, Chapter 23, this volume). William Bateson (1861–1926), a product of the Balfour school at Cambridge, abandoned work on the ancestry of the vertebrates for the study of discontinuous variations and heredity. Another product of the same school, W. F. R. Weldon (1860–1906), pioneered the study of variation in wild population and used statistical studies to verify the workings of natural selection. When linked to the emerging population genetics, this work paved the way for the synthesis of Darwinism and genetics that was to dominate evolutionism from the 1940s onward. Weldon's interest in the study of populations in their natural habitat paralleled other manifestations of the desire to place field studies on a more "scientific" basis, thus breaking the monopoly of the laboratory-based disciplines. Biogeography had flourished in the late nineteenth century and now fed into the study of the genetic structure of populations. Fieldworkers such as Ernst Mayr (1904–2005) studied the effects of geographical isolation and were able to relate their work to the developments in population genetics and the theory of natural selection (see Hodge, Chapter 14, this volume). Ecology, a term coined by Haeckel, also became important (see Acot, Chapter 24, this volume). Linked to this was the emergence of a scientific ethology (the study of animal behavior) – Julian Huxley (1887–1975), another founder of modern synthetic Darwinism, did important early work on the evolutionary explanation of bird behavior.

In many ways, the emergence of these new research programs threatened the unity once imposed by the category "zoology" when the study of animal form had been paramount. Yet the new programs were often pioneered

[49] A. Naeff, *Idealistische Morphologie und Phylogenie* (Jena: Gustav Fischer, 1919); E. Lubosch, "Geschichte der vergleichenden Anatomie," in *Handbuch der Anatomie der Wirbelthiere*, ed. L. Bolk et al., 7 vols. (Berlin: Urban und Schwarzenberg, 1931–8), vol. 1, pp. 3–76; D. Starck, "Die idealistische Morphologie und ihre Wirkung," *Medizinhistorisches Journal*, 15 (1980), 44–56; D. Starck, "Vergleichende Anatomie der Wirbelthiere von Gegenbaur bis heute," *Verhandlungen der deutschen zoologischen Gesesselschaft Jena* (1966), 51–67.

[50] See W. Coleman, "Morphology and the Evolutionary Synthesis," in *The Evolutionary Synthesis*, ed. E. Mayr and W. Provine (Cambridge, Mass.: Harvard University Press, 1980), pp. 174–80; M. T. Ghiselin, "The Failure of Morphology to Assimilate Darwinism," in Mayr and Provine, *Evolutionary Synthesis*, pp. 180–93; Garland E. Allen, *Life Science in the Twentieth Century* (Cambridge: Cambridge University Press, 1979).

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within traditionally named and structured departments, so the term "zoology" remained in common use through the first half of the twentieth century, at least for organizational purposes. Universities had departments of zoology with senior professors who would have identified strongly with the old tradition, even when their more creative junior colleagues were founding new research programs. T. H. Huxley had attempted to popularize the more general term "biology" in the late nineteenth century as part of his campaign to distance the new laboratory disciplines from the old natural history tradition.[51] This move had some effect in redefining academic programs, especially in the new American research universities such as Johns Hopkins and Chicago. But the category of zoology often survived, even if within the more general remit of a biology program. The authors of the well-known text *Principles of Animal Ecology* (1949) were all identified as zoologists – W. C. Allee, Alfred E. Emerson, and Thomas Park were professors of zoology at Chicago, Orlando Park was professor of zoology at Northwestern, and Karl P. Schmidt was chief curator of zoology at the Chicago Natural History Museum.[52]

This last point reminds us that many museums also continued the traditional divisions, allowing zoology to retain its umbrella-like role covering a variety of animal studies. Societies kept the tradition alive, too: The British Association for the Advancement of Science and its American equivalent kept separate sections of zoology and botany until well into the twentieth century (the AAAS had actually divided its original section of biology into zoology and botany in 1893). Julian Huxley's last scientific job, from 1935 to 1942, was that of secretary to the Zoological Society of London, which was still responsible for the London Zoo as well as retaining a significant presence in science. The first International Congress of Zoology was held in Paris in 1889, and the congresses met regularly until 1963. The last meeting, in 1972, was to wind up the affairs handled by previous congresses and transfer authority for the International Code of Zoological Nomenclature to the International Union of Biological Sciences.[53] Taxonomy was still practiced separately for animals and plants, and some of the most active late twentieth-century debates took place at the meetings of the Society for Systematic Zoology, founded in 1947, and in the pages of its journal, *Systematic Zoology*.[54]

[51] See Joseph Caron, "'Biology' and the Life Sciences: A Historiographical Contribution," *History of Science*, 26 (1988), 223–68. On the later developments mentioned in this paragraph, see for instance Jane Maienschein, *Transforming Traditions in American Biology, 1880–1915* (Baltimore: Johns Hopkins University Press, 1991); Ronald Rainger, Keith R. Benson, and Jane Maienschein, eds., *The American Development of Biology* (Philadelphia: University of Pennsylvania Press, 1988).

[52] W. C. Allee, A. E. Emerson, T. Park, O. Park, and K. P. Schmidt, *Principles of Animal Ecology* (Philadelphia: Saunders, 1949).

[53] On the international congresses and zoological nomenclature, see Richard V. Melville, *Towards Stability in the Names of Animals: A History of the International Commission on Zoological Nomenclature, 1895–1995* (London: International Trust for Zoological Nomenclature, 1995).

[54] These are described in David L. Hull, *Science as a Process* (Chicago: University of Chicago Press, 1988).

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Even so, the existence of a unified science of zoology was hard to maintain once the authority of morphology had been lost. E. S. Goodrich's disciple Gavin De Beer (1899–1972) published the textbook *Vertebrate Zoology* (1928), part of a series edited by Julian S. Huxley on "Animal Biology." It still focused on morphology and embryology, with a short section on phylogenetic questions in which De Beer made clear his rejection of recapitulation. But the series itself contained separate volumes on physiology, ecology, and genetics, indicating how the territory of zoology was already being parceled out to distinct specializations.[55] Only in taxonomy did use of the term "zoology" survive in the technical literature, Ernst Mayr publishing *Principles of Systematic Zoology* as late as 1969. Elsewhere, use of the umbrella term "zoology" had gradually diminished, and in the late twentieth century the vast majority of zoology departments vanished in universities, if not in museums. What was left was an ostensibly unified field of biology or life sciences containing a multitude of specializations that were in practice often quite distinct.

[55] G. De Beer, *Vertebrate Zoology* (London: Sidgwick and Jackson, 1928).

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# 13

# BOTANY

*Eugene Cittadino*

Botany has played a key role in the history of the life sciences over the past two centuries. Modern taxonomic concepts and methods had their origins in studies of the plant world. Biogeography similarly began with studies of plant distribution. Darwin's two strongest allies in England and North America, Joseph Dalton Hooker and Asa Gray, respectively, were both plant taxonomists interested in problems of geographical distribution. Darwin's own botanical interests ranged well beyond classification and distribution to include minute studies of the fertilization of flowers and the movements of climbing plants. Meanwhile, a growing laboratory tradition, centered in Germany, made seminal contributions to cell theory, morphology, anatomy, physiology, and plant pathology, many of which aided the development of agricultural science. In the twentieth century, the new science of genetics was based on Gregor Mendel's earlier work on cross-breeding garden plants, rediscovered by turn-of-the-century botanists and then expanded in agricultural experiment stations before becoming established in university research laboratories. Ecological science owes both its conceptual and its institutional foundations to the work of other turn-of-the-century botanists, who combined the earlier plant geography tradition with the new laboratory approach. Later in the twentieth century, cytogenetics became established, first among botanists. Studies of plant viruses and fungal genetics led to major developments in molecular biology, many of the initial applications of biotechnology involved research on plants, and ethnobotany developed into a global enterprise under the dual influences of environmentalism on the one hand and the search for useful, and profitable, pharmaceuticals on the other.

As with most branches of natural history, botany became more professional, more specialized, more laboratory oriented, and less appealing to amateurs over the course of the nineteenth century. This transformation was perhaps more dramatic in botany than in other fields because botany had enjoyed immense popularity among amateur naturalists in the late eighteenth and early nineteenth centuries. Whereas at the beginning of the nineteenth



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century it was a favorite preoccupation of European genteel society, a morally uplifting activity engaged in by women and men, by the end of the century, botany had become the primary occupation of a growing body of middle-class professionals, almost exclusively male, situated in university departments, botanic gardens, and a variety of newer institutions, such as agricultural colleges and research stations. Although the attraction of botany for amateurs did not cease, the interests of amateurs and professionals diverged to such an extent that the two groups had little in common. Similarly, although opportunities for women continued to exist throughout the nineteenth century, more so in botany than in many other sciences, the professionalization of the discipline served to exclude women from positions of responsibility and authority. In the first half of the nineteenth century, the association of plant taxonomy with nature study and with women may have diminished the status of botany in general among male scientists until growing ranks of career-oriented men effectively appropriated all branches of the science for the new professional class. In the twentieth century, career opportunities gradually increased across gender and social class boundaries, particularly in the period since the Second World War.[1]

Botany enjoyed its greatest status as an independent discipline in the last quarter of the nineteenth century, when the success of laboratory-oriented programs in the German universities inspired the expansion of university chairs and departments elsewhere in Europe and in the United States. Although botany certainly has persisted as a discipline, a new trend toward the consolidation of various life sciences specialties under the more comprehensive term "biology" was already in place by the end of the nineteenth century. Conceptually, this trend owed its origins to the growing recognition of the essential unity of all living things, reinforced in the second half of the nineteenth century by evolution theory, along with mounting embryological, physiological, and chemical evidence. Institutionally, its impetus derived almost directly from Thomas Henry Huxley's (1825–1895) course in elementary biology for teachers initiated in 1872 at the Royal School of Mines in London. Huxley's students and assistants promoted the notion of a single unified biological science and, following their mentor, helped to establish laboratory instruction as an integral aspect of biological training.[2] A more recent trend in the reorganization of the life sciences, particularly since World War II, stresses divisions based on the level of organization or methodology,

[1] Anne Shteir, *Cultivating Women, Cultivating Science: Flora's Daughters and Botany in England, 1760–1860* (Baltimore: Johns Hopkins University Press, 1996), especially pp. 165–9; Peter F. Stevens, *The Development of Biological Systematics: Antoine-Laurent de Jussieu, Nature, and the Natural System* (New York: Columbia University Press, 1994), pp. 209–18; David E. Allen, *The Naturalist in Britain: A Social History* (Princeton, N.J.: Princeton University Press, 1994), pp. 158–74.

[2] Wesley C. Williams, "Huxley, Thomas Henry," *Dictionary of Scientific Biography*, VI, 589–97; Gerald L. Geison, *Michael Foster and the Cambridge School of Physiology* (Princeton, N.J.: Princeton University Press, 1978), pp. 116–47; C. P. Swanson, "A History of Biology at the Johns Hopkins University," *Bios*, 22 (1951), 223–62.

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so that a specialist in plant science, depending on the specialty, might be located at one institution in a department of evolution, systematics, and ecology, at another in a department of genetics and cell biology, or at still another in a department of molecular biology, with none of the institutions having an independent program in botany as such.[3]

## BEYOND LINNAEUS: SYSTEMATICS AND PLANT GEOGRAPHY

The system of plant classification devised by Carl Linnaeus (1707–1778) in the mid-eighteenth century continued to dominate the world of amateur botanists and collectors well into the nineteenth century, even as a growing body of professionals worked at developing more sophisticated systems based on "natural" relationships among plant taxa. Few systematists found fault with Linnaeus's binomial method of classification, which established the practice of assigning to each species a genus name followed by a trivial, but unique, species name. However, Linnaeus's so-called sexual system, based, in essence, on the number and arrangement of reproductive structures in the flower, left much to be desired. Linnaeus had been well aware of its limitations and its artificial nature, but he acknowledged the difficulty of devising an entirely natural system, especially because knowledge of the world's flora was woefully incomplete. Nevertheless, many of the Linnaean families (he referred to them as orders) were recognized by later botanists as representing natural groups, and, more importantly, the system proved to be immensely practical for the naturalist in the field. Countless field botanists, from amateurs to serious collector/explorers, utilized Linnaeus's artificial system as a quick and efficient method for grouping new specimens. British botanist Robert Brown (1773–1858), for example, made use of the Linnaean system during the years he spent collecting in Australia, Tasmania, and New Zealand at the turn of the nineteenth century, where he discovered hundreds of species new to Europeans. After his return, however, Brown wrote up his monographs using a modified version of Antoine-Laurent de Jussieu's natural system.[4]

As Brown's itinerary suggests, the collection and classification of plants was tied closely to European exploration and colonization. Not surprisingly, the largest imperial centers – Paris, London, and later Berlin and New York – became centers of plant systematics. Brown was an important agent of change.

[3] Based on personal examination of recent university catalogs.

[4] Gunnar Eriksson, "Linnaeus the Botanist," in *Linnaeus: The Man and His Work*, ed. Tore Frängsmyr (Berkeley: University of California Press, 1983), pp. 63–109; John Reynolds Green, *A History of Botany in the United Kingdom from the Earliest Times to the End of the 19th Century* (London: J. M. Dent, 1914), pp. 253–353; D. J. Mabberly, *Jupiter Botanicus: Robert Brown of the British Museum* (Braunschweig: J. Cramer, 1985), pp. 141–76; William T. Stearn, "Brown, Robert," *Dictionary of Scientific Biography*, II, 516–22.

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His major work on the southern flora, *Prodromus florae Novae Hollandiae* (Preliminary Study of the Flora of New Holland, 1810), effectively introduced de Jussieu's natural system to a generation of British botanists. In 1859, J. D. Hooker, director of the Royal Botanic Gardens at Kew and himself an eminent botanist-explorer, characterized it as "the greatest botanical work that has ever appeared."[5] In France, Brown's contemporary and close acquaintance Swiss botanist A. P. de Candolle (1778–1841) served a similar role in extending and interpreting the natural system of Antoine-Laurent de Jussieu (1748–1836), who had been one of his mentors in Paris at the Jardin des Plantes. The central idea behind de Jussieu's work, first published in the late eighteenth century, was to ground a classification system on natural affinities determined from a wide spectrum of structures, not just floral parts. The intent, in principle, was to include all structures, including the microscopic, but natural classification systems did not probe beneath the surface of the plant. If plant taxonomy until quite recently has relied primarily on external features, it has also relied heavily on the taxonomic categories set down by de Jussieu and modified only slightly by de Candolle. The last attempt at a comprehensive natural classification, that of George Bentham and Hooker, begun in the 1860s, adopted most of de Candolle's families and genera, and these categories have remained, with relatively little modification, to the present day. Botanist and historian of plant systematics Peter Stevens argues, in fact, that botanical systematics after de Jussieu remained relatively stable through the nineteenth century and well into the twentieth. Stevens cites a number of factors, including the training and antitheoretical bias of systematists, the elusive nature of the botanical categories (genera and families) themselves, and the continual pressures for constancy from the large field of gardeners and amateurs.[6]

The Bentham and Hooker scheme made no attempt to reconstruct phylogenetic relationships, despite the general establishment of evolution theory by the 1860s and despite Hooker's close association with Darwin. Although an evolutionary perspective assumes common ancestry as the basis for affinities between organisms, in practice it is very difficult, and often unreliable, to use inferred phylogenetic relationships as the basis for a classification. Most systematists have preferred to construct a phylogenetic scheme from independently recognized taxonomic categories rather than use phylogeny to construct the categories. Almost all of the phylogenetic schemes proposed since the late nineteenth century are modifications of either the scheme

[5] Quoted in Mabberly, *Jupiter Botanicus*, p. 166.

[6] A. G. Morton, *History of Botanical Science* (London: Academic Press, 1981), pp. 294–313, 371–4; J. Reynolds Green, *A History of Botany, 1860–1900, Being a Continuation of Sachs' "History of Botany, 1530–1860"* (New York: Russell and Russell, 1967), pp. 110–53; George Bentham and Joseph Dalton Hooker, *Genera Plantarum*, 3 vols. (London: Williams and Norgate, 1862–83); Clive Stace, *Plant Taxonomy and Biosystematics*, 2nd ed. (London: Edward Arnold, 1989), pp. 25–9; Stevens, *Development of Biological Systematics*, pp. 111–18, 251–61.

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developed by August Eichler (1839–1887) and Adolf Engler (1844–1930), successive directors of the Berlin Botanical Garden from 1878 through 1914, or that developed independently by Charles E. Bessey (1845–1915) in the United States and Hans Hallier (1831–1904) in Germany around the turn of the twentieth century. Since that time, the major change in plant systematics has been the increasing use of quantitative methods, particularly, but not exclusively, those that rely on evidence from cytogenetics and molecular biology. Such methods have been utilized to determine taxonomic affinities from a neutral perspective, as in numerical taxonomy, and to reconstruct specific phylogenetic relationships, as in cladistics.[7]

Because the practice of botanical systematics was tied closely to global exploration, studies of the spatial, as well as temporal, distribution of plants developed alongside taxonomy almost from the beginning. In the nineteenth century, both paleobotany and botanical geography came into their own, with the latter commanding most of the attention. Beginning in the first decade of the century with Alexandre Brongniart's (1770–1847) impressive tabulation of the fossil plants in the vicinity of Paris, paleobotany quietly established a place for itself, as the description, identification, and cataloging of fossilized plants became an indispensable tool of stratigraphy. The general acceptance of evolution theory conferred even greater significance on paleontological studies, and the latter half of the century saw a gradual increase in both the compilation of fossil plant evidence and its application to questions regarding the past distribution of plant life. By the end of the century, systematists such as Adolf Engler in Berlin were applying paleontological evidence to the solution of phylogenetic problems, and in the twentieth century paleobotany found significant applications in ecology, anthropology, and even agricultural science.[8]

Meanwhile, botanical geography, or phytogeography, developed in two distinct, but not entirely separate, directions in the nineteenth century. On the one hand, floristic studies emphasized regional and worldwide distribution patterns of particular taxa, mainly flowering plant families and genera, with the resulting division of the globe into specific floristic provinces. Much of the work of Joseph Dalton Hooker (1817–1911) and Asa Gray (1810–1888), Darwin's most valued botanical allies, focused on problems of plant distribution. Hooker's work, as the result of his extensive travels, concentrated on the southern flora, especially Tasmania and New Zealand, and on the flora of India and Tibet. Gray, whose travels were limited, nevertheless made use

[7] Stace, *Plant Taxonomy and Biosystematics*, pp. 29–63; Stevens, *Development of Biological Systematics*, chaps. 10, 11, and Epilogue; Richard A. Overfield, *Science with Practice: Charles E. Bessey and the Maturing of American Botany* (Ames: Iowa State University Press, 1993), pp. 178–99.

[8] Martin Rudwick, *The Meaning of Fossils: Episodes in the History of Paleontology*, 2nd ed. (New York: Neale Watson, 1976), pp. 127–49; Karl Mägdefrau, *Geschichte der Botanik* (Stuttgart: Gustav Fischer, 1973), pp. 231–51; Stanley A. Cain, *Foundations of Plant Geography* (New York: Harper and Row, 1944), p. ii.

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of the extensive collections of numerous botanical colleagues and students who ventured far into the interior of North America as European settlement spread westward during the century. Both Hooker and Gray identified floral provinces and made comparative studies involving global north–south and east–west patterns of distribution, and Darwin incorporated their work into the chapters on geographical distribution in *On the Origin of Species*. Much of the work in floristic plant geography in the first half of the nineteenth century, including statistical studies that explored naturally recurring patterns in ratios of genera and species, was summarized in Alphonse de Candolle's (1806–1893) major treatise, *Géographie botanique raisonée* (A Rational Geographical Botany), published in 1855.[9]

De Candolle's treatise also reflected the second direction in botanical geography – that of linking particular forms of plants, and plant groups, with particular physical conditions, mainly climate and soil, a tradition already begun in the late eighteenth century and given a strong impetus by the work of turn-of-the-century naturalist-explorer Alexander von Humboldt (1769–1859). In addition to bringing back to Europe hundreds of as yet unnamed plant specimens, mainly from South America, Humboldt extended the study of whole assemblages of plants, a German tradition in which he had been schooled, to include the identification of plant physiognomy with climate. Most notable was his treatment, inspired by explorations in the Andes, of the parallels between the vertical pattern in vegetation from the base to the summit of a mountain and horizontal patterns from the equator to the poles.[10] This discussion of zonation, along with Humboldt's grouping of plants by physiognomic type, began a tradition that has persisted through the twentieth century. Humboldt's original sixteen physiognomic types, or life forms, included such broad categories as grasses, succulents, palms, and deciduous trees. During the nineteenth century, a number of European phytogeographers expanded these categories and elaborated various systems by which to identify and classify whole environmental groups, first dubbed "formations" in 1838 by Humboldt's follower August Grisebach. Through the work of Grisebach (1814–1879), Anton Kerner von Marilaun (1831–1898), Eugenius Warming (1841–1924), and A. F. W. Schimper (1856–1901), among others, this school of vegetational studies became linked with work in plant physiology, physical geography, soil science, and other fields to emerge at the end of the nineteenth century as one of the central features of the new science of plant ecology (see Acot, Chapter 24, this volume). In the twentieth century, the floristic and vegetational sides have persisted as separate branches of phytogeography, with the floristic linked more closely with plant systematics

[9] Janet Browne, *The Secular Ark: Studies in the History of Biogeography* (New Haven, Conn.: Yale University Press, 1983), pp. 32–85; Andrew Denny Rodgers III, *American Botany, 1873–1892: Decades of Transition* (New York: Hafner, 1968), chaps. 2–6; A. Hunter Dupree, *Asa Gray, 1810–1888* (New York: Atheneum, 1968), pp. 185–96, 233–63; Ray Desmond, *Sir Joseph Dalton Hooker: Traveller and Plant Collector* (London: Royal Botanic Gardens, Kew, 1999), pp. 253–60.

[10] Browne, *Secular Ark*, pp. 42–52.

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and phylogenetics and the vegetational linked more closely with ecology, particularly community ecology. Sometimes these two sides are characterized as historical and ecological phytogeography, respectively.[11]

## BOTANICAL GARDENS

For much of the nineteenth century, the central botanical research institution was the formal botanical garden or, to be more exact, the botanical garden and museum, including as one of its essential features an extensive herbarium with cabinets and drawers well stocked with dried, mounted specimens. The modern botanical garden got its start in the sixteenth and seventeenth centuries as a site for the display of plant life from all sectors of the globe, with the dual rationale of providing, on the one hand, a very tangible symbol of Christian European imperialism and, on the other, a diversity of herbs potentially capable of curing any known disease. Begun as university gardens associated with the medical faculties at Padua and Leiden, these facilities quickly caught the attention of wealthy and powerful patrons throughout Europe. By the eighteenth century, the impressive university gardens, such as those at Cambridge and Uppsala, were eclipsed by extensive urban gardens established in the large imperial centers of Europe – Paris, London, Berlin, and Vienna. From the beginning, these gardens served multiple purposes – aesthetics, education, research, breeding and acclimatization, and, of course, display of the spoils of global exploration and conquest.[12]

Before the nineteenth century, most botanical expeditions outside Europe were French sponsored, and the Jardin des Plantes in Paris reaped the benefits of such dominance with superb collections that served several generations of plant systematists, including Jean-Baptiste Lamarck, Bernard and Antoine-Laurent de Jussieu, and A. P. de Candolle. The Jardin des Plantes remained the premiere European garden well into the nineteenth century, although the English model of more natural plantings on extensive grounds had already begun to replace the older formal design on which the Paris garden was based. For reasons other than outward design, the balance began to shift to England in the late eighteenth century, when Joseph Banks (1743–1820) brought back the first botanical collections from James Cook's voyages and began serving

[11] Malcolm Nicolson, "Humboldtian Plant Geography after Humboldt: The Link to Ecology," *British Journal for the History of Science*, 29 (1996), 289–310; Eugene Cittadino, *Nature as the Laboratory: Darwinian Plant Ecology in the German Empire, 1890–1900* (Cambridge: Cambridge University Press, 1990), pp. 118–20, 146–57; Robert P. McIntosh, *The Background of Ecology: Concept and Theory* (Cambridge: Cambridge University Press, 1985), pp. 127–45; Heinrich Walter, *Vegetation of the Earth: In Relation to Climate and the Eco-physiological Conditions*, trans. Joy Wieser (London: The English Universities Press, 1973), pp. 1–27.

[12] John Prest, *The Garden of Eden: The Botanic Garden and the Re-creation of Paradise* (New Haven, Conn.: Yale University Press, 1981), pp. 38–65; Richard Drayton, *Nature's Government: Science, Imperial Britain and the 'Improvement' of the World* (New Haven, Conn.: Yale University Press, 2000), pp. 137–8.

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as director of the Royal Botanic Gardens at Kew, outside London. Both the collections of preserved specimens and the live plantings at Kew expanded considerably during Banks's tenure. By the second decade of the nineteenth century, Kew had become the center of a worldwide network of colonial gardens that served as sites for further exploration as well as horticultural experiment stations and acclimatization centers for exotic plants disseminated throughout this network. Nevertheless, the French example of generous state patronage still served as inspiration for the reorganization of Kew in the 1840s under William Jackson Hooker (1785–1865), as it had for the reorganization of the Berlin Botanical Garden under Karl Willdenow (1765–1812) in the first decade of the century.[13]

Although the gardens clearly served the interests of botanical science, their directors and supporters seldom promoted them as sites for the pursuit of pure science. Neither aesthetic nor scientific goals served as well as economic ones in garnering public support and encouraging state funding. The case of Kew is again instructive. Historian Richard Drayton argues that securing stable state funding for Kew required not only the promise of economic reward but economic reward tied closely to the idea of empire. Once botany at Kew was perceived as serving the expansion of empire, then Kew's directors, particularly Hooker's son Joseph and Joseph's son-in-law William Thiselton-Dyer, were able to use the garden's service to empire as a rationale for public support of an expanding domain of professional botany. As Drayton states it, "Imperial science would produce a scientific empire."[14] In the 1870s, Joseph Hooker had made use of the expanding network of colonial gardens, including Ceylon (Sri Lanka), Calcutta, Singapore, Burma, and Borneo, to experiment with the best methods of rubber tree cultivation. His successor, William Thiselton-Dyer (1843–1928), who served as director from 1885 to 1905, managed to forge even stronger links to economic botany, especially colonial agriculture, in a myriad of separate enterprises. Nevertheless, he was also instrumental in securing a place in Britain for the new ideas in botanical research and teaching that had emerged in Germany during the middle third of the century. He supervised the translation of Julius Sachs's influential textbook on botany, he established and directed the first botanical research laboratory in Britain, the Jodrell Laboratory, at Kew in 1875, and, through the example of Jodrell, he was instrumental in encouraging the establishment of botanical research laboratories at Oxford and Cambridge as well as the newer universities.[15]

[13] Henry Savage, Jr., "Introduction," in Marguerite Duval, *The King's Garden*, trans. Annette Tomarken and Claudine Cowen (Charlottesville: University Press of Virginia, 1982), p. ix; Henry Potonié, "Der königliche botanische Garten zu Berlin," *Naturwissenschaftliche Wochenschrift*, 5 (1890), 212–13; Drayton, *Nature's Government*, pp. 229–30.

[14] Drayton, *Nature's Government*, p. 168.

[15] Lucille Brockway, *Science and Colonial Expansion: The Role of the British Botanic Gardens* (New York: Academic Press, 1979), pp. 156–60; Ray Desmond, *Kew: The History of the Royal Botanic Gardens* (London: Harvill Press, 1995), pp. 290–301; Green, *History of Botany in the United Kingdom from the Earliest Times to the End of the 19th Century*, pp. 525–39.

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The Berlin Botanical Garden served a similar dual function as scientific research center and coordinator of colonial botany once the united German state entered the colonial arena with acquisitions in Africa and the Pacific. By the end of the century Germany had established colonial gardens and experiment stations in East Africa, South-West Africa (present-day Namibia), and Cameroon. Adolf Engler, the director of the Berlin facility during most of the colonial period, supervised the transfer of the garden to its new site at Dahlem, where he proceeded to arrange plants in natural groups corresponding to Grisebach's formations and used his advantageous position to extend his research in taxonomy and plant geography, adding several volumes on the flora of Africa to his already impressive list of publications. He also established facilities for horticultural experimentation both in Berlin and at the colonial gardens and set up an office for disseminating information, as well as seeds and live plants, to planters in the colonial regions. The Jardin des Plantes likewise continued to serve as a center for horticultural experimentation and acclimatization as well as pure research, although its efforts in all these areas were overshadowed by those of Kew and Berlin by the end of the century. One notable colonial facility, the Botanic Garden at Buitenzorg (now Bogor) on the island of Java, perhaps the largest botanical garden in existence, became an important center for pure research into tropical botany in the 1880s, when its new director, Melchior Treub (1851–1910), established both a modern botanical laboratory and a montane research garden at the site. Although Treub maintained the garden's primary role of service to the Dutch colonial agricultural interests, he managed to attract a steady stream of academic botanists to the site and provided a journal for publication of their results. The New York Botanical Garden came into existence at the turn of the twentieth century, when the United States began to acquire overseas territories. Its founder, Nathaniel Lord Britton (1859–1939), like his counterparts at the Berlin Botanical Garden, had been inspired by the example of Kew. Rather than promote economic botany, however, he chose to emphasize pure taxonomic research. Access to the Caribbean opened up following the war with Spain, and Britton managed to organize over seventy separate collecting expeditions between 1898 and 1916. By working out a joint venture with Harvard University and the National Herbarium in Washington, he later expanded the sphere of the garden to include parts of South America.[16]

## THE "NEW BOTANY"

Even as large urban botanical gardens became research centers for plant systematics, biogeography, and the acclimatization of exotic plants, a new

[16] Bernhard Zepernick and Else-Marie Karlsson, *Berlins Botanischer Garten* (Berlin: Haude und Spener, 1979), pp. 90–103; Cittadino, *Nature as the Laboratory*, pp. 76–9, 135–9; Henry A. Gleason, "The Scientific Work of Nathaniel Lord Britton," *Proceedings of the American Philosophical Society*, 104 (1960), 218–24.

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kind of research center, the botanical laboratory, began to take shape. The study of plant form, structure, and function, including the algae, fungi, lichens, mosses, and liverworts, as well as all vascular plants, became a central preoccupation of these new botanical laboratories or institutes, especially those situated within the expanding German university system, and in the German-speaking universities of Austria and Switzerland. By the second half of the century, these new research institutes came to dominate the science of botany and attract the attention of a growing number of newcomers to the discipline. Perhaps the best examples were the botanical institutes associated with Julius Sachs (1832–1897) at the University of Würzburg from the 1860s to the 1890s and Anton de Bary (1831–1890) at the restructured German university at Strasbourg from the end of the Franco-Prussian War to the late 1880s. There doctoral candidates, assistants, privatdozents, and occasional visitors worked at their assigned spaces, usually on projects selected by the professor in charge. De Bary's institute offered specialized work in mycology (the study of fungi, including fungal diseases of crop plants) and anatomy. Sachs's institute focused on plant physiology, a field that he probably did more than any other individual to help create. Both institutes were frequented by foreign botanists, who used their experiences in Germany to encourage the development of laboratory botany in their respective countries.[17]

That the laboratory enterprise should find its home first in Germany had to do with several factors. The proliferation of universities within the politically fragmented but economically advancing German-speaking states during the first half of the nineteenth century led to competition to match facilities and attract the best professors. At the same time, a new model for the university as both a teaching and research institution was inaugurated by the University of Berlin, founded during, and influenced by, the French occupation of Prussia just after the turn of the century. In addition, the physical design and hierarchical structure of the German research facilities encouraged minute investigations carried on at one's assigned station in the laboratory, an arrangement that lent itself particularly well to microscopical work. Because so much of the new direction in botanical research involved microscopical studies, one might be tempted to attribute these developments to technical advances in microscopy and to the general availability of quality instruments. However, some of the most significant early work, such as Robert Brown's studies of the nucleus, pollen tube generation, and fertilization in flowers and Hugo von Mohl's (1805–1872) prolific studies of cell formation, were carried out with simple single-lens instruments. One might well make a case, as do both Julius Sachs and Brown's biographer D. J. Mabberly, that these early successes with simple instruments served to draw more researchers into the

[17] S. H. Vines, "Reminiscences of German Botanical Laboratories in the 'Seventies and 'Eighties of the Last Century" and D. H. Scott, "German Reminiscences of the Early 'Eighties," *The New Phytologist*, 24 (1925), 1–8, 9–16.

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field and create a need for better and cheaper microscopes. In any event, the steady improvement during the 1830s and 1840s of quality compound instruments that eliminated much spherical and chromatic aberration certainly aided the new botanical investigations.[18]

One of the immediate applications of microscopical inquiry was in working out the life cycles of the so-called cryptogams – plants, such as fungi, algae, mosses, and ferns, that produce neither flowers nor seeds and whose means of reproduction were poorly understood or unknown at that time. During the period from 1830 to 1850, the cryptogams became much less cryptic, as researchers described the details of gamete formation and exchange in one organism after another. The culmination of this work was the publication in 1851 of Wilhelm Hofmeister's (1824–1877) modest but seminal treatise describing a universal alternation of generations throughout the plant kingdom. Hofmeister, a music publisher and self-taught botanist, demonstrated convincingly that all multicellular green plants, from the bryophytes (mosses and liverworts) to the angiosperms (flowering plants), have life cycles involving the alternation of a gamete-producing haploid generation with a spore-producing diploid generation remarkably similar in their structural details. Hofmeister's discovery provided a powerful unifying theme for the plant sciences at mid-century and served as a powerful stimulus to further research.[19]

Hofmeister had been inspired by the microscopical studies of Robert Brown and Hugo von Mohl and by the writings of Matthias Schleiden (1804–1881), one of the architects of the cell theory and author of a groundbreaking botanical textbook that encouraged empirical studies in anatomy and morphology and offered guidelines for the use of the microscope. Schleiden's "scientific botany" became the programmatic model for a new generation of professionals finding employment within the expanding German university system. Armed with cell theory, Hofmeister's alternation of generations, increasing knowledge of the chemical composition of plant life, and, after 1860, evolution theory, botanists at the new laboratories worked out details of the life cycles, developmental processes, and anatomical structures of all types of plants. Anatomy and morphology dominated this early phase in laboratory botany, but by the 1860s, plant physiology also emerged as a specialty, largely due to the efforts of Julius Sachs, who applied his background in both medical physiology and agricultural science to create a highly influential teaching and research program in plant physiology at the University of Würzburg. Much of Sachs's research concerned the study of tropisms,

[18] Morton, *History of Botanical Science*, pp. 362–4, 387–97; Brian Ford, *Single Lens: The Story of the Simple Microscope* (New York: Harper and Row, 1985), pp. 143–64; Julius von Sachs, *A History of Botany*, trans. H. E. F. Garnsey and I. B. Balfour (Oxford: Clarendon Press, 1890), pp. 220–6; Mabberly, *Jupiter Botanicus*, pp. 113–14.

[19] Johannes Proskau, "Hofmeister, Wilhelm Friedrich Benedikt," *Dictionary of Scientific Biography*, VII, 464–8; Morton, *History of Botanical Science*, pp. 398–404.

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responses to stimuli such as light, gravity, and touch, for which he invented an impressive array of ingenious mechanical devices. His botanical institute became the training ground for a generation of botanists, including, among many others, Wilhelm Pfeffer (1845–1920), his eventual successor as Germany's premier plant physiologist, Hugo de Vries (1848–1935), one of the rediscoverers of Mendel's work, and Francis Darwin (1848–1925), who studied under both Sachs and de Bary while assisting his father, Charles, with his investigations into the movements of plants. In addition to his institute, Sachs published a highly influential botanical textbook that was translated widely and became the model for the transference of the German botanical program elsewhere.[20]

By the last two decades of the nineteenth century, the transformation in botany that was centered in Germany came to be called the "new botany" in the United States and England. Young botanists from all over the world traveled to Germany to receive the kind of training that was available nowhere else, most often working with Sachs and de Bary before the 1880s but thereafter visiting the botanical institutes at Bonn under Eduard Strasburger, Leipzig under Wilhelm Pfeffer, or Munich under Karl Goebel (1855–1932), each of whom had been trained at one time or another by either Sachs or de Bary. Inspired by the German model, laboratory training became an essential feature of botanical programs in British and American universities. By the end of the century, the traditional emphasis on taxonomy gave way to morphology, anatomy, and physiology, including applications of these specialties in agricultural science.[21]

Typical of the "new botanists" was Marshall Ward (1854–1906), who held the chair in botany at Cambridge University from 1895 until his death in 1906. One could identify many similar career trajectories among botanists in Europe and the United States, but a brief look at Ward's career should suffice to illustrate the major features of this trend. Born into a family of modest means, Ward obtained his initial education in the sciences in T. H. Huxley's teacher training course at the Royal School of Mines in London. There his instructors in botany were William Thiselton-Dyer and Sidney Vines (1849–1934), both of whom had worked in German botanical laboratories. Thiselton-Dyer went on to set up the Jodrell Laboratory and direct the Kew Gardens. Vines became the principal agent in establishing the new botany first at Cambridge and then at Oxford. Ward's exceptional work in botany at the School of Mines helped him obtain a scholarship to attend Cambridge. After graduating, he traveled to Germany for advanced work in Sachs's institute

[20] Karl Goebel, "Julius Sachs," *Science Progress*, 7 (1898), 150–73; E. G. Pringsheim, *Julius Sachs: Der Begründer der neueren Pflanzenphysiologie, 1832–1897* (Jena: Gustav Fischer, 1932), pp. 218–30; Cittadino, *Nature as the Laboratory*, pp. 17–25; Julius Sachs, *Text-book of Botany, Morphological and Physiological*, trans. A. W. Bennett and W. T. Thiselton-Dyer (Oxford: Clarendon Press, 1875).

[21] Rodgers, *American Botany*, pp. 198–225; F. O. Bower, "English and German Botany in the Middle and Towards the End of Last Century," *The New Phytologist*, 24 (1925), 129–37; Overfield, *Science with Practice*, pp. 72–99.

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before accepting a colonial post as "Government Cryptogamist" to study coffee diseases at a plantation in Ceylon. On his return to England, he was appointed professor of botany at the Forestry Institute of the Royal Indian Engineering College in London. Among other projects, he undertook the English translation of Sachs's book of lectures on plant physiology. In 1895, he accepted the chair at Cambridge, where, as a result of his extensive practical experience with plant diseases, he promoted the study of plant pathology.[22]

## LINKING FIELD AND LABORATORY, THEORY AND PRACTICE

Ward's career reflects the merging of fieldwork with laboratory research, of practical applications with pure science, an interaction of methodologies and agendas that more realistically captures the character of many late nineteenth- and twentieth-century developments in botany than the use of such dichotomies as "pure" and "applied" science or "naturalists" versus "experimentalists." The connections between botany and agricultural science extend back to the mid-nineteenth century, when university-trained botanists were finding positions in new agricultural colleges and experiment stations. Agricultural research, in turn, stimulated changes in academic botany. The considerable attention given at the agricultural stations to the nutritional requirements of crop plants provided a strong impetus to the development of plant physiology. Julius Sachs began teaching the subject in one of Germany's new agricultural colleges in the early 1860s before setting up his laboratory at the University of Würzburg. In the 1880s, the Agricultural College of Berlin became a major center for training in plant physiology as well as plant pathology, a science whose modern origins can be traced to studies on the fungal diseases of plants initiated by Anton de Bary in the 1850s. When opportunities for botanists opened up in the many agricultural colleges established in the United States in the second half of the nineteenth century, as well as in the U.S. Department of Agriculture and the nationwide network of agricultural experiment stations, the study of various rusts, smuts, and mildews affecting crop plants became a major preoccupation in these institutions. Meanwhile, the study of diseases of economically valuable plants, such as coffee and sugar, became one of the central tasks of European, and later American, botanists dispatched to colonial regions in the tropics.[23]

[22] S. M. Walters, *The Shaping of Cambridge Botany* (Cambridge: Cambridge University Press, 1981), pp. 83–5; Green, *History of Botany in the United Kingdom from the Earliest Times to the End of the 19th Century*, pp. 543–69; W. T. Thiselton-Dyer, "Plant Biology in the 'Seventies," *Nature*, 115 (1925), 709–12.

[23] Charles E. Rosenberg, *No Other Gods: On Science and American Social Thought* (Baltimore: Johns Hopkins University Press, 1976), pp. 153–84; Arthur Kelman, "Contributions of Plant Pathology to the Biological Sciences," in *Historical Perspectives in Plant Science*, ed. Kenneth J. Frey (Ames: Iowa State University Press, 1994), pp. 89–107.

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Plant ecology emerged as a specialty around the turn of the twentieth century, when field researchers in Europe and the United States applied some of the techniques, and especially the viewpoints, of the newer laboratory and experimental programs to studies involving plant adaptation and the distribution and dynamics of whole plant communities. In the United States, where the new science developed its strongest institutional affiliations, many of those who entered the field received their initial botanical training at the newer state universities and agricultural colleges established in the Midwest and took part in vegetational surveys of plains, forests, and range land at the then western borders of cultivation. The field developed in several directions in the twentieth century, often with distinct national and regional styles, taking the form of phytosociology in Scandinavia and parts of continental Europe, where the principal concern was the careful delineation of specific plant groups, or community ecology, especially in the United States, where the main emphasis was on the dynamics of vegetational change over time, or geobotany as it came to be called in Russia, where plant communities were viewed as integral parts of entire biogeophysical complexes.[24]

The laboratory tradition nevertheless continued as a dominant trend through the twentieth century, infused with a variety of new experimental techniques, such as chromatography, use of the ultracentrifuge, and labeled isotopes. Physiologists in the first half of the century succeeded in working out the details of photosynthesis and explaining the important role played by plant hormones in various growth and developmental processes, a line of inquiry actually initiated by Charles and Francis Darwin in the 1870s. Similarly, plant anatomy at first benefited from late nineteenth-century improvements in conventional light microscopy and then received a new life with the advent of electron microscopy after 1950. Yet both physiological and anatomical research were often conducted with practical applications in mind or in applied settings. Katherine Esau (1898–1987), one of the premier plant anatomists of the twentieth century and a pioneer in the use of the electron microscope, received much of the inspiration for her work from her interest in viral diseases of crop plants acquired from her training at the Agricultural College of Berlin and employment at a sugar company on first emigrating to the United States. Similarly, university-trained plant physiologists working for the Bureau of Plant Industry of the U.S. Department of Agriculture during the first quarter of the twentieth century were largely responsible for applying the Mendelian hereditary theory to the development of hybrid corn, a project whose completion involved direct cooperation between USDA botanists and a private seed company in Illinois. Somewhat later in the century, Barbara McClintock (1902–1992) and George W. Beadle (1903–1989), both of whom

[24] Cittadino, *Nature as the Laboratory*, pp. 146–57; Cittadino, "Ecology and Professionalization of Botany in the United States, 1890–1905," *Studies in the History of Biology*, 4 (1980), 171–98; Malcolm Nicolson, "National Styles, Divergent Classifications: A Comparative Case Study from the History of French and American Plant Ecology," *Knowledge and Society*, 8 (1989), 139–86.

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had trained in the same graduate program in agricultural genetics at Cornell University in the 1930s, made major contributions to the understanding of the structure and behavior of DNA – McClintock with her work on maize genetics and Beadle with his studies of the bread mold *Neurospora*.[25]

The three European botanists who rediscovered Gregor Mendel's work in 1900 – Carl Correns (1864–1933), Hugo de Vries, and Erich von Tschermak (1871–1962) – had all been conducting studies in variation inspired by Darwin's work on the fertilization of flowers, but it was plant breeders in the United States who most readily embraced the Mendelian theory and attempted to apply it. Genetics research soon found a home in the universities, but many of the American university botanists who helped establish Mendelian genetics came from agricultural backgrounds or had worked directly in plant breeding. In Germany, by contrast, with the exception of a program at the Agricultural College of Berlin, genetics research remained part of academic biology and did not establish strong links with agricultural breeders. For this reason, as well as differences in university structure between the United States and Germany, German genetics emphasized cytoplasmic, in addition to nuclear, inheritance and focused less on practical applications. In Britain, the value of Mendelian genetics to plant breeding was a matter of debate in the early years of the century, with seed companies somewhat reluctant to throw in their lot with Mendelians at first, as they had done in the United States. The result was the establishment of several independent plant-breeding centers, all of which eventually came under state control within the purview of the Agricultural Research Council, which maintained close ties with university genetics programs. By mid-century, these centers had developed new varieties of wheat, barley, oats, and potatoes that outcompeted those produced by domestic private seed companies. Plant breeders at the French National Institute for Research in Agronomy achieved similar success in the 1950s, when they were able to develop varieties of corn that could thrive in the relatively cool climate of Europe north of the Alps. The result was the exportation of French-produced hybrids to other European countries beginning in the 1960s and the gradual northern extension of the limits of cultivated corn.[26]

[25] Morton, *History of Botanical Science*, pp. 448–53; P. R. Bell, "The Movement of Plants in Response to Light," in *Darwin's Biological Work, Some Aspects Reconsidered*, ed. P. R. Bell (Cambridge: Cambridge University Press, 1959), pp. 1–49; Lee McDavid, "Katherine Esau," in *Notable Women in the Sciences: A Biographical Dictionary*, ed. Benjamin F. Shearer and Barbara S. Shearer (Westport, Conn.: Greenwood Press, 1996), pp. 113–17; Deborah Fitzgerald, *The Business of Breeding: Hybrid Corn in Illinois, 1890–1940* (Ithaca, N.Y.: Cornell University Press, 1990), pp. 30–74, 150–69; Barbara A. Kimmelman, "Organisms and Interests in Scientific Research: R. A. Emerson's Claims for the Unique Contributions of Agricultural Genetics," in *The Right Tools for the Job: At Work in Twentieth-Century Life Sciences*, ed. Adele E. Clarke and Joan H. Fujimura (Princeton, N.J.: Princeton University Press, 1992), pp. 198–232.

[26] Jonathan Harwood, *Styles of Scientific Thought: The German Genetics Community, 1900–1933* (Chicago: University of Chicago Press, 1993), pp. 138–80; Paolo Palladino, "Between Craft and Science: Plant Breeding, Mendelian Genetics, and British Universities, 1900–1920," *Technology and*

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After World War II, the United States began actively exporting the products and techniques of its plant-breeding programs to developing nations. In the 1940s, Norman Borlaug (b. 1914), a plant pathologist by training, was sent to Mexico in a joint venture involving the U.S. government, the Rockefeller Foundation, and the Mexican Ministry of Agriculture. He shifted his focus from pathology to breeding experiments and soon produced a variety of wheat that greatly increased Mexican yields. By the 1960s, the so-called Green Revolution had spread to India, Pakistan, Turkey, and other nations and expanded to include rice and other crops besides wheat. Borlaug was awarded the Nobel Peace Prize for this work in 1970, although the program came under considerable criticism from environmentalists for its heavy dependence on fossil fuels and chemical fertilizers and its effects in reducing natural genetic diversity. During the last two decades of the century, a new kind of joint venture, involving agricultural researchers, university botanists, and private capital, led to some of the first successful, and equally controversial, applications of recombinant DNA technology to the production of disease-resistant crop plants. Some fifty such transgenic plants, produced by transferring a gene from a pathogenic virus to the host plant, were approved for field testing in the United States between 1987 and 1995. Other projects have met with less success, such as the use of a bioengineered frost-inhibiting bacterium on crop plants and attempts to employ bioengineering techniques to transfer nitrogen-fixing bacteria to nonleguminous plants, neither of which proved to be commercially viable.[27]

The promise of practical applications often led to fundamental insights regarding the nature of inheritance and the process of evolution. Early in the century, fieldwork and laboratory research combining ecological and paleontological knowledge with cytogenetics transformed plant systematics by offering new insights into the process of speciation. In the Soviet Union, geneticist N. I. Vavilov (1891–1951) applied such a perspective in his seminal studies concerning the origins of crop plants conducted in the 1920s and 1930s, before his research program was cut short by the anti-Mendelian policies of agronomist and Soviet ideologue T. D. Lysenko (1898–1976). Motivated by his theory that plants exhibit the greatest genetic diversity nearest their centers of origin, Vavilov coordinated extensive worldwide collecting expeditions and followed these with comparative cytogenetic studies. In the United States, the new Carnegie Institution, with long-term practical applications in mind, established a Desert Botanical Laboratory at Tucson,

*Culture*, 34 (1993), 300–23; Paolo Palladino, "Science, Technology, and the Economy: Plant Breeding in Great Britain, 1920–1970," *Economic History Review*, 49 (1996), 116–36; Neil McMullen, *Seeds and World Agricultural Progress* (Washington, D.C.: National Planning Association, 1987), pp. 147–63.

27 John H. Perkins, *Geopolitics and the Green Revolution: Wheat, Genes, and the Cold War* (New York: Oxford University Press, 1997), pp. 223–46; Charles S. Levings III, Kenneth L. Korth, and Gerty Cori Ward, "Current Perspectives: The Impact of Biotechnology on Plant Improvement," in Frey, *Historical Perspectives in Plant Science*, pp. 133–60; Sheldon Krimsky and Roger P. Wrubel, *Agricultural Biotechnology and the Environment: Science, Policy, and Social Issues* (Urbana: University of Illinois Press, 1996), pp. 73–97, 138–65.

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Arizona, and a second laboratory at Carmel, California, within the first decade of the century, with the aim of combining field and laboratory research in physiology, ecology, genetics, and cytology to gain a better understanding of the process of evolution in plants. Research conducted at the Carnegie facilities concerned the geographical distribution and physiological tolerances of desert plants, the identification of distinct ecological "races" within plant species, and studies of polyploid species (that is, species with more than one complete set of chromosomes), also the subject of much of Vavilov's research. This combination of techniques and perspectives contributed significantly to plant systematics and to the synthesis of Darwinian evolution theory and Mendelian genetics (see Burian and Zallen, Chapter 23, this volume).[28]

Along somewhat different lines, the tobacco mosaic virus, which figured prominently in early speculations regarding the chemical nature of the gene, was discovered and analyzed through another combination of basic and applied research. The first virus identified as such – by Russian botanist D. I. Ivanovsky (1864–1920), who was sent to the Crimea in the 1890s to study diseases affecting tobacco plants in that region – tobacco mosaic virus became the subject of considerable biochemical investigation in the early twentieth century. Its isolation in crystalline form in the 1930s involved research carried out at the Rockefeller Institute plant pathology division in Princeton, New Jersey, the Rothamsted Experimental Station in England, and the Boyce Thompson Institute in New York, a unique private facility dedicated to basic research in botany. The critical experimental work on tobacco mosaic virus was done by Wendell Stanley (1904–1971) of the Rockefeller Institute in 1935, extending a research program begun a few years earlier at Boyce Thompson. Plant pathologists at the Rothamsted station shifted their focus from a virus affecting potatoes, a more economically important crop in Britain, to the tobacco virus, when they realized the significance of the initial work at the Boyce Thompson Institute. They corroborated Stanley's work in 1936, but neither they nor Stanley recognized the role played by nucleic acid, in this case RNA, in the virus. Nevertheless, x-ray diffraction photos of tobacco mosaic viruses yielded crucial clues in James Watson and Francis Crick's discovery of the helical structure of DNA in 1953.[29]

[28] Loren R. Graham, *Science, Philosophy, and Human Behavior in the Soviet Union* (New York: Columbia University Press, 1987), pp. 117–38; N. I. Vavilov, *The Origin, Variation, Immunity and Breeding of Cultivated Plants*, trans. K. Starr Chester (Waltham, Mass.: Chronica Botanica, 1951); Sharon E. Kingsland, "An Elusive Science: Ecological Enterprise in the Southwestern United States," in *Science and Nature: Essays in the History of the Environmental Sciences*, ed. Michael Shortland (Oxford: British Society for the History of Science, 1993), pp. 151–79; Joel B. Hagen, "Experimentalists and Naturalists in Twentieth-Century Botany: Experimental Taxonomy, 1920–1950," *Journal of the History of Biology*, 17 (1984), 249–70; Vassiliki Betty Smocovitis, "G. Ledyard Stebbins, Jr. and the Evolutionary Synthesis (1924–1950)," *American Journal of Botany*, 84 (1997), 1625–37.

[29] Robert Olby, *The Path to the Double Helix* (Seattle: University of Washington Press, 1974), pp. 156–60; William Crocker, *Growth of Plants: Twenty Years' Research at Boyce Thompson Institute* (New York: Reinhold, 1948), pp. 1–9; Angela N. H. Creager, *The Life of a Virus: Tobacco Mosaic Virus as an Experimental Model, 1930–1965* (Chicago: University of Chicago Press, 2002).

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Botany's long association with medicine and pharmacology took on new dimensions during the last two centuries as a result of wide-ranging field investigations combined with advances in physiology and biochemistry. Botanical gardens played a major role. Strychnine was introduced into medical research in the early nineteenth century when A. L. de Jussieu identified the plant source of an arrow poison brought back to the Jardin des Plantes by a botanist returning from Java. In the 1860s and 1870s, Joseph Hooker dedicated the resources of Kew and several colonial gardens to the collection and cultivation of cinchona, the bark of which was the source of quinine for the treatment of malaria. Collectors usually had to rely on the expertise of local people to identify the correct trees. By the turn of the century, the term "ethnobotany" had been given to this practice of utilizing local folk knowledge to identify valuable plant resources either as a research tool for cultural anthropology or as a means for discovering important medicines and drugs. The pharmaceutical industry maintained a keen interest in natural botanical sources because the first stage in the manufacture of synthetic drugs is always the identification of the biologically active substance in the natural product. For example, ephedrine, long used in its natural form in China, was introduced to Western medicine in the 1920s, when German and Chinese pharmacologists succeeded in isolating it from its plant source, duplicating work that had been done first by a Japanese researcher in the 1880s. Throughout the twentieth century, university laboratories, botanical gardens, and drug companies collected and studied various poisons, narcotics, and hallucinogens. By the late twentieth century, ethnobotany had become a mainstay of the research programs of several institutions, including the Harvard Botanical Museum, the New York Botanical Garden, and the Kew Gardens, which often sponsored collecting expeditions into the tropics as joint ventures with pharmaceutical companies. Although the identification of useful, and marketable, botanical drug sources is still a central preoccupation, attention in ethnobotany has shifted since the 1980s to include questions of intellectual property rights, the preservation of biodiversity, and the health and rights of indigenous peoples. The broader and more socially responsible perspective is reflected in the increased use of the term "ethnoecology" for this research.[30]

[30] E. Wade Davis, "Ethnobotany: An Old Practice, a New Discipline," and Bo R. Holmsted, "Historical Perspective and Future of Ethnopharmacology," in *Ethnobotany: Evolution of a Discipline*, ed. Richard Evans Schultes and Siri von Reis (Portland, Ore.: Dioscorides Press, 1995), pp. 40–51, 320–37, respectively; Drayton, *Nature's Government*, pp. 206–11; Darrell A. Posey, "Safeguarding Traditional Resource Rights of Indigenous Peoples," in *Ethnoecology: Situated Knowledge/Located Lives*, ed. Virginia D. Nazarea (Tucson: University of Arizona Press, 1999), pp. 217–29; Gary J. Martin, *Ethnobotany: A Methods Manual* (London: Chapman and Hall, 1995), pp. xvi–xxiv.

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# 14

# EVOLUTION

*Jonathan Hodge*

Biologists today answer many questions with the theory of evolution. How do new species arise? By evolution: by descent with modification from older species. Why do bird species all have two legs and two wings? Because they have all descended, evolved, from a single common ancestral species with these features. How has life progressed from the first few simple organisms billions of years ago? By evolution: by multiplication, diversification, and complexification of their descendants.

The study of evolution today forms a distinct discipline: evolutionary biology. This discipline more than most invokes its own ancestors. A recent contributor such as John Maynard Smith looks back to J. B. S. Haldane in the 1920s and to August Weismann in the 1880s. They in turn looked back to Charles Darwin, author of *On the Origin of Species* (1859), who saw himself following paths first taken by his own grandfather, Erasmus Darwin, and by Jean-Baptiste Lamarck, both writing around 1800.

All these conscious followings of earlier precedents constitute a genuine historical continuity of succession. However, when today's biologists look back to Charles Darwin or Lamarck, they usually add two further judgments. First, they assume a sameness of enterprise, with everyone contributing to evolutionary biology as found in a current textbook. However, a historian of science cannot make this assumption, being trained and paid, indeed, to ask: How might the enterprises and thus the agendas have changed and why? The second assumption biologists usually make is that only evolution gives fully scientific answers to their questions, and all other answers are ancient religious dogmas or persistent metaphysical preconceptions. This view – that the theory of evolution is a requirement for being a properly modern professional man (women were hardly included) of science – goes back to the 1860s campaigns for Darwin. Science was then often demarcated, in accord with new positivist notions of science, by this very contrast with religion and metaphysics, so that the rise of evolution and fall of Hebrew creation or Hellenic stasis was subsumed within the rise of modern, scientific



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ways of thinking and feeling about ourselves and nature. Again, historians are trained and paid to study such subsumptions but not to embrace them, for they promote questionable assumptions, especially about unities of enterprise.

Antidotes to such assumptions are most needed when considering the earliest members of a continuous succession celebrated by biologists today. One good antidote is the truism that everyone, especially pioneers, forms and enacts their intentions as responses to what has already happened and not to what still lies decades in the future. A history for the succession that this chapter is about should begin, then, by recalling how students of life's history and diversity around 1800 viewed their own past. To what did they look back? Whose footsteps did they wish to follow or to avoid? These are always instructive opening queries for a historian of any human activity.[1]

## THE INFLUENCE OF BUFFON AND LINNAEUS

Ask the preceding questions of the natural philosophers and natural historians active around 1800, and it is plain that they were far from operating within a common consensus of ideals and practices, or Kuhnian paradigm (see Di Gregorio, Chapter 12, this volume). However, they did often share the view that a principal challenge was what to do with decisive but divisive legacies from the generation before: the works of the Frenchman Georges Buffon (1707–1788) and the Swede Carl Linné, better known in Latin as Linnaeus (1707–1778). Naturally, they disagreed over how to meet this challenge.

For Buffon, the two principal tasks for the naturalist as theorist were the theory of the earth and the theory of generation. Both tasks demanded cosmogonies: a macrocosmogony for the origins of the order in the solar system and a microcosmogony for the origins of the order in any adult animal generated initially as a germinal chaos. Buffon's *Époques de la Nature* (Epochs of Nature, 1778) integrates the two theories. On any planet, as it cools, heat produces organic molecules that spontaneously generate the first members of any new species, and the stable configurations of force among

[1] There are many histories of evolution theory. Classics include Loren Eiseley, *Darwin's Century: Evolution and the Men Who Discovered It* (New York: Doubleday, 1958); John C. Greene, *The Death of Adam: Evolution and Its Impact on Western Thought* (Ames: Iowa State University Press, 1959). A study by one of the founders of the modern synthesis includes much on evolution; see Ernst Mayr, *The Growth of Biological Thought: Diversity, Evolution and Inheritance* (Cambridge, Mass.: Harvard University Press, 1982). Three recent works with extensive bibliogaphies are: Michael Ruse, *Monad to Man: The Concept of Progress in Evolutionary Biology* (Cambridge, Mass.: Harvard University Press, 1996); Donald J. Depew and Bruce H. Weber, *Darwinism Evolving: Systems Dynamics and the Genealogy of Natural Selection* (Cambridge, Mass.: MIT Press, 1995); Peter J. Bowler, *Evolution: The History of an Idea* (Berkeley: The University of California Press, 1983; 3rd ed., 2003). For a collection of recent evaluations, see Michael Ruse, ed., "The 'Darwinian Revolution': Whether, What and Whose?" Special issue of *Journal of the History of Biology*, 38 (Spring 2005), 1–152.

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these molecules, stable organic molds, enable the species to perpetuate itself as long thereafter as the temperatures needed are accessible. In Buffon's quest, there was no search for any taxonomic order, for classification and nomenclature were always, he held, arbitrary and conventional rather than natural.[2] By contrast, Linnaeus eschewed cosmogonies and took the reform of classification and nomenclature to be his prime responsibility as a naturalist. Where Buffon brought Newtonian natural philosophy to Lucretian and more recent Cartesian cosmogonical tasks, Linnaeus took up the Aristotelian systematic agenda as revived in the Renaissance by Andreas Cesalpino and others. Besides constructing artificial systems of classification, Linnaeus argued, too, for a natural classification, grouping and dividing animals, plants, and minerals according to their natural essential properties and relations as given them at creation by the biblical God.

These comprehensive contrasts between Buffon and Linnaeus made them a hard pair of acts to follow – and make implausible the claim by Michel Foucault that they were both singing off the same episteme, the same epochal structure of rules for the constitution of knowledge.[3] On the largest issues dividing the two men, no follower could avoid taking sides. However, on a raft of consequential matters, some picking and mixing was going on by 1800. Consider three instances. First, Linnaeus's teaching that plants, like animals, have sex went well with Buffon's delimitation of species as intersterile races, for species among all living beings could then be seen as reproductively separated successions. Second, when arranging taxonomic groupings by organizational affinities, it was agreed, as Buffon and Linnaeus had suggested, that no single linear serial arrangement was feasible and that figures such as trees, maps, and nets fit better. Third, on the literal geography of species around the world, Buffon, who had each species originating at a single place but different species at different places, was seen to have discredited Linnaeus's single original island Eden.

The great divergences among, say, Georges Cuvier, Lorenz Oken, and Jean Lamarck were prosecuted despite any consensus over such pickings and mixings. Cuvier (1769–1832), deploying the comparative anatomy newly developed since Buffon and Linnaeus, referred inner structural resemblances and differences to natural discriminations among the functions of digestion, respiration, sensation, locomotion, and so on. Here, there was no engagement with any legacy from Buffon's two cosmogonies. For Cuvier, successive extinctions of species and, possibly, progressive introduction – by unspecified means – of higher and higher types of life upon the earth were

[2] On Buffon, see Jacques Roger, *Buffon: A Life in Natural History*, trans. Sarah L. Bonnefoi (Ithaca, N.Y.: Cornell University Press, 1997). More generally on the eighteenth century, see Jacques Roger, *Life Sciences in Eighteenth-Century French Thought*, trans. Robert Ellrich (Stanford, Calif.: Stanford University Press, 1998).

[3] Michel Foucault, *The Order of Things: The Archaeology of the Human Sciences* (New York: Pantheon, 1970).

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revealed by research in stratigraphic paleontology rather than derived from any cosmogonical scheme.[4] This disengagement from cosmogonical science went well with Cuvier's personal caution during turbulent times, and with one emerging conception of a professional savant who, unlike Buffon of the ancien régime, was expected to keep his public theorizing close to consensual evidential norms.

The tradition of idealistic German nature philosophy that Oken (1779–1851) embraced was widely thought to be excessively speculative, although even critics valued Oken's embryology and anatomy. The philosophical speculations inspired comparative and taxonomic inquiries into transcendental unities (the skull is composed of vertebrae), into parallels between the small and large (embryos successively assume in their epigeneses the forms of animal types below them in the scale of perfection), into gradations between the lower and the higher (all animal structures are so many dismemberments of the highest human form), and into developmental laws directing all forces to forms (the massive and generic is everywhere made differentiated and individual).[5] With life and soul informing the mineral realm as well as plant and animal realms, even the first humans may have arisen in parentless, spontaneous generations.

The preoccupation with inner powers tending toward structural symmetries entails giving the formal priority over the functional and historical.[6] For Oken, marine fishes differ from land mammals not, ultimately, as designs for different ways of life lived in different circumstances but because they are lower on the scale of form from man. The unity between fish and man, and the lower perfection distinguishing fish, is disclosed by every current, epigenetic, ontogenetic transformation from fish to man. But geological or geographical histories of temporal, spatial, and causal relations between land and life are marginal to Oken's agenda of relating forces and forms.

## LAMARCK: THE DIRECT AND INDIRECT PRODUCTION BY NATURE OF ALL LIVING BODIES

First published in 1800, the views for which Lamarck (1744–1829) became notorious had arisen in the 1790s when he replaced very different views he

[4] Martin J. S. Rudwick, *Georges Cuvier, Fossil Bones and Geological Catastrophes* (Chicago: University of Chicago Press, 1997). See also Rainger, Chapter 11, this volume.

[5] On the role of the law of parallelism between embryological and evolutionary development, and its later manifestation as the law of recapitulation, see Stephen Jay Gould, *Ontogeny and Phylogeny* (Cambridge, Mass.: Harvard University Press, 1977).

[6] The classic study is E. S. Russell, *Form and Function: A Contribution to the History of Animal Morphology* (London: John Murray, 1916). A recent study of the idealist movement in German thought, including Oken's morphology, is Robert J. Richards, *The Romantic Conception of Life: Science and Philosophy in the Age of Goethe* (Chicago: University of Chicago Press, 2002).

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had held since the 1770s.[7] Although once a protégé of Buffon, he never adopted his mentor's two cosmogonies. The early Lamarck's earth has been steadily heated by the sun for a limitless past, with the present plant and animal species perpetuating themselves fixedly. Only the special forces in living bodies can compound matter into minerals such as chalk, and these decompose once life's action on them ceases, progressively degrading into lower minerals such as granite. No natural powers can produce any living body, so, as the highest minerals are products of life and the lower products of the higher, no minerals, and indeed no bodies at all, are properly products of nature.

By 1800, Lamarck had reversed himself strikingly. The earth continues to be steadily heated as before, with its cyclic destruction and renovation of land credited to untiring aqueous agencies, while the highest mineral compounds remain directly, and the lower indirectly, produced by vital actions. But now Lamarck has all living bodies produced by nature. Only the simplest can ever arise as direct productions from ordinary matter in spontaneous, parentless productions, so all the more complex ones have necessarily been produced successively over vast eons of the earth's limitless, uniform past as a habitable, terraqueous globe.

Now, on standard historiographical routines, one would label Lamarck's indirect production "evolution" (or "transformism" or some other term from later in the century) and proceed to distinguish Lamarck's "factual evidence for evolution" from his "theory of its causal mechanism" before bringing the case of Lamarck within one's scheme for the "rise of evolutionary thinking." However, when rejecting the unity of enterprise assumptions made by these routines, one asks instead how Lamarck himself was responding to what was available to him.

The decisive issue arose with Lamarck's new awareness, in the mid-1790s, of a graduated scale of internal structural organization in the series of classes from the mammals down to the infusorians. Lamarck, formerly a botanist, had ranked plant genera in a perfectional series but not in a ranking of internal organization down to the minimum consistent with any vital activity, such as the new comparative anatomy of animals disclosed. The issue was then whether to go beyond his new acceptance of this graduated scale to interpreting it as an order of successive, continuous, progressive production from low to high, an inverted complement of his long-standing production of minerals from high to low. For this step, Lamarck had to reverse years of putting the production of life outside nature and beyond science, and his writings of the mid-1790s show him explicitly making that reversal. In Revolutionary France, a scientific servant of the republican citizenry more

[7] See for instance M. J. S. Hodge, "Lamarck's Science of Living Bodies," *British Journal for the History of Science*, 5 (1971), 323–52; Richard W. Burkhardt, Jr., *The Spirit of System: Lamarck and Evolutionary Biology* (Cambridge, Mass.: Harvard University Press, 1977).

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naturally engaged in explanatory tasks the ancien régime had assigned to the church.

Just as mineral degradation had always, for Lamarck, resulted from the essence of mineral composition, so the inner essence of all living bodies – active, contained fluids moving in solid containing cellular tissues – is now responsible for making structural organization more complex over myriad generations and independently of all external contingencies. A secondary, accidental causation disturbs this serial progression from one class to the next, for on meeting contingent aquatic circumstances, say, land mammals have acquired new habits in catching fish, entailing new limb movements and so new motions for inner fluids, with the effects, webbed feet, passed hereditarily to future generations. Whereas the primary essential causation makes for a linear class progression that is not adaptive, the secondary accidental causation yields adaptive ramifying diversification within a class, giving new genera and orders of new species, so that fossils of species no longer living may not record terminal failures to survive changed circumstances.

Past reptiles and future reptiles arise from past and future complexification of fish antecedents, but not of the same fishes, so their common reptile characters result not from a common ancestry but from a common complexifying tendency limited at any degree of class organization to one structural type. Even with secondary causal contingencies, were a particular higher species extinguished it would eventually be replaced, although only, Lamarck insists, over the long ages required by an indirect production starting from merest monads.

For the Newtonian Lamarck, as for Buffon, nature's ultimate powers are attractive (gravitational) and repulsive (thermal) forces, and as with Buffon, nature has by these powers produced all organization. But the intermediary for Lamarck is not any organic molecules (explicitly discredited by his mineral composition theory) able in ancient, hotter times to assemble themselves as readily into a mammoth as they now do into an infusorian. The intermediary has always been infusorial organization in steady production on a steady earth free from Buffonian thermal decline. So, the correct account of how gravity and heat cause organized bodies requires the first complex ones to come along after the first simple ones because uniformity, lack of advance or decline in the physical world of nature, entails a progression in the living world.

With Lamarck's theorizing read, as he himself understood it, as a Newtonian replacement for Buffon's two Newtonian cosmogonies, the latest "evolution" historiography can now be evaluated. Surveying the entire procession from Lamarck to Maynard Smith, Michael Ruse urges that evolution as an idea in biology has always been an idea about society – progress – transferred to nature.[8] There is an initial difficulty with this transfer scheme in that

[8] Ruse, *Monad to Man.*

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social progress was a commonplace, definitive indeed of modernity itself, from about 1700 on, and yet "evolution in biology" only shows up around 1800. And the scheme fails to fit that paradigmatic pioneer, Lamarck, insofar as his progressionism about life is a corollary of his exclusion of progress and regress from nature itself. This failure does not discredit all relatings of scientific ideas to social ones, but it calls for a fresh analysis avoiding that historiographic anachronism "evolution in biology."

## AFTER CUVIER, OKEN, AND LAMARCK

There was, unsurprisingly, no single resolution of the fundamental differences among Cuvier, Oken, Lamarck, and others publishing in the first three decades of the century. Most options were seen as having disturbing metaphysical, religious, and political consequences. Lamarck's animal ancestry for man, and referral of mental differences to organizational diversity, looked threateningly materialistic and thus subversive of private and public moral order. Although Lamarck himself took up no radical cause, others invoked his views in doing so. Oken's idealism and animism seemed pantheistically unorthodox as religion and disconcertingly liberal in celebrating spirit as a principle of freedom in nature and man. By contrast, Cuvier's hostility to materialism, idealism, and animism coupled with his respect for biblical scholarship in integrating human and prehuman history was congenial to many of his fellow Christians.[9]

It can be tempting to view the great Muséum d'Histoire Naturelle in Paris, under Cuvier's direction, as an epitome for all natural history and comparative anatomy. There Cuvier opposed not only Lamarck but another colleague, Etienne Geoffroy Saint-Hilaire (1772–1844). Although closer to Oken than the two others, at least in his insistence on the priority of formal unities over functional identities, Geoffroy's views were more materialist than idealist or animist and agreed with Lamarck's in holding species indefinitely modifiable in changing circumstances. When Geoffroy proposed that all animals, invertebrate or vertebrate, embodied a single common plan, so that morphology transcended teleology, Cuvier countered publicly, just as he had attacked assumptions central to Lamarck's system.

The temptation to take the Parisian trio of Cuvier and his two opponents as a complete epitome of the age should be resisted because it not only reads German developments out of the story but suggests that two

[9] On the response to Lamarck and other early nineteenth-century controversies, see P. Corsi, *The Age of Lamarck: Evolutionary Theories in France, 1790–1830* (Berkeley: University of California Press, 1988); Adrian Desmond, *The Politics of Evolution: Morphology, Medicine and Reform in Radical London* (Chicago: University of Chicago Press, 1989); Toby Appel, *The Cuvier-Geoffroy Debate: French Biology in the Decades before Darwin* (Oxford: Oxford University Press, 1987).

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polarizations – "form" (Geoffroy) versus "function" (Cuvier) and "evolution" (Lamarck) versus "creation" (Cuvier) – provide an adequate matrix of available positions. But all dichotomous schemes, however permuted, oversimplify the multifarious, contingent, and contextual alignments then adopted.

Complexities in these alignments are illustrated by the ambitions of various younger men who became prominent in the 1820s. Two examples may suffice: Karl Ernst von Baer (1792–1876), an Estonian working in Germany, who sought to advance comparative anatomy and embryology; and Charles Lyell (1797–1875), a Scot working in England, who aimed to reform geology.

Whereas Oken and others had each embryo progressing from low to high, so that a rat has the form of a fish before mammalian form, von Baer had development going from the general to the specific. The rat is successively vertebral, mammalian, rodent, and then rat, and so never piscine. Moreover, no vertebrate is ever molluscan, so Cuvier's opposition to Geoffroy's unity of all animal types is upheld no less than his opposition to Lamarck's serial progression. Opposed, too, was the view of Oken's recent allies that embryonic progressions recapitulate developmental transmutations in the distant past. However, von Baer, too, liked to compare microcosm to macrocosm, likening these successive differentiations to those in the heavens whereby nebulae became stellar. Identifying degree of structural perfection with extent of differentiation and distinguishing degrees from types of structure, von Baer insisted that any degree is consistent with various types and that types of embryonic structure indicate natural classificatory groupings and divisions, thus advancing the Cuvierian taxonomic program while dropping its privileging of teleology over morphology.

Lyell's reform of geology opposed Cuvier's denial that geology could emulate more prestigious sciences by referring all ancient events recorded in the rocks to changes occurring in the present and potentially accessible to human experience. Reviving and modifying James Hutton's (1726–1797) theory of a stable, balanced system of aqueous and igneous agencies maintaining a permanently habitable earth's surface, Lyell argued that these presumptions should be favored because they entail the possibility of finding present causes for past effects. He rejected an emerging synthesis, favored by many geologists, of physical decline and organic progression, where neo-Buffonian cooling and calming has made the earth progressively fitter for higher and higher types of life, created in a progressive succession culminating in that most recent species, man. Such schemes implied, unacceptably to Lyell, that catastrophic events with many species extinctions, followed by new stockings, were confined to special periods quite unlike an allegedly quiescent present. Such progressionist schemes also encouraged moves from discontinuous miraculous creations to natural progressive productions like Lamarck's. In any case, as Lyell argued at great length, all that is

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known of species' lives at present discredits the mutability of species required by Lamarck.[10]

Lyell's account of how fixed species come and go on a steady, stable neo-Huttonian earth integrated geology and geography as never before. The integration invoked a providential principle of adaptation. Each species is created as a single pair, at the place best suited to its subsequent life of multiplying its numbers, extending its range, and varying adaptively and varietally within its specific limits to fit itself to variations in conditions before further changes in conditions, often favoring other species, bring a loss of competitive balance and numbers, gradually leading to extinction. Collectively, species are born and die not in big batches at special times but gradually, continually, although too infrequently for any species origin to have been authoritatively witnessed and recorded. Limited migratory powers and opportunities, not adaptive limitations, explain the absence of, say, the lion from South America or jaguar from Africa, barriers to migration, such as mountain ranges or seas, having been made and unmade during the vast time extant species have been originating. However, adaptive limitations do explain supraspecific presences and absences. Remote oceanic tropical islands likely never had mammal species originate on them, for they are better suited for reptile life. On this steadily habitable earth – where climate changes are caused not by irreversible losses of initial heat but reversible changes in the distribution of land and sea – somewhere there was land suitable for mammals when, with Europe having tropical temperatures, the oldest known fossiliferous rocks, the carboniferous, were formed, so the principle of adaptation entails no progressive introduction of life's main types.

The births and deaths of species Lyell construes demographically and statistically for, with births balancing deaths over the long run, rock formations can be ordered in time by the percentage of extant rather than extinct species they entomb. Here what counts for Lyell are not the comparative anatomists' groupings and gradings by type or degree of organization but the entirely abstract requirement that, as with counts of individual people or rabbits, one can tell one species, as a quasi-individual, from another, and that no species dies or is born more than once, so that extinction is forever and each species birth is the birth of a new species.

In Lyell's integration of geology and geography, with its providential teleology and abstract statistics for the exchange of species, there are more pages, hundreds not dozens, devoted to generalizing about species among living beings than for any previous author. The reform of geology proposed in the

[10] On Lyell's uniformitarianism, see Martin J. S. Rudwick, "Introduction," in the facsimile reprint of Charles Lyell, *Principles of Geology*, 3 vols. (Chicago: University of Chicago Press, 1990–1), vol. 1, pp. vii–lviii. More specifically, see Michael Bartholomew, "Lyell and Evolution: An Account of Lyell's Response to the Prospect of an Evolutionary Ancestry for Man," *British Journal for the History of Science*, 6 (1973), 261–303.

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three volumes of his *Principles of Geology* (1830–3) entailed that species as quasi-individuals take on as a topic lives (and deaths) of their own as never before.

Even twenty years on, almost no one was supporting Lyell's new history for life on earth. Two who agreed in not doing so, while disagreeing among themselves, were the German Swiss, soon to be American, Louis Agassiz and the Scot Robert Chambers. For Agassiz (1807–1873), a progressionist and catastrophist history for life on earth revealed a threefold parallelism between ontogenetic progression, organizational ranking, and a paleontological sequence from low to high, undifferentiated to specialized, executing a grand Platonic plan essentially unconditioned by the transformations of the inorganic world. Each fixed species was created independently of earlier ones and, even initially, over a wide range in large numbers.[11] By contrast, Chambers (1802–1871) saw, in the nebular condensations in the heavens, progressive changes effected by natural, unmiraculous agency, and presumed that any plan such as Agassiz's for terrestrial life could be executed no less lawfully. If occasionally an ontogenetic progression advances beyond the adult parental peak, so that the offspring is of a different, slightly higher species, then over eons life could rise from the lowest forms, even now provided by spontaneous generation, to the highest types. Those very young islands, the Galapagos, have as yet no mammals, only a development from marine fish to terrene reptiles. Continental Africa and South America have both had their life lines rise independently to monkey form, showing, because intercontinental monkey migration is impossible, that the same laws of development have produced the same outcome, with only minor variations caused by local conditions. Denounced by many professionals, Chambers's anonymous *Vestiges of Creation* (1844), complete with its ape ancestry for man, was a popular sensation.[12]

## DARWIN: THE TREE OF LIFE AND NATURAL SELECTION

When Charles Darwin's *Origin of Species* appeared in November 1859, European and American discussion of life's history and diversity was mainly focused on the issues dividing Agassiz, Chambers, and Lyell. The theorizing in Darwin's book, however, was largely a product not of the 1850s but of two years' private work in 1837–9.[13] To understand Charles Darwin's own

[11] Edward Lurie, *Louis Agassiz: A Life in Science* (Chicago: University of Chicago Press, 1960).

[12] James A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation* (Chicago: University of Chicago Press, 2000). On Chambers's vision of evolution, see M. J. S. Hodge, "The Universal Gestation of Nature: Chambers' *Vestiges* and *Explanations*," *Journal of the History of Biology*, 5 (1972), 127–52.

[13] There are many biographies of Darwin; a recent study in two volumes is Janet Browne, *Charles Darwin: Voyaging* (London: Jonathan Cape, 1995) and *Charles Darwin: The Power of Place* (London: Jonathan Cape, 2002). For a sociological approach to his life and thought, see Adrian Desmond

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understanding of his successional context requires relating this work to four sources: Lyell, Robert Grant (1793–1874), Erasmus Darwin (1731–1802), and Lamarck. On any "evolution" historiography, one would assume that the last three, being "evolutionists," must have moved Charles Darwin to replace entirely Lyell's "creationist" account of life's diversity and history. But this assumption proves deeply misleading in that, on the contrary, Lyell's teaching often led Darwin to depart from precedents set by the others.

Charles Darwin's most comprehensive ambitions as a scientific theorist, as formed in the HMS *Beagle* voyage years (1831–6), were those tracing to his extensive informal apprenticeship in invertebrate zoology with Grant in Edinburgh in 1826–7 and those arising from his zealous commitment to Lyell's geological doctrines.[14] Grant at Edinburgh – who sided with Lamarck and Geoffroy against Cuvier and admired Erasmus Darwin – did not then prompt Charles Darwin to embrace any transmutationist views but did give him an abiding preoccupation with two highly general issues: individual life (as in a rabbit) versus associated or colonial life (as in some coral polyps) and sexual versus asexual generation. Lyell gave him a preoccupation with the gradual exchange of new species for old on a stably habitable earth and with the issue of how far adaptation had determined the timing and placement of those births and deaths of species.

In March 1837, Charles Darwin decided that Lyell's principle of adaptation should be replaced with a common ancestry for related species, therefore requiring the transmutation of species, because the species of many genera and families had originated in very diverse conditions and their common characters were best explained as caused by heredity, that is, by descent from a single common ancestral species, their differences being a subsequent branching adaptive diversification.

Lyell had insisted that anyone favoring any transmutation of species should engage Lamarck's whole system: spontaneous generation, the progression of classes, orang ancestry for man, and all. By July 1837 and the opening of his *Notebook B*, Darwin had done just that. This great systemic leap in his thinking presents a major biographical challenge best met by looking to Erasmus Darwin as Charles Darwin himself then did. Assimilated to the landed gentry in his final years, his memory celebrated by his son, Charles

and James R. Moore, *Darwin* (London: Michael Joseph, 1991). A valuable collection of Darwin scholarship, especially strong on the origins of the evolution theory, may be found in David Kohn, ed., *The Darwinian Heritage* (Princeton, N.J.: Princeton University Press, 1985); see also Jonathan Hodge and Gregory Radick, eds., *The Cambridge Companion to Darwin* (Cambridge: Cambridge University Press, 2003, 2nd edition in press). For access to the complete works of Darwin, go to www.darwin-online.org.uk. See also Howard E. Gruber, *Darwin on Man: A Psychological Study of Scientific Creativity* (New York: Dutton, 1974).

14 On Darwin and Grant, see Philip R. Sloan, "Darwin's Invertebrate Program, 1826–1836," in Kohn, *The Darwinian Heritage*, pp. 71–120. On the influence of Lyell, see M. J. S. Hodge, "Darwin and the Laws of the Animate Part of the Terrestrial System (1835–1837)," *Studies in the History of Biology*, 6 (1982), 1–106.

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Darwin's revered father, Erasmus Darwin was a proof within the family that radical ideas about life – often associated with Lamarck's – and about society were a natural corollary of high rank and respectability. By July 1837, Charles Darwin had reread his grandfather's *Zoonomia* and put that word, meaning "the laws of life," as the very heading for his own *Notebook B*. Erasmus Darwin had offered no systemic structure to be emulated and the comprehensive zoonomical system opening his grandson's notebook accordingly conforms to the structure given Lamarck's system by Lyell. The grandparental precedent inspired and sanctioned this emulation of the Lamarckian precedent.

Lyell's exposition of Lamarck's system departed strikingly from Lamarck's own, and Charles Darwin's system makes further departures, most notably in including no internally caused tendency toward progression independent of adaptation to changing circumstances. What is more, further fundamental changes are made right away that bring an endless exchange of species within an unlimited arboriform descent as eventually depicted in the one diagram in *Origin of Species*. Charles Darwin wonders why the most perfect groups of animals, such as mammals, have the most extinctions and most conspicuous character gaps between their subgroups, and his reflections are both Lyellian and Grantian. One parent species, he reflects, generates one or more offspring by splitting: a quasi-budding, a quasi-asexual generation. These multiplicative births by division must be balanced by deaths, extinctions. So, for every species that has a dozen descendant species, eleven in the same period must end without issue. Splitting is accompanied by divergence, so with more time and splittings and divergences, and the production of wider and wider groupings – families, orders, and on to classes – the greater will be the gaps between subgroups, all the way even to the division between plants and animals. Darwin's former correlation between group perfection, extinctions, and gaps is replaced, then, with one between group width, extinctions, and gaps. The resultant scheme is Lyellian in that it is an abstract representation of continual, endless species loss and repletion. It is un-Lyellian in allowing for progress; Darwin continues to think that although all change is adaptive, most adaptive change is progressive. But this is progress as a concomitant of adaptational innovation rather than progress necessitated by the completion of God or nature's plan. Darwin held that adaptive change, and thus progress, is all made possible by the two features distinguishing sexual from asexual generation: two parents and maturation is the offspring. Maturation is recapitulative of past change and also innovative, for an immature organization can acquire new heritable, adaptive variations in changing conditions. Biparental breeding is conservative in blending out minor variations caused by fluctuating local alterations in conditions, thus allowing for progress as species adapt slowly and irreversibly to permanent changes over their whole range. Increasingly, Charles Darwin traced adaptive structural changes to changes in habits leading to heritable changes in the use of limbs, say, much in the manner of Lamarck, although Darwin mistakenly thought

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the Frenchman's own theory invoked conscious will rather than unconscious habits. Slow, prolonged adaptive divergences between two or more varieties of a species will eventually be accompanied by an aversion to interbreeding and later intersterility, Darwin argued, and thus to the formation of races that would count not as mere varieties but as good species.

In late September 1838, on reading Robert Malthus's essay on the tendency of populations to vastly outstrip food supplies, Darwin added to his own theory the argument that although Malthusian populational wedging makes all species liable to extinction, as Lyell had argued, in competitive defeats initiated by slight changes in conditions, this wedging also ensures that the winning species become adapted to these changes by the sorting out of structural variations and by the retention of advantageous, and elimination of disadvantageous, variants.[15] But at this time Darwin drew no comparison between this sorting and the art of selective breeding practiced by farmers and gardeners.

Late in November 1838, he distinguished explicitly for the first time between two principles of adaptive change in structure. In one, familiarly now, a parent blacksmith who develops strong arms through habitual use passes this character to the children; in the second, a child born by chance with stronger arms survives more surely than others to pass on the advantageous variation. However, Darwin admitted defeat in deciding which adaptive changes might be caused by which of these two principles. A week or so later, he appears to deliberately circumvent rather than resolve this dilemma by enunciating three principles that can, he says, account for all changes. These three principles seem designed to subsume, rather than choose between, the earlier two, for they are quite general: heredity; a tendency toward variation in changing conditions; and Malthusian superfecundity. Within a few more days, Darwin articulated for the first recorded time a comparison between the sorting, entailed by the struggle for existence consequent on that superfecundity, and the formation of races of dogs, say, by man's selective breeding. The comparison is soon articulated as an argument by analogy, by proportion. The power of natural selection, because of its much greater comprehensiveness, precision, and prolongation, is vastly greater than that of man's selection; as a greater power, it will be capable of proportionally greater effects than man's and thus of producing the unlimited adaptive diversification of a species into many descendant species as represented in the tree of life. Again, although, as Darwin soon emphasizes, this analogy allows adaptive changes to start as chance variations, there is no exclusion of his older commitment to adaptive, structural changes arising from the inherited effects of habitual use. Nor will

[15] The influence of Malthus is a source of much controversy, arising from the implication that the selection theory may be a product of laissez-faire social philosophy. See Robert M. Young, "Malthus and the Evolutionists," reprinted in Robert M. Young, *Darwin's Metaphor: Nature's Place in Victorian Culture* (Cambridge: Cambridge University Press, 1885); Peter J. Bowler, "Malthus, Darwin, and the Concept of Struggle," *Journal of the History of Ideas*, 37 (1976), 631–50.

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that exclusion ever be made, for, in *Origin of Species*, the three principles of late 1838 are still comprehending rather than deciding between the two principles they were originally designed to subsume.

*The Origin of Species* can be and was read as ultimately a conjunction of the tree of life, as a theory about the course of life's history, and natural selection as a theory of the main agency causing life's history to take that course. A crude "evolution" historiography for the book might say that in it Darwin made evolution branching (whereas Lamarck had made it lineal) and credited it to natural selection rather than to Lamarckian causes. What the notebook work and its various contextual conditionings show is that such a summary misconstrues Darwin's own understanding of what he was doing, including his conscious following of precedents set by Lamarck, and misrepresents the challenge he presented to his readership in 1859.

As for the wider contexts of his theorizing, historiographical consensus proves hard to come by. Is Darwin's theorizing in the manner of Adam Smith's "invisible hand" in political economy, with individuals' pursuit of their self-interest making for the greatest collective advantage? One may doubt it in that sexual generation, an essential cause of adaptive change, is for Darwin not in the individual's interest but in the higher interest of the species. Is his theorizing in the manner of Newton's celestial mechanics? One may doubt this, too, in that there is no law, for Darwin's cause, natural selection, which is to that cause as Newton's inverse-square law is to the gravitational force. Are Darwin's ideas the ideas of a new ruling class – an urban, industrial bourgeoisie? Perhaps, but perhaps not: The bourgeoisie were not yet the ruling class in England, and Darwin's thinking, including his use of Malthus, often has affinities with the ideals and practices of the older aristocratic and gentlemanly capitalisms embodied in landed estates, agricultural improvements, colonial settlements, and foreign trade rather than in cities, factories, and machines. Malthus, with his political and economic privileging of land and food, and pamphlets favoring the Corn Laws, was aligned with these older capitalisms rather than the newer capitalism epitomized by Manchester and Leeds. Relating Darwinian science to England's aristocratic and gentlemanly capitalisms rather than to its bourgeois capitalism requires rethinking both that science and that society, but such a rethinking may well be needed.

## AFTER DARWIN

The altered state of opinion created by Charles Darwin was less consensual than is often thought, for biologists did not merely disagree about the causes of evolution while agreeing about evolution itself; they disagreed deeply about evolution as such. Peter Bowler, modifying distinctions made by Stephen Jay Gould, emphasizes three enduring issues dividing biologists since the

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1860s:[16] Is evolution gradual or jumpy? Is it externally or internally directed? And is it regular or irregular? A major advantage to concentrating on these issues is, as Bowler emphasizes, that they bring out how all thinking about evolution after Darwin has also come after Cuvier, Lamarck, Geoffroy, Oken, von Baer, Owen, and the rest, not to mention Plato, Aristotle, and Lucretius. Take Darwin himself; he is a gradualist or smoothie, not a jumper or saltationist; an externalist or extrovert, not an introvert; and an irregular rather than regular guy. By contrast, Chambers has saltationary changes determined, like a puppy growing into a dog, by internal causes and following reliable regularities, with all reptiles always tending to mammalhood rather than a very few exceptional reptiles happening once, thanks to special circumstances, to become ancestral to the first mammals. Predictably enough, not everyone sided with Darwin on all three issues. A smoothie could be an introvert and a regular guy, and all other permutations are represented. Some authors could provide plural precedents. Those making Lamarck's inheritance of acquired characters, arising from changes in habits, the sole cause of evolution could be as gradualist, externalist, and irregularist as Darwin himself, while Lamarck's internally caused tendency toward progressive escalation independently of environmental circumstances could be opposed to this trio of alignments.

Hostility to natural selection abounded; many biologists disparaged it as chancy, unreliable, cruel, and wasteful, as well as insufficiently supported, perhaps indeed refutable, by what was known about heredity or about the limited time some physicists thought available for evolution because a young earth would have been too hot for life. But independently of dissatisfactions with natural selection, Darwin's tree of life satisfied some biologists less than others. Geographers and geologists often followed Darwin in referring the common confinement of a family or order of species to a geographical region or to a geological epoch to their common ancestry and arboriform diversification. Comparative anatomists could remain unimpressed, however; the Cuvierian emphasis on the fitting of inner structures, such as the heart and lungs, to each other, making possible the life of the whole, was hardly illuminated thereby. Again, the unities of type, beloved by morphologists, often conformed to symmetries and repetitions in structural elements that common ancestries and ramifying diversifications left little understood. These dissatisfactions never reduced to any unanimity, for there was, as in the social science of the day, little agreement on how to adjudicate between structural, functional, and historical interpretations and analyses. The late nineteenth

[16] Peter J. Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900* (Baltimore: Johns Hopkins University Press, 1983); Peter J. Bowler, *The Non-Darwinian Revolution: Reinterpreting a Historical Myth* (Baltimore: Johns Hopkins University Press, 1988). On the immediate response to Darwinism in different countries, see Thomas F. Glick, ed., *The Comparative Reception of Darwinism*, 2nd ed. (Chicago: University of Chicago Press, 1988).

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century saw much effort devoted to reconstructing the evolution of life on earth from anatomical, embryological, paleontological, and geographical evidence, but underlying conceptual debates often remained unresolved.[17]

When, in 1868, Darwin did offer a theory about the generation of individual organisms, his hypothesis of pangenesis, it hardly threw more light on either teleology or morphology. This was not surprising, as it was never designed to do so.[18] Constructed most likely around 1841, pangenesis – the culmination of Darwin's Grantian comparisons and contrasts between sexual and asexual generation – conjectured that all generation from chicken reproduction to healing in tree bark and budding in polyps is micro-ovulational gemmation. Each part of the two chicken parents buds off minute gemmules, minifacsimiles of the parent tissue, and the two lots of gemmules then come together to form the conceptus, through their growths, maturations, and fertilizations, eventually yielding an offspring like the parents. The hypothesis, being quite general and abstract in its articulation of this micro-ovulational gemmation, included no subsidiary suggestions as to how the undifferentiated conceptus of a higher organism becomes structured and functions as a developing fetus. The causal workings of ontogeny's recapitulations of phylogeny were hardly engaged.

Moreover, the hypothesis was seen to conflict with the newest cell theory's thesis that a sperm or an egg is a single cell arising, as all cells do, by the division of a prior cell. This conflict eventually provoked other theorists of generation, notably August Weismann (1834–1914) and Hugo De Vries (1848–1935) in the 1880s, to propose comprehensive hypotheses conforming to such cytological doctrines. However, these proposals led to no consensus about evolution. De Vries saw his theory of intracellular pangenesis as supporting his anti-gradualist, anti-externalist, and anti-irregularist views. Weismann saw his theory of the continuity of the germ plasm as vindicating Darwinian natural selection, divorced from any inheritance of acquired characters, as the all-powerful cause of gradual, externally directed, and irregular evolution. The desirability of integrating evolutionary biology and cellular biology was commonly acknowledged by the 1890s, but there was discord, not accord, about how to do so. Indeed, there was even a reopening of the eighteenth-century debates over preformation versus epigenesis in ontogeny, with explicit retrospects of those old issues.

It is true, then, that Alfred Russel Wallace (1823–1913), Darwin's junior partner in the independent construction of the theory of natural selection, and Weismann were championing in the 1890s a neo-Darwinism more Darwinian and less Lamarckian than Darwin's own, but this was a controversial, minority

[17] Peter J. Bowler, *Life's Splendid Drama: Evolutionary Biology and the Reconstruction of Life's Ancestry, 1860–1940* (Chicago: University of Chicago Press, 1996).

[18] On pangenesis and the later debates over heredity and evolution, see Jean Gayon, *Darwinism's Struggle for Survival: Heredity and the Hypothesis of Natural Selection* (Cambridge: Cambridge University Press, 1998).

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view.[19] The late nineteenth century, heir as it was to earlier divergences of outlook and doctrine, some centuries old, never settled into consensus, and so likewise for all the decades since.

## EVOLUTIONARY BIOLOGY SINCE MENDELISM

The year 1900 is often called the year when Gregor Mendel's work on heredity was rediscovered (see Burian and Zallen, Chapter 23, this volume). Although difficulties with this phrasing abound, it remains the case that within a few years many, if not all, biologists were convinced that what would soon be called Mendelism was here to stay and had fundamental implications for the understanding of evolution. Foremost among them was the saltationist, internalist, and regularist William Bateson (1861–1926). Opposing Bateson's Mendelism were W. F. R. Weldon (1860–1906) and Weldon's biometrician ally Karl Pearson (1857–1936), both of whom followed Darwin not only in his gradualism, externalism, and irregularism but in crediting most of evolution to natural selection. In the case of England, a main question is when and why this Mendelian–biometrician opposition was resolved so that a younger generation, headed perhaps by R. A. Fisher (1890–1962) from the mid-1910s on, could see consilience, not conflict, between Darwin's and Mendel's legacies. However, the question is less apt elsewhere. In the United States, E. B. Wilson (1856–1939) and W. E. Castle (1867–1962), for instance, were not raising students to choose between these two legacies, although there was little agreement on how consilience proceeded because Mendelian genetics itself contained dissent. Castle initally thought his modification of hooded rat coats by selective breeding required modification by mutual contamination of Mendelian genes, whereas others favored change in the frequency of modifier genes. Early integrations of Mendelism and Darwinism did not all fit the form that eventually became canonical in the 1930s.[20]

Furthermore, when turning to the 1930s and to the three men later looked back to as founders of a new evolutionary genetics that was both Mendelian and Darwinian – Fisher, Sewall Wright (1889–1988), and J. B. S. Haldane (1892–1964) – it is their divergences as much as their convergences that reveal the state of science in their day. Fisher and Wright, despite eventually aligning

[19] Wallace is a difficult figure to place. His independent discovery of natural selection in 1858 has prompted claims that he was a major player who has been systematically edited out of the story. In fact, there were significant differences between Darwin's and Wallace's views on natural selection, and Wallace's major contributions to biology came from his later work. For a recent study, see Martin Fichman, *An Elusive Victorian: The Evolution of Alfred Russel Wallace* (Chicago: University of Chicago Press, 2004).

[20] See William B. Provine, *The Origin of Theoretical Population Genetics* (Chicago: University of Chicago Press, 1971); Ernst Mayr and William B. Provine, eds., *The Evolutionary Synthesis: Perspectives on the Unification of Biology* (Cambridge, Mass.: Harvard University Press, 1980); Gayon, *Darwinism's Struggle for Survival.*

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their mathematics, disagreed on their biological conclusions. Fisher favored a single large interbreeding population subject throughout to common selective influences as most conducive to adaptive, progressive evolution, whereas Wright preferred a large population divided into small local ones, which interbred only a little and were subject to inbreeding and genetic drift as well as selection within and selective migration between them. More deeply, the two men had divergent agendas and styles.[21] For Fisher, the ultimate task was to use the new mathematics and the new genetics to vindicate natural selection as the only counterentropic agency and thus the only possible cause for evolution, now that Lamarckian influences were excluded. For Wright, an adjudicational pluralist rather than vindicational monist, and always a striker of balances as a theorist, the aim was to decide the relative contributions of many factors, some making for homogeneity and others for heterogeneity, in those causal interactions that had proven optimal in animal breeding practices and so, presumptively, in nature, too. Consensus resided in the view that the hereditary variation generated by the genetical system had now been shown – especially by T. H. Morgan's group at Columbia University – to be quite inadequate to produce adaptive and progressive change on its own, without natural selection. Although the variations from sexual reproduction with its myriad permutatory gene recombinations were enormously numerous, they were small in size and random with respect to adaptation, whereas those from gene mutations were, additionally, arising at very low, unalterable rates and were mostly recessive and mostly disadvantageous. However, the difficulty in following their mathematics, their failure to agree as evolutionary biologists, and their commitments to these views about mutations, views of which many biologists remained wary, ensured that few people decided that Fisher, Haldane, and Wright had cleared up all the mysteries in the genetics of evolution.

Nor was time for digestion and assimilation all that was needed over the next decade for these integrations of Mendelism and Darwinism to be seen to herald a new dawn, a new or "modern" synthesis as it would be called in the 1940s. That far more was needed is shown by the career of Theodosius Dobzhansky (1900–1975) and his book *Genetics and the Origin of Species* (1937), the single most influential text of its generation, perhaps of the century.[22] This book brought novel mathematical evolutionary genetics together with two other traditions. The first, drawn on by Dobzhansky before he emigrated to the United States in 1927, was the peculiarly Russian work on the experimental genetics of wild populations, especially *Drosophila*

[21] See M. J. S. Hodge, "Biology and Philosophy (Including Ideology): A Study of Fisher and Wright," in S. Sarkar, ed., *The Founders of Evolutionary Genetics* (Dordrecht: Kluwer, 1985), pp. 185–206. On Wright, see William B. Provine, *Sewall Wright and Evolutionary Biology* (Chicago: University of Chicago Press, 1986).

[22] See Mark B. Adams, ed., *The Evolution of Theodosius Dobzhansky* (Princeton, N.J.: Princeton University Press, 1994).

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flies, work that meshed closely with his earliest research on the taxonomy and biogeography of ladybird beetles. The second tradition was the cytological genetics of the Morgan school, which he joined on arriving in the United States. So, thanks to his personal collaborations with Wright after 1932, Dobzhansky was uniquely able to integrate Wright's theorizing with other kinds of genetical, biogeographical, and taxonomic research. The title of his book indicated a broader ambition than anyone else could assay in bringing to the old Darwinian questions the whole new science of genetics.

The Darwinian precedents are invoked by Dobzhansky in introducing the ultimate aim and structure of his book's entire exposition. Organic diversity, with its structural and functional discontinuities among species and higher taxa, is to be explained as the product of a gradual and continuous arboriform process, wherein changes above the level of species arise in reiterations of changes at and below the species level. The argument pivots on two central chapters, one on variation in natural populations and the next on selection. The earlier chapters of *Genetics and the Origin of Species* build toward the first of these, and the latter ones build on the two taken together. The opening analysis of gene mutations as studied in the laboratory is accordingly followed by an account of gene mutations and chromosomal aberrations as the basis for individual and racial differences in the wild. Variation in natural populations is then related both to the equilibrial tendencies entailed by Mendelian principles and to population size and structure, all in conformity with emphases shared by Dobzhansky's naturalist Russian mentors and his theoretician associate Wright. The selection chapter likewise moves to a Wrightian finale in explaining how the only cause of adaptive change, selection, is facilitated by inbreeding and genetic drift arising from the subdivisions of a species into small, partially isolated local populations. A chapter on polyploidy acknowledges the role of this source of sudden species formations in plants, but the remaining chapters, on the isolating mechanisms involved in species formation and on hybrid sterility, resume the gradualist, adaptationist, and selectionist themes, thus preparing the way for a Darwinian integration of evolution and classification in the book's closing chapter. Dobzhansky, knowingly partisan here, insisted from the start that his book's science, like Darwin's, was quite properly causal rather than historical, concerning the causes of evolution, not its course. Furthermore, the book was physiological rather than morphological, as Dobzhansky put it, in analyzing the agencies responsible for evolutionary processes rather than examining regularities in the products. The Christian, Romantic, liberal, anti-Stalinist Dobzhansky was passionately partisan in his life as in his science. A debate in the 1950s with another American Darwinian geneticist, the atheist, rationalist, and erstwhile Soviet sympathizer H. J. Muller, was initially over whether natural selection usually consumes genetic variation by favoring fitter homozygotes or often maintains it by favoring heterozygote individuals, but this debate escalated into clashes over the mutational consequences of atomic weapons

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testing, over eugenics, and thus to overtly irreconcilable oppositions of values and visions.

It can be tempting to draw a line in one's mind from Dobzhansky's first edition of his book, in 1937, to its fourth and final version, under a new title, in 1970, and on to his more general textbook *Evolution*, published in 1977 with three of his colleagues from the University of California at Davis and reaffirming without fundamental qualifications his mid-1930s views on how genetics contributes to evolutionary biology.[23] With *Evolution* taken, as many took it in 1977, to be a canonical exposition of the prevailing orthodoxy of its day, one could then see the accumulated agreements and disagreements with that 1930s position as paving the way for the late 1970s orthodoxy. However, the disagreements are too many and too fundamental to be read as disagreements about the conclusions constituting any 1930s legacy, for they have also been disagreements about assumptions, approaches, and strategies.

There are so many diverse reasons why such disagreements have arisen in the twentieth century that no tidy categorization of them can satisfy. Five clusterings may, however, serve to indicate the historiographical challenge. First, evolutionary theories have always involved divisive issues about nature and nurture, race and civilization, origins and destinies, progress and degeneration, chance, necessity, and design. Second, evolutionary theories have always faced difficulties of extrapolation, generalization, and instantiation in moving from fruit flies to humans, from the experimental short run to the natural long run, and from mathematical possibilities to empirical actualities. Third, disciplinary diversity makes for doctrinal discord. Embryologists and ecologists, for example, have often felt that their concepts and practices have been too little drawn on by orthodox evolutionary theory, which is after all supposed to link these two fields. Fourth, a sense of loss can promote dissatisfaction. Naturally, there is no unanimity over which traditions to revive, but J. W. von Goethe, Richard Owen, Wilhelm Roux, D'Arcy Thompson, and William Bateson are among the individuals, from the more or less remote past, whose teachings are still invoked in urging that proper attention should at last be given to, say, laws of form, structural archetypes, or developmental mechanics. Fifth, programmatic innovations can often seem threatening or distracting. When molecular biology first encroached on evolutionary biology in the 1960s, some saw it as hegemonically reductionistic in its doctrinal aims and economically aggressive in its territorial claims. More recently, complexity theorists' modelings of order at the edge of chaos have often seemed too distant from research into actual processes in real organisms.

These and other sources of diversity in evolutionary biologists' beliefs and attitudes obviously demand a historical geography and ecology of their own that would do justice to diversities in natural scientific cultures and to

[23] Theodosius Dobzhansky, Francesco J. Ayala, G. Ledyard Stebbins, and James W. Valentine, *Evolution* (San Francisco: W. H. Freeman, 1977).

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their conditioning by political, economic, and other developments. In the mid-century period, for example, central European emigration to the United States, together with that country's being less disrupted than others by World War II, allowed American biology to take the lead, as many scientists perceived it, in evolutionary biology, and, moreover, for American theorists to see themselves in the 1960s as less cut off than their English colleagues from valuable continental European traditions in morphological biology. Again in the 1960s, France, a country long conspicuously underrepresented in evolutionary theories taken up in other nations, became a dominant center for bacterial genetics and its bearing on evolutionary biology. This was no anomaly, as microbiological work itself was descended from a strong national tradition going back to Pasteur a century before. In its regional and national diversification, evolutionary biology is like most other human cultural activities in the last century, being directed and disseminated, or distracted and diverted, by all those trends and events studied by historians with no eye on the history of science, who can nevertheless greatly aid historians of science in their tasks.

## CONCLUSION: CONTROVERSIES AND CONTEXTS

The permanent tendencies toward controversy obviously make broad contextual considerations unavoidable of any historiography of this area of science. Or, rather, there is a need to question traditional views about where science begins and ends and where its surrounding context – whether political, religious, or whatever – begins and ends. Indeed, any talk of an inner scientific center and an outer setting that is economic, say, rather than scientific, needs questioning. A historian can then ask how such demarcations have been deployed in various ways for various purposes. Attempts were made in the 1940s and 1950s to give evolutionary biology a secure professional status as a recognized subdiscipline within and fundamental to biology, and these attempts appealed to particular demarcational lines of inclusion and exclusion that distinguished evolutionary theories as science from evolutionary theories as ideology. However, in the 1960s, when claims about the end of ideology were challenged, some biologists challenged the older inclusionary and exclusionary principles.

Equally, any history of the history of science profession in those three decades would yield parallel conclusions. But these are parallels, not convergences. Amicable collaborations between historians of science and biologists exploring recent evolutionary biology can be gratifying and fruitful. It remains unlikely, however, that historians' history of science and scientists' history of science will ever coincide, in their ends and means, as they work together but think for themselves.

This is not to say that all historians of science think alike when thinking for themselves. This chapter's very delineation of its topic is one that some

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historians would wish to see superseded in future work. Successions of grand theories, no matter how explicated textually or placed contextually, are precisely what much history of science now seeks to get away from and instead study the places, bodies, and practices (*praxes*) of many ordinary people at work in science, whether in the field, laboratory, museum, or lecture hall (see the following chapters in this volume: MacLeod, Chapter 3; Winsor, Chapter 4; Benson, Chapter 5; Harwood, Chapter 6). There is never likely to be agreement as to whether any one historiographical program or agenda needs to displace any other, much less all others, in pursuing its distinctive aims, or whether an irenic pluralism is possible. The history of the history of science suggests that at some times monistic attitudes predominate at least locally, while at others they do not. All the people – the natural philosophers, natural historians, and biologists – whether prominent and professional or entirely otherwise, who have made the history this chapter addresses have themselves disagreed sufficiently that it is hardly likely that any one historiographical alignment will satisfy every audience and readership. Perhaps one can hope that many kinds of flowers in many different habitats should be allowed to bloom.

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# 15

# ANATOMY, HISTOLOGY, AND CYTOLOGY

*Susan C. Lawrence*

> It is as though, when we look at the living body, we look at its reflection in an ever-running stream of water. The material substratum of the reflection, the water, is continually changing, but the reflection remains apparently static. If this analogy contains an element of truth, if, that is to say, we are justified in regarding the living body as a sort of reflection in a stream of material substance which continually passes through it, we are faced with the profound question – what is it that actually determines the 'reflection'? Here we approach one of the most fundamental riddles of biology – the 'riddle of form' as it has been called, the solution of which is still entirely obscure.
>
> Wilfred E. Le Gros Clark, *The Tissues of the Human Body*, 6th edition (Oxford: Clarendon Press, 1971), p. 9

Anatomy, histology, and cytology are sciences of form that have largely depended on the study of the dead: dead bodies, dead tissues, and dead cells. Each science began with observers isolating, identifying, and naming the external and internal structures of living things, first with the naked eye and then with microscopes. For some investigators, the primary goal has been classification, arranging the bewildering array of plants, insects, fish, birds, and animals into groups and subgroups based on the shapes and arrangements of their parts. For most, however, understanding structure was, and is, inextricably connected to understanding function and development. The configuration of parts, from lungs and stomachs to neurons and cell membranes, provides vital clues to the ways that individual organisms replicate and nourish themselves and how populations of similar creatures emerged and died out over time. Studying the internal parts of living things often requires researchers to make dynamic systems into static objects, to stop change in order to grasp it. Over the last two centuries, the closer that curious investigators tried to get to life's processes, the more they had to inspect and analyze sequences of dead specimens. The techniques and technologies they devised to see and map biological structures provided the tools for discoveries and theories in physiology, embryology, microbiology, biochemistry, and genetics.



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In the nineteenth and twentieth centuries, the biological sciences emerged when studies of living things moved into universities, research institutes, and particularly into laboratories. The traditional medical sciences of early modern universities, notably anatomy, the materia medica, and the "institutes of medicine," which included physiology, became academic subjects in reformed departments of anatomy, physiology, pharmacology, and pathology. At the same time, areas once unified under the umbrella of natural history found new homes in departments of zoology, botany, geology, and anthropology created in the faculties of the liberal arts (and sciences), outside of the faculties of medicine. The details of institutional organization varied considerably among European, British, American, and colonial universities, but the main thrust was to push a wide range of subjects into formal academic disciplines, each with its own scholarly societies, professional meetings, journals, and acceptable research protocols. During this ongoing restructuring, anatomy, histology, and cytology developed as clusters of theoretical orientations and research methodologies, not as well-defined fields with stable boundaries.[1]

This chapter focuses on the scientific study of form at three structural levels. "Anatomy" encompasses the charting and naming of structures at the macroscopic level, all that can be seen by unaided vision, with the intent to construct a definition of the parts of "normal" bodies.[2] "Gross anatomy" now typically refers to the study of human anatomy, but investigators since antiquity have used the basic methods of gross dissection to investigate a wide range of living creatures, especially those with domestic value, such as horses, or novelty to Euro-Americans, such as kangaroos.[3] Comparative anatomy, the study of structures across diverse species, provided one of the foundations for the emergence of modern biology from early modern natural history and, as such, spurred the development of theories of evolution and mathematical systematics.

"Histology" covers the study of tissue structure and organization. Tissues are clearly perceptible at the gross level, as bone obviously differs from muscle, and muscle differs from skin. For centuries, philosophers and anatomists

[1] Lynn K. Nyhart, *Biology Takes Form: Animal Morphology and the German Universities, 1800–1900* (Chicago: University of Chicago Press, 1995). Nyhart has superbly laid out the importance of going beyond disciplinary labels to understand the interactions of philosophical ideas, institutional politics, specific research programs, and intellectual contexts in the emergence of modern biology. Also see Andrew Cunningham, "The Pen and the Sword: Recovering the Disciplinary Identity of Physiology and Anatomy before 1800. I: Old Physiology – the Pen," *Studies in the History and Philosophy of Biology and Biomedical Sciences*, 33 (2002), 631–55, for a nuanced discussion of the change from eighteenth-century anatomy and physiology to the experimental physiology of the nineteenth century.

[2] K. D. Roberts and J. D. W. Tomlinson, *The Fabric of the Body: European Traditions of Anatomical Illustration* (Oxford: Clarendon Press, 1992), provides a good survey of major texts in the history of human anatomy.

[3] Carolo Runi, *Dell'Anatomia et dell'Infirmita del Cavallo* [*On the Anatomy and Diseases of the Horse*] (Bologna, 1598); Harriet Ritvo, *The Platypus and the Mermaid, and Other Figments of the Classifying Imagination* (Cambridge, Mass.: Harvard University Press, 1997), pp. 1–84.

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acknowledged these "similar" or "consimilar" parts in discussions of human anatomy, but commentary on them was largely descriptive and philosophical. Between 1800 and 1802, Xavier Bichat (1771–1802) put forward the idea that tissues are fundamental elements of physiology, with each tissue (he counted 21) having a distinct function.[4] For Bichat and his followers, tissues became the organizing principles of a new, physiologically active "general anatomy," and the foundations for a new pathological anatomy of disease and dysfunction. "Cytology," the inquiry into the structure of cells, also emerged in the early decades of the nineteenth century, although Robert Hooke (1635–1703) had first named a microscopic "cell" in 1665. The articulation of the cell theory in the nineteenth century is one of the key elements of modern biology. Considerable debate over the physiological primacy of cells, the development of multicelled organisms from single-celled beginnings, and the significance of structures seen within cells energized researchers well into the twentieth century. Among late nineteenth-century biologists, cytology was folded into the study of all living forms, from protozoa to mammals, as one aspect of the more inclusive "cell biology." Within twentieth-century medicine, in contrast, "cytology" has come to refer more narrowly to the use of cells scraped from tissues or aspirated in fluids to diagnose pathological conditions in humans and animals and will not be addressed in this chapter.[5]

## ANATOMY: HUMANS AND ANIMALS

The history of anatomy has two main subsets: human anatomy and comparative anatomy, or the anatomies of all nonhuman macroscopic creatures. Both of these areas have long histories in the West, extending well back into Greek culture, and thus had significant classical and early modern philosophical orientations at the start of the nineteenth century. Arguments based on teleology and divine design dominated most of the overarching explanations for anatomical forms, especially in mainstream works. William Paley's *Natural Theology; or, Evidences of the Existence and Attributes of the Deity, Collected From the Appearances of Nature* (London, 1802) was but one of the popular publications that disseminated a comfortable message of God's morphological order at the turn of the nineteenth century. In this order, God had designed all the parts of living beings for specific purposes, so examining structures revealed this design and the purpose (*telos*). Humans were at once

[4] John M. Forrester, "The Homoeomerous Parts and Their Replacement by Bichat's Tissues," *Medical History*, 38 (1994), 444–58.

[5] See, for example, Michael Cohen et al., "Classics in Cytology II: The Diagnosis of Cancer of the Uterine Cervix in Smears," *Acta Cytologica*, 31 (1987), 642–3; Neil Theise and Michael Cohen, "Classics in Cytology III: On the Puncture of the Liver with Diagnostic Purpose," *Acta Cytologica*, 33 (1989), 934–5; Stephen R. Long and Michael Cohen, "Classics in Cytology IV: Traut and the 'Pap Smear,'" *Acta Cytologica*, 35 (1991), 140–2.

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part of nature, embodying the most perfect version of God's mammalian template and distinct from it, having been the only creatures endowed with a soul.[6] Less theologically oriented but still idealist philosophies of purpose-driven progress in nature remained important in shaping causal explanations for morphological development in both embryos and species throughout the nineteenth century.[7] Charles Darwin's theory of evolution by natural selection, in contrast, saw form as the contingent outcome of the changing relationships that living things had with their environment. Researchers in the late nineteenth century turned away from anatomy as conceptually interesting, although it remained a significant tool in the study of living things.

Methodologically, human and animal anatomy centered on dissection and the preservation of large specimens. In the first decades of the nineteenth century, air-tight submersion in jars of alcohol was the main way to save parts that could not be dried.[8] Anatomists injected vessels with various fluids, such as mercury or heated wax, in order to trace fine branches during dissection; after dissection, if a particularly good wax cast remained after all the tissue was removed, it was saved to use in teaching. After mid-century, the search for other techniques led to innovations, such as slicing entire frozen bodies in order to study the transverse relationships of structures, and to new preservatives. Formaldehyde, discovered in 1859, became inexpensive enough to use to disinfect and fix large parts in the late nineteenth century.[9] Twentieth-century technologies used in conjunction with dissection included the gamut of radiographic imaging devices (x-ray, CT, and MRI) and, most recently, the introduction of plastination for keeping human and animal parts free from decay and deterioration.

## HUMAN ANATOMY

By the beginning of the nineteenth century, work on human anatomy was largely the province of university medical faculties, independent medical schools, and medical corporations such as the Royal College of Surgeons of London. The intellectual shift toward the anatomical localization of internal diseases, and the increasing sophistication of surgical techniques, reinforced anatomy's primacy as a core science for well-educated medical practitioners. Medical faculties and schools could monopolize the study of normal human anatomy after 1800 because they took on the problems and responsibilities

[6] William Coleman, *Biology in the Nineteenth Century: Problems of Form, Function and Transformation* (Cambridge: Cambridge University Press, 1977), pp. 58–61.

[7] Nyhart, *Biology Takes Form*, pp. 6–12, 112–21.

[8] F. J. Cole, *A History of Comparative Anatomy from Aristotle to the Eighteenth Century* (New York: Dover, 1975), pp. 445–50.

[9] Nikolai Pirogov, *Anatomia topographica sectionibus per corpus humanum congelatum triplici directione ductis illustrate*, 5 vols. (St. Petersburg: J. Trey, 1852–9); G. H. Parker and R. Floyd, "Formaldehyde, Formaline, Formol, and Formalose," *Anatomischer Anzeiger*, *Series 3*, 1 (1895–6), 469.

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of providing access to human dissection for teaching and research. In major European cities, such as Paris and Vienna, authorities in the eighteenth century had allowed the unclaimed bodies of those who died in certain public hospitals to be used for student dissection, along with those made available to universities and corporations after state executions. Elsewhere, most subjects for students to work on came from grave robbing and body snatching. The early to mid-nineteenth century saw the widespread adoption of laws that permitted instructors to use the bodies of the unclaimed poor for medical teaching. The most well-studied instance of such legislation, the British Anatomy Act of 1832, became the template for similar legislation in the British dominions and in the United States.[10] Although anatomists at various medical schools still complained about the supply of cadavers, it seems that none had serious shortages again until well after World War II. The reasons for this are complex, but the rise of the welfare state in various forms in Western countries reduced the numbers of those who had to be buried at state expense as paupers. The body donation movement, which began in the mid-1960s in the United States, arose as medical schools solicited such anatomical "gifts" and supported legislation enacted to cover both organ donation for therapeutic ends and deeded bodies for research and teaching.[11]

In 1800, there were few serious research frontiers left in macroscopic human anatomy. Much of the work in gross anatomy in the nineteenth century led to textbooks and atlases containing more detail, not new discoveries of macroscopic parts per se. The major exceptions to this generalization for the next two centuries were biomechanics and physical anthropology. A handful of nineteenth-century anatomists studied the physical properties of human biological structures, such as characteristics of the vascular system that maintained fluid circulation under cardiac pressure and the biophysics of muscles and joints that allowed certain movements; the latter area developed into the sciences of kinesiology and biomechanical engineering in the twentieth century.[12]

Physical anthropology grew out of research on human variations. Anatomists had been attuned to the variability of human bodies for centuries and had sought ways to construct a single template for an ideal (for idealists) or typical (for empiricists) human structure out of diverse observations. At the same time, they tried to distinguish the distorted, or pathological,

[10] Ruth Richardson, *Death, Dissection and the Destitute*, 2nd ed. (Chicago: University of Chicago Press, 2001); Michael Sappol, *A Traffic of Dead Bodies* (Princeton, N.J.: Princeton University Press, 2002); Susan C. Lawrence, "Beyond the Grave – The Use and Meaning of Human Body Parts: A Historical Introduction," in *Stored Tissue Samples: Ethical, Legal, and Public Policy Implications*, ed. Robert Weir (Iowa City: University of Iowa Press, 1998), pp. 111–42.

[11] Susan C. Lawrence and Kim Lake, "Selling a Noble End: The Twentieth Century Rise in Body Donation" (unpublished manuscript).

[12] Nyhart, *Biology Takes Form*, pp. 81–4. See also, for example, Arthur Steindler, *Mechanics of Normal and Pathological Locomotion in Man* (Springfield, Ill.: Charles C. Thomas, 1935).

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from the properly formed, as well as to characterize the peculiarities of female and infant anatomies compared with those of adult males. In the mid-eighteenth century, moreover, European anatomists turned their attention to the anatomical features of other races. Morphological studies of racial "types" contributed significantly to scientific racism in the nineteenth and early twentieth centuries, especially when eugenicists linked anatomical features, such as cranial size, to progressive evolutionary development.[13]

More methodologically sophisticated analyses of variations in human bones emerged in the late nineteenth and twentieth centuries in conjunction with scrutiny of prehistoric grave sites and the search for fossil evidence of primate and human evolution. In 1891, for example, Eugene Dubois (1858–1940), who had studied medicine at the University of Amsterdam and worked briefly as a lecturer on anatomy, discovered part of a skull, a femur, and two teeth in Java, which he announced to be evidence of an apelike man who walked upright; he named the new species *Pithecanthropus erectus* (later *Homo erectus*).[14] Dubois returned to Europe and became a professor of paleontology at the University of Amsterdam in 1899, a step that illustrates how physical anthropology became institutionalized. In the late 1920s and 1930s, statistical study of bone variations led Wilton M. Krogman (1903–1987), a physical anthropologist working at Case Western Reserve University and the University of Chicago, to produce *A Guide to the Identification of Human Skeletal Material* for the U.S. Federal Bureau of Investigation in 1939. This manual for determining the probable race, gender, and age of unidentified human remains spurred further research on gross human morphology for forensic as well as anthropological purposes.[15]

## COMPARATIVE ANATOMY

Work on the structures of living things other than humans was interwoven with a wide range of subjects in natural history, philosophy, and theology before the nineteenth century. By the late 1700s, much ink had flowed about

[13] John P. Jackson, Jr., and Nadine M. Weidman, *Race, Racism, and Science: Social Impact and Interaction* (Santa Barbara, Calif.: ABC-CLIO, 2004); George W. Stocking, ed., *Bones, Bodies, Behavior: Essays on Biological Anthropology* (Madison: University of Wisconsin Press, 1988); Nancy Stepan, *The Idea of Race in Science: Great Britain, 1800–1960* (London: Macmillan, 1982). For anatomical variation, see Ronald A. Bergman, Adel K. Afifi, and Ryosuke Miyauchi, *Illustrated Encyclopedia of Human Anatomic Variation* [electronic resource] (Iowa City: University of Iowa, 2000–4), at http://www.vh.org/Providers/Textbooks/AnatomicVariants/AnatomyHP.html.

[14] John Daintith and Derek Gjertsen "Dubois, Marie Eugène François Thomas," in *A Dictionary of Scientists* (Oxford: Oxford University Press, 1999) through Oxford Reference Online (accessed June 15, 2004). Peter J. Bowler, *Theories of Human Evolution: A Century of Debate, 1844–1944* (Baltimore: Johns Hopkins University Press, 1986), pp. 34–5, discusses the controversy surrounding Dubois's claims.

[15] William A. Haviland, "Wilton M. Krogman (1903–1987)," *National Academy of Sciences Biographical Memoirs*, 63 (1994), 292–307.

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the proper arrangement of living forms into groups that reflected a unifying plan for natural diversity. The idea that humans were the pinnacle of creation, moreover, had long led philosophers to try to arrange living things into a hierarchical sequence from the "lowest" forms of life, simple plants, to the "highest" primates. The multitude of names given to various plants and animals over time did not make the task of organizing the natural world any easier. In the mid-eighteenth century, Carl Linnaeus (1707–1778) systematically applied binomial identification, using a single genus and species name, to organisms. His *Species Plantarum* (*Species of Plants*) of 1753 and *Systema Naturae* (*System of Nature*) of 1758 established a formula for biological nomenclature that most naturalists subsequently adopted. (National rivalries and priority disputes, however, stirred passions over the naming of species well into the twentieth century. It took until 1930, for instance, for botanists from the United States, England, and Germany to finally agree that if a plant had appeared in Linnaeus's 1753 *Species Plantarum*, then the name that he gave was the official one.[16]) Comparative anatomy was the key method underlying taxonomy (the science of classification), and the more that eighteenth-century naturalists explored and compared anatomical details across different creatures, the harder it became to discern a unifying plan for all living things, much less a strictly hierarchical one.[17]

In the first decades of the nineteenth century, Georges Cuvier (1769–1832) promoted significant shifts in orientation for comparative anatomy. Cuvier spent most of his career associated with the Musée National d'Histoire Naturelle in Paris, one of the preeminent institutions for the collection and study of specimens of European and colonial fauna. First, Cuvier abandoned a single hierarchical vision for animal life and introduced instead four distinct body forms: the Vertebrata (vertebrates, animals with a backbone); Mollusca (soft-bodied animals, such as squids); Articulata (segmented invertebrates, such as worms and insects); and Radiata (radially symmetric organisms, such as starfish and jellyfish). The members of each of these groups had their own hierarchical arrangement from simple to more complex. By overturning the obsession with a single linear scale of being, Cuvier removed a philosophical constraint and inspired others to join in rethinking the principles of classification. Second, Cuvier insisted that extinct forms be included in taxonomies. Spurred by geologists' work on stratification and fossil forms, Cuvier demonstrated that fossils really were the remains of species that had died out. He compared the fossil bones of elephant-like animals found in Europe and Siberia to the bones of current Indian and African elephants, for example, and demonstrated that the "mammoth" was a long-dead species.

[16] Ronald H. Petersen, *A Guide to Botanical Nomenclature* [electronic resource] (Knoxville: University of Tennessee), at http://fp.bio.utk.edu/mycology/Nomenclature/nom-intro.htm; International Commission on Zoological Nomenclature, *International Rules of Zoological Nomenclature* (Washington, D.C.: International Commission on Zoological Nomenclature 1926), introduction.

[17] Ritvo, *Platypus and the Mermaid, and Other Figments of the Classifying Imagination*, pp. 19–34.

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Cuvier did not believe that species naturally changed over time, however, and was confident that geologists would eventually explain the events that had led to mass extinctions. Finally, Cuvier resolutely maintained that function, not form alone, had to direct comparative anatomists' interpretations of relationships among species. For Cuvier, living creatures were integrated wholes. Their parts worked together, with every part coordinated with every other part. Change one feature and others would have to be different. The same function, moreover, could be carried out by different arrangements of structures, while superficially similar parts could have quite different purposes. Cuvier used this insight to reconstruct animals from incomplete fossil remains, as well as to promote comparative anatomy as a theoretically sophisticated research method.[18]

Other comparative anatomists adopted, extended, and debated Cuvier's work. Richard Owen (1804–1892), curator of the Hunterian collection at the Royal College of Surgeons of England and then superintendent of the natural history department of the British Museum, and Louis Agassiz (1807–1873), founder of the Museum of Comparative Zoology at Harvard (1859), both added considerably to the development of comparative anatomy based on meticulous dissection and analysis of form across many species. Collections of specimens, and their representation in illustrated publications, flourished, stimulating both academic and amateur passions for finding, describing, and naming species, from fossil corals and exotic insects to reptiles and birds, especially in regions new to Euro-American scrutiny. While theorists debated taxonomic principles, many contributors focused on descriptive morphology, producing works that added to the weight of available information about the diversity of living forms.[19]

At mid-century, two concerns decisively pushed static animal anatomy into a secondary, supportive role within the emerging biological sciences. Embryology and Darwinian evolution shifted fundamental questions about form from understanding the overall design of nature's plan(s) to the processes of development itself, for the individual and for species. Embryologists still had to detail the changing forms through which minute specks passed into adult shapes, but how and why change occurred increasingly became the

[18] Coleman, *Biology in the Nineteenth Century*, pp. 18–21, 63–4; Toby A. Appel, *The Cuvier-Geoffroy Debate: French Biology in the Decades before Darwin* (New York: Oxford University Press, 1987); Georges Cuvier, *Le règne animal distribué d'après son organisation, pour servir de base à l'histoire naturelle des animaux et d'introduction à l'anatomie comparée* [*The Animal Kingdom, Arranged According to Its Organization, Serving as a Foundation for the Natural History of Animals, and an Introduction to Comparative Anatomy*], 1st ed. (Paris, 1817).

[19] David E. Allen, *The Naturalist in Britain: A Social History* (Princeton, N.J.: Princeton University Press, 1994); Richard Owen, *The Hunterian Lectures in Comparative Anatomy, May and June 1837*, ed. Philip R. Sloan (Chicago: University of Chicago Press, 1992); Mary P. Winsor, *Reading the Shape of Nature: Comparative Zoology at the Agassiz Museum* (Chicago: University of Chicago Press, 1991). For examples of this genre, see John O. Westwood, *Arcana Entomologica; or, Illustrations of New, Rare, and Interesting Insects* (London: W. Smith, 1845); John O. Westwood, *Catalogue of the Genera and Subgenera of Birds Contained in the British Museum* (London: The Trustees of the British Museum, 1855).

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important research questions.[20] Charles Darwin's (1809–1882) *On the Origin of Species by Means of Natural Selection: Or, the Preservation of Favored Races in the Struggle for Life* (1859) made form, and changing forms over time, highly contingent on a species's interaction with its environment. Most significantly, Darwin's theory laid out a new explanatory relationship for creatures with similar structures: They were related by descent from common ancestors, not by variations on nature's plans for life's diversity.[21]

Anatomy's important, but nearly invisible, role in the twentieth-century biological sciences is best conveyed by two examples. First, although finding and describing new species remains a vital task for field zoologists, most funding and attention goes to laboratory-based research. Starting in the late nineteenth century, scientists particularly detailed the macroscopic structures of the animals used for laboratory experiments. Among these, Thomas Hunt Morgan's (1866–1945) choice of the (pseudo-) fruit fly (*Drosophila melanogaster*) for his work in genetics has made this insect's anatomical variations (both "natural" and induced in the laboratory) among the most studied in the world. With the complete mapping of the fly's genome in 2000, researchers are seeking a one-to-one correspondence between DNA sequences, protein expressions, embryological development, and adult structures.[22] Similarly, the choice of the common gray house mouse (*Mus musculus*) as a laboratory object led to the development of white mouse strains whose anatomical features are similarly well known and increasingly correlated with specific genetic code. The successful expression of transgenic DNA (genetic material from one species inserted into the eggs, sperm, or embryo of another) is often determined by morphological as well as physiological changes in the adult, thus underscoring anatomy's place as an experimental tool.[23]

Second, although the general acceptance of evolution by natural selection implied that scientists should be able to determine a "natural" taxonomy based on lines of descent and divergence from common ancestors, that was an elusive goal in practice. Taxonomists had to rely on how they interpreted the extent of shared structures among diverse species and, in the early to mid-twentieth century, acknowledged that identification, naming, and grouping were primarily based on conventions within areas of expertise rather than on much empirical data on genetic relationships. To replace this unsatisfactory philosophical and methodological basis for taxonomy, a number of

20 Coleman, *Biology in the Nineteenth Century*, pp. 35–56; Nyhart, *Biology Takes Form*, pp. 95–6, 151–3, 245–51, 263–74, 280–98; Henry Harris, *The Birth of the Cell* (New Haven, Conn.: Yale University Press, 1999), pp. 117–37.

21 Nyhart, *Biology Takes Form*, chaps. 4–6; Jane Maienschein, *Transforming Traditions in American Biology, 1880–1915* (Baltimore: Johns Hopkins University Press, 1991), pp. 105–14; Yvette Conry, *L'introduction du darwinisme en France au Xe siècle* (Paris: J. Vrin, 1974).

22 Robert Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994); E. W. Myers et al., "A Whole-Genome Assembly of *Drosophila*," *Science*, 287 (2000), 2196–204.

23 Karen A. Rader, *Making Mice: Standardizing Animals for American Biomedical Research, 1900–1955* (Princeton, N.J.: Princeton University Press, 2004); Matthew H. Kaufman and Jonathan Bard, *The Anatomical Basis of Mouse Development* (San Diego, Calif.: Academic Press, 1999).

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biologists proposed to use mathematical analysis of discrete characteristics to determine statistical measures of evolutionary "closeness" among species. Two works stand out as inaugurating this complex field: *Phylogenetic Systematics* (1966) by Willi Hennig (1913–1976), first published in German in 1950, and *The Principles of Numerical Taxonomy* (1963) by Robert Sokal and Peter Sneath. Since the 1970s, the application of mathematical modeling and data processing have expanded the tools used to understand and arrange macroscopic biological structures, just as such late twentieth-century approaches have provided ways to deal with the levels of information associated with molecular biology. Whether the new systematics can produce a convincing "natural" taxonomy of living forms remains a very open question.[24]

## TISSUES AND CELLS

Tissues and cells quite literally leapt into focus with the development of the microscope. The turning point for the use of the microscope as a definitive research tool came with Joseph Jackson Lister's 1826 invention of an objective lens that significantly reduced both chromatic and spherical aberration. This technology did not in itself create the concepts of tissues and cells, but the way that Lister's lenses reduced the optical problems with earlier lenses helped to cut through the arguments that had raged about what observers actually saw using older devices. The rings, blurry spots, penumbras, and colors that frequently appeared when seventeenth- and eighteenth-century instrument makers tried to increase magnification, excluding exceptional grinders and observers such as Antony van Leeuwenhoek (1632–1723), had encouraged a number of investigators, including Xavier Bichat (1771–1802), to dismiss the device as useless. By the mid-1830s, however, instrument makers across Europe had mastered and begun to improve Lister's microscope, seeking ways to enhance magnification, mount specimens, and direct light onto or through the optical field. Having sharper fields of focus for magnifications higher than 200× (up to approximately 450× to 500× by the 1850s and to 2500× by 1880) reinvigorated interest in the microscopic anatomy of plants, animals, and tiny individual organisms. Although observers reached a consensus on some claims about microscopic structures, new debates regularly emerged over what could be seen and, if what was seen were "real" forms and not artifacts or illusions, what they all meant.[25]

[24] Joseph Felsenstein, "The Troubled Growth of Statistical Phylogenetics," *Systematic Biology*, 50 (2001), 465–7; Robin Craw, "Margins of Cladistics: Identity, Difference and Place in the Emergence of Phylogenetic Systematics, 1864–1975," in *Trees of Life: Essays in Philosophy of Biology*, ed. Paul Griffiths (Dordrecht: Kluwer, 1992), pp. 64–82.

[25] Harris, *Birth of the Cell*, pp. 15–32; John V. Pickstone, "Globules and Coagula: Concepts of Tissue Formation in the Early Nineteenth Century," *Journal of the History of Medicine*, 23 (1973), 336–56; Brian Bracegirdle, "J. J. Lister and the Establishment of Histology," *Medical History*, 21 (1977), 187–91; L. Stephen Jacyna, "Moral Fibre: The Negotiation of Microscopic Facts in Victorian Britain," *Journal of the History of Biology*, 36 (2003), 39–85.

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By the mid-nineteenth century, tissues and cells had become foundational concepts for understanding both the structures and functions of complex multicellular life. Bichat had laid out a vision of tissues as the basic functional units of anatomy in his three works, *A Treatise on the Membranes* (1800), *Physiological Researches on Life and Death* (1800), and *General Anatomy* (1802), and these inspired others to think in terms of a general physiological anatomy in which the functions of organs and systems (such as the vascular and nervous systems) resulted from the functions carried out in living tissues. As Bichat focused on human anatomy and was especially interested in seeing tissues as the locus for macroscopic pathological changes in human diseases, it was not at all clear if his generalization could extend to quite different forms of life, such as plants and insects, or what sort of distinct physiological properties inhered in his twenty-one separate kinds of living substances. Studies using microscopes revealed that Bichat's tissues were made up of cells and other structures and that some kinds of cells appeared in more than one tissue. Tissues held promise for human anatomy and physiology, but another sweeping generalization soon arrived to derail the idea that they were the fundamental units of life.

## THE CELL THEORY

Matthias Schleiden (1804–1881), a botanist, and Theodor Schwann (1810–1882), an anatomist-physiologist, have been credited with articulating the first unified cell theory, in 1839. They were not the only ones in the 1830s leaping from partial observations to broad generalizations about the significance of cells, but they arguably were the boldest.[26] In a paper published in 1838, Schleiden proposed, with a mix of observation and speculation, that the elementary living components of all plant tissues were cells. Schwann, considering Schleiden's claim for plants, looked again at specimens from animal bodies and, seeing nuclei in many cellular structures within animal tissues, extended Schleiden's generalization to all animal life. The statement that the cell is the fundamental unit of all living things, both plants and animals, had enormous appeal as an overarching theory because it defined a unifying principle at a time when both anatomists and philosophers were struggling to bring order to nature. "The" cell, as Schleiden and Schwann defined it, had a set of primary characteristics: a nucleus containing a nucleolus, an inner medium (protoplasm, later called cytoplasm), and an outer boundary (a wall or a membrane). Within tissues, structures that were *not* cells, such as the matrix of solid-looking parts in bones, were produced by cells, and the extracellular fluids carried the elements and compounds that cells needed. Schwann coined the term "metabolic" to describe all of the chemical changes that took place in (and perhaps around) the cell that made it a unit of life, even though

[26] Harris, *Birth of the Cell*, pp. xi–xii, 64–75, 82–93.

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most of the specific processes were unknown. Both Schleiden and Schwann also emphasized the importance of tracing embryological development as a further way to link life's structures and functions to their cellular origins.[27]

Schleiden and Schwann's publications inspired both further research and passionate criticism, leading to intense focus on a number of issues. Among these, the problem of how cells formed during embryological development and how growth occurred particularly taxed embryologists and physiologists. Schleiden and Schwann suggested that cells multiplied in at least two ways. Cells generated within cells, forming around one or more daughter nucleoli, which then separated. Cells also emerged from the extracellular fluids in a process that Schleiden described as analogous to the way that crystals form in a saturated solution. A tiny coalescence in the rich materials surrounding cells created a nucleolus, which then attracted the other components of cellular substance and, when enough had merged, a boundary formed around a nucleus. The nucleus then generated a vesicle that eventually enclosed it, becoming the new cell. Neither Schleiden nor Schwann provided much convincing evidence to support these far-reaching propositions.[28]

By the late 1840s, observations by Franz Unger (1800–1870) and other botanists threw considerable doubt on Schleiden's view that cells could form out of extracellular material in plants. They simply could not see any intermediary forms for that process, but they could see cells in some stages of division. Robert Remak (1815–1865), having closely observed a number of specimens, including the division of embryonic red cells in developing chicks, also denied extracellular origins for cells and argued that all animal cells reproduced by division. Rudolf Virchow (1821–1902), a prominent pathological anatomist, came up with the most sweeping generalization, in his work *Cellular Pathology* (first edition 1858), when he declared "omnis cellula e cellula" ("all cells from cells"), a Latin phrase first used by the French physician François-Vincent Raspail (1794–1878).[29] Virchow actually enunciated this overarching principle in the context of his work on human tissues and their pathological changes, however, not from extensive examination of diverse life forms. For Virchow, the major point was that disease resulted from disturbances in the functions and structures of normal cells and tissues; when cells faltered and failed, or reproduced defective copies of themselves, sickness ensued.[30] Cells were the units not only of life but also of death.

[27] Lois M. Magner, *A History of the Life Sciences*, 2nd ed. (New York: Marcel Dekker, 1994), pp. 192–201; Theodor Schwann, *Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants* (1839); Theodor Schwann and Matthias Schleiden, "Contributions to Phytogenesis" (1838), trans. Henry Smith (London: Sydenham Society, 1847).

[28] Magner, *History of the Life Sciences*, pp. 196–200; Harris, *Birth of the Cell*, pp. 97–116. See especially Marsha Richmond, "T. H. Huxley's Criticism of German Cell Theory: An Epigenetic and Physiological Interpretation of Cell Structure," *Journal of the History of Biology*, 33 (2000), 247–89.

[29] Harris, *Birth of the Cell*, pp. 31–3, 106–16, 128–36.

[30] Harold M. Malkin, "Rudolf Virchow and the Durability of Cellular Pathology," *Perspectives in Biology and Medicine*, 33 (1990), 431–9.

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That "omnis cellula e cellula" applied to all living organisms was more of a challenge for further research than a conclusion drawn from a solid range of evidence. This principle also turned attention to the next set of puzzles. If cells reproduced by division, how did that occur? And how, in that process, did they replicate their forms and functions? For those attentive to embryology, deciding that the changing forms taken on by a fertilized egg (particularly observed in species of birds, frogs, and fish, whose eggs were visible and easily controlled) were the results of cell division clarified some of the steps of early development, but figuring out how these cells differentiated into tissues was a daunting prospect. To approach these questions, investigators had to observe a wide variety of cells passing through all of the stages of emergence and reproduction. The more that researchers wanted to see, however, the more they had to devise consistent techniques that would make microscopic structures visible.

Underlying the history of histology and cytology from the 1840s to the present is a history of laboratory instruments, reagents, and protocols, as well as of funding, staffing, and administration.[31] To see much detail in tissues, for example, especially fragile animal tissues that decay rapidly, requires that specimens be fixed and cut into extremely thin slices. Soft tissues needed to be hardened, and even hardened tissues needed to be held in a matrix, such as wax, to preserve the specimen's borders. Researchers, sometimes on their own but usually with skilled instrument makers, developed some microtomes in the 1840s to 1860s. These improved significantly in the late 1870s and after, as industries invested in the research needed to create the precision machinery required for mass manufacturing.[32] Thin slicing was not enough, however, as investigators also discovered, because the thinner the sections are, the fainter the natural colors of tissue and cell structures become. The solution, first developed largely by serendipity and unsystematic trial and error, was to immerse the specimen in chemicals that stained microscopic structures. Some coloring of substances for microscopic inspection had been done in the eighteenth and early nineteenth centuries, but enthusiasm for finding chemicals and methods took off in the 1850s. In 1858, for instance, Joseph von Gerlach (1820–1886) discovered that a solution of carmine (a red coloring agent made from the bodies of the insect *Dactylopius coccus*) stained the nuclei of nerve cells in hardened brain tissue, which opened up work on the microanatomy of the nervous system as well as the visual enhancement of nuclei in other tissues. Aniline dyes, compounds derived from coal tar in the

[31] See Adele E. Clarke and Joan H. Fujimura, "What Tools? Which Jobs? Why Right?," Frederic L. Holmes, "Manometers, Tissue Slices and Intermediary Metabolism," and Patricia P. Gossel, "A Need for Standard Methods: The Case of American Bacteriology," in *The Right Tools for the Job*, ed. Adele E. Clarke and Joan H. Fujimura (Princeton, N.J.: Princeton University Press, 1992), pp. 3–44, 151–71, 287–311, respectively.

[32] Brian Bracegirdle, *A History of Microtechnique* (Ithaca, N.Y.: Cornell University Press, 1978), pp. 111–288; Nyhart, *Biology Takes Form*, pp. 201–4; Nathan Rosenberg, "Technological Change in the Machine Tool Industry, 1840–1910," *Journal of Economic History*, 23 (1963), 420, 426, 429–32.

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1850s to 1880s, spurred both the development of industrial chemistry and the regular application of new chemicals to tissue and cell specimens to see what might appear. The passion for sectioning and staining in the late 1870s led Ernst Haeckel (1834–1919), a prominent comparative anatomist, to fear that young scientists "will only know *cross sections* and *colored* tissues, but neither the *entire* animal nor its mode of life!"[33]

Staining rendered previously vague nuclei into clear structures and so enabled more forms to be identified as cells. More significantly, a number of observers started to follow the stained material through stages of cell division. One of the troublesome problems for cell theorists who emphasized the vital presence of a nucleus for creating new cells was that in many cases it seemed to disappear when cells divided. With better fixatives and stains, researchers such as Eduard Strasburger (1844–1912), Eduard Balbiani (1823–1899), Walther Flemming (1843–1905), and Heinrich Waldeyer (1836–1921) determined that when the nucleus seemed to dissolve, the stained rods, or threads, that it had contained seemed to line up and then separate into two clumps. Waldeyer in 1888 named the colored shapes "chromosomes," a term that replaced the variety of names given to the color-stained nuclear material by various authors. Researchers detailed two kinds of cell division. One (mitosis) led to duplicate cells, the other (meiosis) to reproductive cells, eggs, and sperm. In 1892, August Weismann (1834–1914) published *The Germ-Plasm: A Theory of Heredity*, which synthesized two decades of work on cell division and offered the third major component to nineteenth-century cell theory. Cells were the fundamental units of life, all cells derived from other cells, and the nucleus carried the material basis of inheritance.[34]

Even as researchers from Remak to Weismann pondered how cells reproduced in the context of embryogenesis and tissue formation, others turned to the investigation of minute, cell-like organisms, whose independent life had so surprised early microscopists. To what had been observed in the seventeenth and eighteenth centuries, nineteenth-century studies added thousands of new creatures. Linnaeus had placed all such tiny beings into the class "Chaos" within the category of "Vermes" (worms), but that did not satisfy taxonomists for very long. Certain kinds of microscopic life acquired a great deal more significance by the mid-nineteenth century, moreover, as investigators, Theodor Schwann and Louis Pasteur (1822–1895) among them, determined that these tiny forms participated in processes with direct human

[33] Quoted in Nyhart, *Biology Takes Form*, p. 203 (emphasis in original); Bracegirdle, *History of Microtechnique*, pp. 65–82; Pio Del Rio-Hortega, "Art and Artifice in the Science of Histology" (trans. William C. Gibson from a 1933 paper), *Histopathology*, 22 (1993), 515–25.

[34] Harris, *Birth of the Cell*, pp. 138–48, 153–70; Rasmus G. Winther, "August Weismann on Germ-Plasm Variation," *Journal of the History of Biology*, 34 (2001), 517–55. For a more complex analysis of the meaning of chromosomes for cell theory, see Marsha L. Richmond, "British Cell Theory on the Eve of Genetics," *Endeavour*, 25 (2001), 55–60; Jean-Pierre Gourret, "Modelling the Mitotic Apparatus: From the Discovery of the Bipolar Spindle to Modern Concepts," *Acta Biotheoretica*, 43 (1995), 127–42.

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interest, such as the fermentation of alcohol (yeast) and putrefaction (bacteria). The role of bacteria in plant, animal, and human diseases inspired even more scrutiny and stimulated the emergence of bacteriology and, by the early twentieth century, microbiology, as new disciplines of specialized research and teaching.[35] The study of microorganisms intersected repeatedly with the study of tissues and cells as both concepts and techniques developed in nineteenth-century laboratories. Quite a number of single-celled, or unicellular, organisms lacked nuclei, for example, which complicated the elegance of the cell theory. The characteristics of this group, the bacteria, challenged a number of generalizations about cell structure and function well into the twentieth century. In 1937, Herbert Copeland (taking up an idea first suggested by Ernst Haeckel in 1866) proposed that the bacteria should be taxonomically separated into their own kingdom, one at the same level as plants and animals. In the 1970s, some biologists divided all living things into two major groups (super kingdoms), the prokaryotes (cells with no nucleus) and eukaryotes (cells with nuclei, including the protists, plants, and animals), in part because the morphology of these basic units confounded a single unifying definition of "cell."[36]

## HISTOLOGY

While the emerging cell theory dominated theoretical discussions about the fundamental units of life, researchers also struggled to understand how cells and their surrounding media made up quite different kinds of tissues. Anatomists in medical schools especially turned toward the study of tissues as the components of human organ systems. Histology opened up new fields of research for anatomists at a time when research became increasingly important for individual and institutional prestige, and so microscopic anatomy generally entered the medical curriculum under the purview of traditional anatomy departments. A number of mid-century contributions mark the way that those who focused on tissues struggled to provide both a comprehensive descriptive account of tissue structures and a theoretical foundation for tissue organization based on embryological development. Rudolf Albert von Kölliker (1817–1905) published his *Handbuch der Gewebelehre des Menschen* (*Textbook of Human Histology*) in 1852, and it was soon one of the definitive guides to descriptive human histology. In a series of publications in the 1840s to mid-1850s, Robert Remak (1815–1865) proposed that the three different cell layers that emerged in the vertebrate embryo (the ectoderm, mesoderm, and endoderm) each produced different tissues. This was quite

[35] William Bulloch, *The History of Bacteriology* (London: Oxford University Press, 1938), remains a useful, if dated, survey.

[36] Jan Sapp, "The Prokaryote–Eukaryote Dichotomy: Meanings and Mythology," *Microbiology and Molecular Biology Reviews*, 69 (2005), 292–305.

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an appealing theory for the tidy mapping of tissues onto germ layers, as the basic embryonic layers were called. A direct correlation between tissues and germ layers was extraordinarily difficult to establish, however, and at some point in the late nineteenth or early twentieth century, Remak's hypothesis had to be quietly abandoned. In 1857, Franz von Leydig (1821–1908) produced his *Lehrbuch der Histologie des Menschen und der Tiere* (Textbook of Human and Animal Histology), which laid out a broad comparative view of tissues across species. Leydig, one of Kolliker's students, was probably sympathetic to the germ-layer theory, but he rested his classification of tissues on fundamental similarities of structure and function. He proposed the four basic types still used in medical histology: epithelial tissue, connective tissue, muscular tissue, and nervous tissue. Each of these has a number of subtypes that cover Bichat's original twenty-one tissues and more.[37] As slicing and staining technologies improved after mid-century, researchers published increasing amounts of detail about tissue structure, organization, development, and deterioration across vertebrate and invertebrate species, continuing to seek connections with embryological structures and hoping to find traces of evolutionary change in the tissues that formed complex organ systems.[38]

Of all the tissues that engaged histologists and physiologists in the nineteenth and twentieth centuries, those of the nervous system were among the most intriguing. Since antiquity, philosophers and physicians had theorized about how information could travel seemingly instantaneously from one part of a body to another. Herophilus (ca. 330–260 B.C.E.) had identified macroscopic nerves as the primary conduits of sensation and motion and, by the early nineteenth century, anatomists had traced in considerable detail the distribution of nerves and their connections to the spinal cord and brain in humans and a number of other species. In the mid-nineteenth century, methods for hardening brain tissue and staining the nuclei of nerve cells launched a promising wave of research into the microscopic morphology of the nervous system. While physiologists turned to experiments on animals to try to localize functions within the brain, to distinguish somatic (voluntary motor and sensory) nerves from autonomic (involuntary motion, visceral sensation) nerves, and to understand reflex actions, microscopists searched for the structures that made such an array of functions possible.[39]

The disagreement that arose between two major researchers in the latter decades of the nineteenth century aptly illustrates how a staining technique

[37] Nyhart, *Biology Takes Form*, pp. 85–7, 121–2, 128; Coleman, *Biology in the Nineteenth Century*, pp. 43–7; Magner, *History of the Life Sciences*, p. 211. Later research revealed that both epithelial and connective tissues arise from more than one of Remak's germ layers. See, for example, Thomas W. Sadler, *Langman's Medical Embryology*, 8th ed. (Philadelphia: Lippincott Williams and Wilkins, 2000), pp. 88, 97, 102.

[38] Nyhart, *Biology Takes Form*, pp. 175–206.

[39] Erwin H. Ackerknecht, "The History of the Discovery of the Vegetative (Autonomic) Nervous System," *Medical History*, 18 (1974), 1–8.

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could spur alternative interpretations as it made new structures in tissues visible. The two main actors were the Italian, Camillo Golgi (1843–1926), and the Spaniard, Santiago Ramón y Cajal (1852–1934), who shared the Nobel Prize in Medicine or Physiology in 1906 for their work on the nervous system. In the early 1870s, Golgi developed the "black reaction," a way of staining nerve cells that revealed not only the cell's complex of relatively short dendrite branches but also its axon, which can also have branches at its tip. He demonstrated that the axon was clearly part of the cell itself. Working primarily on human brain tissue, Golgi argued that his work supported the theory that nerve fibers, the dendrites and axons, formed a dense network with each other, intersecting at multiple points and reducing the significance of any particular nerve cell. For Golgi, the complex, integrated functions of the central nervous system required a tissue structure that allowed parts of it to act in unison; his view was more holistic than reductionist.[40]

In contrast, Cajal, who took up and enhanced Golgi's stain, generally used the brains of small, young birds and mammals in which the delicate dendrites and axons of individual nerve cells could be traced from one cell to another. He rejected Golgi's network theory in favor of a theory of sequential pathways, where the axon of one nerve cell connected to a specific dendrite or body of another single nerve cell. Cajal's demonstration that what appeared to be a tangle of dendrites and axons could be resolved into elegant communicating chains convinced leading European histologists. Waldeyer summarized Cajal's and others' work in a powerful 1891 review, enunciating what has since been known as the "neuron doctrine": The fundamental structural and physiological units of the nervous system are individual neurons [his name for the specialized, information-processing nerve cells] and their distinct connections to each other throughout nervous tissue. How collections of relatively independent individual cells could provide a satisfactory material base for involuntary and voluntary functions, much less for consciousness, had to remain an open question.[41]

Waldeyer's decisive support for Cajal's work seems to be another example of the way in which the effective preparation and staining of microscopic specimens resolved morphological questions in histology. Not all contemporaries were convinced, however, especially those involved in trying to determine how nerve tissue emerged from embryological origins and developed in the maturing animal. The "black reaction" stain, for instance, was known to color only some neurons, not others, and did not uniformly reveal all of a single neuron's processes. Moreover, it was impossible to track how an

[40] Edward G. Jones, "Golgi, Cajal and the Neuron Doctrine," *Journal of the History of the Neurosciences*, 8 (1999), 170–8; Ennio Pannese, "The Golgi Stain: Invention, Diffusion and Impact on Neurosciences," *Journal of the History of the Neurosciences*, 8 (1999), 132–140.

[41] Jones, "Golgi, Cajal and the Neuron Doctrine," 170–8. For more detail, see Gordon M. Shepherd, *Foundations of the Neuron Doctrine* (New York: Oxford University Press, 1991).

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individual nerve cell developed because the laboratory investigator could never see exactly the same piece of tissue at two different points in time. Faced with this interesting problem, Ross Harrison (1870–1959), working at Yale after having studied extensively in Germany, decided to try a new technique. Between 1907 and 1910, he applied the methods that bacteriologists had developed to grow bacteria cells in cultures to the idea of growing tissue cells out of the body. After tinkering for awhile, he placed a tiny specimen of neurogenic tissue from tadpole spinal cord in a drop of frog lymph clinging to a slide cover slip. With the specimen properly sealed, to keep it free of contamination, and carefully incubated, he could actually watch the development of nerve dendrites and axons under a microscope. His account of the outward movement of the cytoplasm in axons growing out from the neuron's cell body strengthened consensus around the neuron doctrine and so settled the interpretation of static histological specimens.[42]

Harrison was not interested in extending this remarkable new laboratory procedure in other directions, but his work inspired Alexis Carrel (1873–1944) and his coworker Montrose Burrows, among a number of others, to culture a range of other animal and human tissues, including cancer cells, in the 1910s to late 1930s. Several of Carrel's boldest claims, such as the possibility of creating "immortal" lines of normal mammalian and human cells, raised expectations for immediate breakthroughs, and disappointments frustrated researchers into the early 1950s. Tissue cultures nevertheless opened new directions for histologists working on the development, physiology, and biochemistry of tissues, and such research areas exploded in the second half of the twentieth century.[43]

## ULTRASTRUCTURE

As the resolution of optical microscopes increased at the end of the nineteenth century, cytologists and histologists argued over the existence of structures other than the nucleus within cells. From at least the 1860s, various theorists and observers claimed that the cytoplasm had to have a complex structure or structures to carry out all the functions necessary for cell life. Some described an internal mesh of lines and fluids; others remarked on various tiny spots, granules, or vesicles where some vital function could be located. In 1898, Golgi published a paper detailing a reticular, or netlike, structure within nerve cells that the "black reaction" stain had made visible. In response,

[42] Hannah Landecker, "New Times for Biology: Nerve Cultures and the Advent of Cellular Life in Vitro," *Studies in the History and Philosophy of the Biological and Biomedical Sciences*, 33 (2002), 667–94. For earlier efforts to cultivate tissues, see Lewis Phillip Rubin, "Leo Loeb's Role in the Development of Tissue Culture," *Clio Medica*, 12 (1977), 33–66.

[43] Jan A. Witkowski, "Alexis Carrel and the Mysticism of Tissue Culture," *Medical History*, 23 (1979), 279–96; Jan A. Witkowski, "Dr. Carrel's Immortal Cells," *Medical History*, 24 (1980), 129–42.

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critics claimed that such ephemeral forms were artifacts produced by fixing, staining, sectioning, or the wishful thinking of microscopists.[44]

In the first decades of the twentieth century, much attention focused on the nucleus, chromosomes, and the morphological basis for heredity, as well as on the refinement of biochemical methods for identifying the complex compounds and reactions involved in cell and tissue metabolism. Researchers in quite different fields in the mid- to late 1930s developed two new instruments that would fundamentally reshape modern biology after World War II interrupted so many lives and plans: the high-speed centrifuge and the electron microscope. The ultracentrifuge took a solution of mashed-up cells and spun it so fast that the parts it contained were distributed by very tiny differences in weight. This method, called tissue fractionation, collected all the similar parts of all of the cells together at various layers. The faster the centrifuge, the more discrimination appears among different cell parts, which biochemists then analyze to determine what sort of substances (such as nucleic acids, proteins, enzymes, sugars, and lipids) appear together.[45] The electron microscope, which used beams of electrons rather than light to make images, allowed vastly smaller structures to be resolved for study. It took several years for investigators to work out how to prepare and section biological specimens before a consensus developed once again that the resultant images captured real forms and not artifacts.[46] Both the ultracentrifuge and the electron microscope spurred hundreds of separate studies, but the explosion of results in cell and tissue biology occurred when the biochemists and the microscopists got together.

Starting in the mid-1950s, the electron microscope revealed even to the most skeptical that the cytoplasm of eukaryotic cells did indeed have component structures, collectively called "organelles." In addition to the nucleus, the organelles include the structures that Golgi identified, which bear his name as "Golgi bodies," as well as the endoplasmic reticulum, mitochondria, lysosomes, and perioxisomes. To these structures biochemists have attached functions revealed by their work on tissue fractionations, hence locating energy production in the mitochondria and protein production in the sections of the endoplasmic reticulum studded with RNA molecules. It is in the studies of "ultrastructure" that form and function merge at the molecular level within cells. Although the story of the nucleus, chromosomes, and the structure of DNA is by far the most well-known instance of the confluence of subcellular parts, molecular forms, and biological functions in post–World

[44] Marina Bentivoglio and Paolo Mazzarello, "The Pathway to the Cell and Its Organelles: One Hundred Years of the Golgi Apparatus," *Endeavour*, 22 (1998), 101–5.

[45] Christian de Duve, "Tissue Fractionation: Past and Present," *Journal of Cell Biology*, 50 (1970), 20D–55D; Christian de Duve and Henri Beaufay, "A Short History of Tissue Fractionation," *Journal of Cell Biology*, 91 (1981), 293s–99s.

[46] Daniel C. Pease and Keith R. Porter, "Electron Microscopy and Ultramicrotomy," *Journal of Cell Biology*, 91 (1981), 287s–92s; Peter Sair, "Keith R. Porter and the First Electron Micrograph of a Cell," *Endeavour*, 21 (1997), 169–71.

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War II science, molecular biology encompasses the full range of questions pondered by anatomists, histologists, and cytologists as each new level of structures appeared accessible to human inquiry.[47]

## CONCLUSION

In many respects, descriptive anatomy, histology, and cytology are sciences of the past. Researchers will undoubtedly fill in many details on the morphology of bodies, tissues, and cells, but the frontiers lie in sophisticated mathematical systematics, ultrastructure, biochemistry, and molecular biology. Historians have barely begun to address how disputes over form, such as the definition of organelles or key characteristics of cell membranes, intersected with debates about functions, including the tendency of biochemists to downplay morphologists' desire for precise localization. Development, genetics, and DNA will continue to absorb much scholarly attention, given their importance in driving research agendas, the transformation of species in laboratories, and issues of personal and political identities. The breakdown of form and function in cells, tissues, and bodies in disease, aging, and death have also absorbed scientists in recent decades, underpinning debates in areas from pharmaceutical innovation to environmental degradation. Historical investigation into these areas, along with ongoing questions about the roles of technology, disciplinary specialization, and philosophical orientations in shaping research trajectories, will undoubtedly help us reassess the ways in which the search for comprehensible forms, whether of animals or molecules, have helped us to grasp stable moments in the face of biological change.

[47] Nicolas Rasmussen, "Mitochondrial Structure and Cell Biology in the 1950s," *Journal of the History of Biology*, 28 (1995), 385–6; Michael Morange, *A History of Molecular Biology*, trans. Matthew Cobb (Cambridge, Mass.: Harvard University Press, 1998).

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# 16

# EMBRYOLOGY

*Nick Hopwood*

"If . . . we say that each human individual develops from an egg, the only answer, even of most so-called educated men, will be an incredulous smile; if we show them the series of embryonic forms developed from this human egg, their doubt will, as a rule, change into disgust. Few . . . have any suspicion," wrote evangelist of evolution Ernst Haeckel in the 1870s, "that these human embryos conceal a greater wealth of important truths, and form a more abundant source of knowledge than is afforded by the whole mass of most other sciences and of all so-called 'revelations.'"[1] Between this extravagant claim and the incredulity and disgust that it invokes lies a contradictory history. In nineteenth-century universities and medical schools embryology was a key science of life; around 1900 modern biology was forged within it; and as developmental biology it buzzes with excitement today. Embryology fired wide publics with Darwinist fervor, sexual knowledge, and the prospect of reproductive control; but it also bored generations of medical students, was molecular biologists' favorite example of scientific decline, and has attracted both feminist and antiabortionist critiques. There are, then, rich histories to be told, and as scholars in various disciplines begin to tell them, existing surveys have come to seem thin. Largely confined to concepts and theories, they tell us little about the daily life of embryology. Written within particular traditions, they do scant justice to the diversity of embryo science and the variety of perspectives on it. In response to these limitations, this highly selective chapter seeks to encourage more adequate attempts at synthesis by following two interlocking transformations: in forms of work and in identity.

Embryology has shared with other sciences two main ways of working: Since the end of the eighteenth century, physicians, professors, and curators,

[1] Ernst Haeckel, *The Evolution of Man: A Popular Exposition of the Principal Points of Human Ontogeny and Phylogeny*, 2 vols. (London: Kegan Paul, 1879), vol. 1, p. xix.

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interested in identifying and classifying, analyzed compound objects into elements; and from the mid-nineteenth century, university researchers claimed experiment as the means to control life.[2] Embryological analysis dealt specifically with development; from collected specimens embryologists derived representations to compare and select, arrange into developmental series and display. Making series of lithographs, wax models, or sonograms may be said to have "produced" development for wide audiences and constructed objects on which to do more work.[3] In the nineteenth century, analysis dealt with germ layers and cells; in the twentieth, it increasingly involved chemicals and macromolecules as well. Early experiments were subordinate to analysis, but in the 1880s some embryologists followed physiology in elevating experimental control above supposedly mere description. Analysis continued, however, and it continued to interact with experiments revealing the potentials of embryonic parts and defining interacting systems. In the mid-twentieth century, biochemical and genetic searches for the molecular agents of these effects intensified. By 2000, deep and subtle interventions went beyond the laboratory, as embryology offered medicine and agriculture to make organisms to order.

Innovations in embryological work have driven and been driven by changes in identity. In the 1960s, "developmental biology" succeeded "experimental embryology" as accounting for most embryological research. The dominant historiography accordingly moves from the "classical descriptive embryology" of the first three quarters of the nineteenth century, through the "classical experimental embryology" that flourished between the 1880s and the 1930s, to its currently prominent successor. It is well known that because embryology rarely acquired its own institutes and professors, most embryologists made their livings wearing physiological, anatomical, zoological, or biological hats. But by taking parts for the whole, historians have seriously underestimated the variety this produced. Treating twentieth-century embryology exclusively as a branch of experimental biology is particularly problematic: The first specifically embryological research institution was founded, during World War I, to describe human embryos; most embryology books were medical texts; and by the 1990s many embryologists worked in fertility clinics. We can best explore the range of embryologies by encompassing a wide spectrum of scientific, technical, and medical activities. More than this, we should begin to look beyond the laboratories and clinics to the encounters of the science with sometimes radically different lay views of generation. For, as this chapter can only hint, it is in the contrasts between the perspectives of professionals

[2] John V. Pickstone, "Museological Science? The Place of the Analytical/Comparative in Nineteenth-Century Science, Technology and Medicine," *History of Science*, 32 (1994), 111–38. This usage is close to the opposite of the common identification of embryological analysis with experiment.

[3] Nick Hopwood, "Producing Development: The Anatomy of Human Embryos and the Norms of Wilhelm His," *Bulletin of the History of Medicine*, 74 (2000), 29–79.

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and laypeople – and not just Haeckel's "educated men" – that we shall find the more general significances of embryological work.

## MAKING EMBRYOLOGY

In the 1930s, embryologist-historian Joseph Needham nominated a Hippocratic writer as "the first embryologist" and traced a straight line through Aristotle, William Harvey, and Karl Ernst von Baer (1792–1876) to the premier embryological journal of his own day.[4] Yet even before Needham, embryologists had treated the decades around 1800 as a break in the history of their science when strange debates over generation gave way to a much more familiar world. Historians who trace our natural sciences to the Age of Revolutions find in the late Enlightenment the making not merely of modern embryology but of embryology itself. The science emerged, however, from both new ways of analyzing embryos and the selective restructuring of earlier investigations into generation.

The mechanistic natural philosophers of the late seventeenth century sought to understand the perpetuation of visible order, but the origin of organized beings – How does a soft, fluid hen's egg become a highly ordered chicken? – remained the subject of endless debate. Epigenesis, the ancient view that organization arose progressively from initially unorganized matter, was tainted with atheist materialism. Its preformationist rival taught that, all appearances to the contrary, adult structures were already present in the egg – or, some said, in the "animalcules" of the male semen – waiting to unfold. This positing of a passive nature determined by divine laws was the orthodox account. But though by no means as ridiculous as embryologists would later suppose, preformation gave the epigenesists plenty to mock. If an omnipotent and benevolent God had made all germs at the Creation, why were there ugly and useless monsters? And what about the sensational discovery in the 1740s that a polyp could regenerate from its parts?[5]

These problems contributed during the second half of the eighteenth century to the triumph of epigenesis over preformation. But the shift was more profound than a simple victory for the materialist theories that had attempted to explain the source of embryonic organization. Earlier naturalists had represented external surfaces; anatomists now dissected organisms to reveal the

[4] Joseph Needham with Arthur Hughes, *A History of Embryology*, 2nd ed. (Cambridge: Cambridge University Press, 1959), pp. 31, 36.

[5] Shirley A. Roe, *Matter, Life, and Generation: Eighteenth-Century Embryology and the Haller-Wolff Debate* (Cambridge: Cambridge University Press, 1981); Shirley A. Roe, "The Life Sciences," in *The Cambridge History of Science*, vol. 4: *Eighteenth-Century Science*, ed. Roy Porter (Cambridge: Cambridge University Press, 2003), pp. 397–416; Emma C. Spary, "Political, Natural and Bodily Economies," in *Cultures of Natural History*, ed. Nicholas Jardine, James A. Secord, and Emma C. Spary (Cambridge: Cambridge University Press, 1996), pp. 178–96.

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inner structural relations and functional activities of the parts. Because organization defined what it meant to be living, its origin no longer needed to be explained, and comparative anatomists concentrated on seeking the laws relating the various plans.[6] The St. Petersburg academician Caspar Friedrich Wolff and the Göttingen professor Johann Friedrich Blumenbach stabilized epigenesis by turning the once most troublesome beings into instruments of their science. Monstrosities, they argued, were produced when the generative force of a newly active nature deviated into deficiency or excess. Seen in the light of a process of development, monsters even became beautiful.[7]

Embryology was created not only from the philosophy of generation and the natural history of monsters but also by male surgeons moving into midwifery and by enlightened medico-legal interest in the unborn child as a future citizen. At first, the stages of pregnancy were determined within a preformationist framework, and specimens that did not fit the ideal of human proportions were rejected as false conceptions. Whereas even seventeenth-century drawings of chick embryos resemble those produced two hundred years later, pictures and models of human embryos continued to show the increase in size of a miniature child. Then, in 1799, the anatomist Samuel Thomas Soemmerring, extending and revising anatomies of the gravid uterus, had his artist create a space in which human embryos could be seen progressively to change shape (Figure 16.1).[8]

The determinacy of this medical image of development contrasted starkly with, and was used to devalue, women's bodily experiences of the precariousness of pregnancy. This usually took nine months, but occasionally seven or eleven. Several missed periods might mean a child, but could equally signal illness or a false conception – until the only sure sign of pregnancy, "quickening," when a woman felt a child move inside her. In practice, this was taken to correspond to the moment at which, in the long-standing Christian–Aristotelian view, the fetus became animate or ensouled. Abortion was generally tolerated before this point, but the Sicilian Jesuit Francesco Emanuele Cangiamila rejected abortion totally, and his *Sacred Embryology* (1745) was unusually obsessed with baptizing even the earliest embryos. In 1803, abortion

[6] Roe, *Matter, Life, and Generation*, pp. 148–56; Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences* (London: Tavistock Press, 1970); François Jacob, *The Logic of Life: A History of Heredity*, trans. Betty E. Spillmann (New York: Pantheon, 1982), pp. 74–129.

[7] Georges Canguilhem, "La Monstruosité et le Monstrueux," in *La Connaissance de la Vie*, 2nd ed. (Paris: J. Vrin, 1989), pp. 171–84, at pp. 178–9; Michael Hagner, "Enlightened Monsters," in *The Sciences in Enlightened Europe*, ed. William Clark, Jan Golinski, and Simon Schaffer (Chicago: University of Chicago Press, 1999), pp. 175–217. See also Armand Marie Leroi, *Mutants: On the Form, Varieties and Errors of the Human Body* (London: HarperCollins, 2003).

[8] Chapters by Barbara Duden, Nadia Maria Filippini, and Ulrike Enke, in *Geschichte des Ungeborenen: Zur Erfahrungs- und Wissenschaftsgeschichte der Schwangerschaft, 17.–20. Jahrhundert*, ed. Barbara Duden, Jürgen Schlumbohm, and Patrice Veit, Veröffentlichungen des Max-Planck-Instituts für Geschichte, vol. 170 (Göttingen: Vandenhoeck und Ruprecht, 2002). See also Janina Wellmann, "Wie das Formlose Formen schafft. Bilder in der Haller-Wolff-Debatte und die Anfänge der Embryologie um 1800," *Bildwelten des Wissens*, 1, pt. 2 (2003), 105–15.

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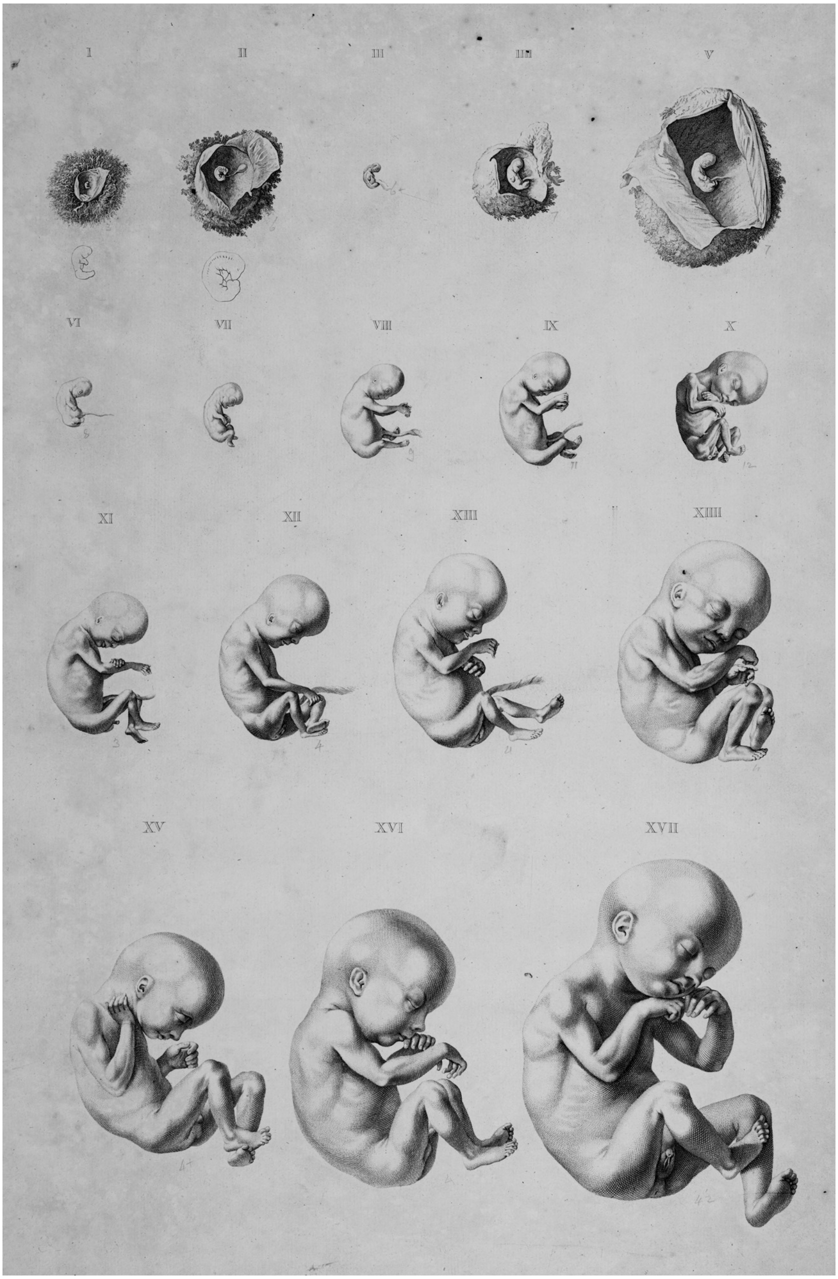


Figure 16.1. Human embryos developing through the first four months of pregnancy. Engraving, after drawings by Christian Koeck, from Samuel Thomas Soemmerring, *Icones embryonum humanorum* (Frankfurt am Main: Varrentrapp und Wenner, 1799), plate I, by permission of the Syndics of Cambridge University Library.

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was made a statutory crime in English law, with a lesser penalty if performed before quickening; medical men used embryology to discredit this official recognition of pregnant women's privileged knowledge. More generally, the learned defined the limits of the science by excluding as vulgar superstitions attitudes they had once shared. These included the doctrine that terrible experiences during pregnancy could produce monsters and the conviction that a woman's pleasure during intercourse was a precondition for procreation. Twentieth-century embryologists were nevertheless still confronting similar views.[9]

As a static world gave way to a dynamically changing and historical one, patterns of development came to be seen as the fundamental relation between the different plans of animal organization. Form was to be understood by following its formation, and – especially in the German heartlands of Romanticism – embryology was morphology's guide.[10] Epigenesis offered the Romantics a congenial image of their own becoming: A preformationist fetus, a merely mechanical product of its mother's body that simply inherited its father's power, had been destined for an arranged marriage; the Romantic fetus made itself and would grow up to marry for love.[11] In a universe no longer a machine but a huge animal, generation was the fundamental metaphor and the development of an embryo the model for a nature pregnant with series after ascending series of forms.

The series of adult animals was understood in terms of the parallel development of an individual human being. According to the doctrine of recapitulation, in the course of their development the higher animals passed, in essence, through the adult forms of the lower and a human fetus through the whole animal kingdom. The lower animals, wrote the Romantic nature philosopher Lorenz Oken around 1810, were conversely a series of human abortions. In the systematizations of Halle anatomist Johann Friedrich Meckel the Younger and the Parisians Etienne and Isidore Geoffroy Saint-Hilaire and Etienne Serres, recapitulation gave general significance to a special science of malformations, teratology. But, from the new Muséum d'Histoire Naturelle in Paris, the comparative anatomist Georges Cuvier powerfully opposed this

9 Angus McLaren, *Reproductive Rituals: The Perception of Fertility in England from the Sixteenth Century to the Nineteenth Century* (London: Methuen, 1984); Angus McLaren, "Policing Pregnancies: Changes in Nineteenth-Century Criminal and Canon Law," in *The Human Embryo: Aristotle and the Arabic and European Traditions*, ed. G. R. Dunstan (Exeter: University of Exeter Press, 1990), pp. 187–207. See also Thomas Laqueur, *Making Sex: Body and Gender from the Greeks to Freud* (Cambridge, Mass.: Harvard University Press, 1990), pp. 149–92.

10 E. S. Russell, *Form and Function: A Contribution to the History of Animal Morphology* (London: John Murray, 1916); Owsei Temkin, "German Concepts of Ontogeny and History around 1800," *Bulletin of the History of Medicine*, 24 (1950), 227–46; Timothy Lenoir, *The Strategy of Life: Teleology and Mechanics in Nineteenth-Century German Biology* (Chicago: University of Chicago Press, 1989).

11 Helmut Müller-Sievers, *Self-Generation: Biology, Philosophy, and Literature around 1800* (Stanford, Calif.: Stanford University Press, 1997).

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transcendental anatomy by breaking the animal series into four distinct modes of organization.[12]

## HISTORIES OF DEVELOPMENT

In the universities of the post-Napoleonic German states, teachers of anatomy and physiology, sympathetic to Romantic nature philosophy but committed to empirical investigations, highlighted the embryological criterion as the key to a true classification. Anatomy without embryology risked artificiality, they taught, but observing developing embryos would show how, from an original internal unity, organisms generated parts in an order corresponding to the natural classification system. By tracing the development of basic structures, one could explain complex morphologies as the elaboration of simple types. Two medical students from the German-speaking Baltic, Christian Pander (1794–1865) and Karl Ernst von Baer, led a host of researchers in creating a new mode of analysis for the science. They showed how organization arose from the transformation of primitive "germ layers," and their followers resolved these into cells.[13]

In 1816, Professor Ignaz Döllinger of Würzburg suggested that his protégés reexamine the classic object of centuries of investigations into generation, the development of the chick in the egg. The noble but impoverished von Baer had to leave the expensive and time-consuming project to Pander, a wealthy banker's son. A custodian ran two incubators so that he could sacrifice several thousand eggs, opening them and probing the embryos with fine needles under a magnifying glass. Extending Wolff's work, Pander expressed his major result in a new vocabulary that replaced earlier circumlocutions: Development began not directly with organ formation but by the organization of sheets of tissue, the germ layers. Pander's greatest expense was paying for engravings that conveyed the complex changes in shape more vividly and in more detail than his words.[14]

Von Baer had followed with interest as Pander proceeded "to wind a laurel wreath of egg-shells around his forehead,"[15] and as soon as he obtained an

[12] Stephen Jay Gould, *Ontogeny and Phylogeny* (Cambridge, Mass.: Harvard University Press/Belknap Press, 1977), pp. 33–68; Toby A. Appel, *The Cuvier-Geoffroy Debate: French Biology in the Decades before Darwin* (New York: Oxford University Press, 1987).

[13] For a survey to about 1880, see Frederick B. Churchill, "The Rise of Classical Descriptive Embryology," in *A Conceptual History of Modern Embryology*, ed. Scott F. Gilbert (Baltimore: Johns Hopkins University Press, 1994), pp. 1–29.

[14] See, most recently, Stéphane Schmitt, "From Eggs to Fossils: Epigenesis and the Transformation of Species in Pander's Biology," *International Journal of Developmental Biology*, 49 (2005), 1–8.

[15] Von Baer to Woldemar von Ditmar, July 10, 1816, quoted in Boris Evgen'evič Raikov, *Karl Ernst von Baer, 1792–1876: Sein Leben und sein Werk*, trans. Heinrich von Knorre, Acta historica Leopoldina, vol. 5 (Leipzig: Barth, 1968), p. 91.

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academic position in Königsberg (now Kaliningrad), von Baer embarked on investigations to correct, extend, and generalize his friend's account. These led in 1828 to part of *Über Entwickelungsgeschichte der Thiere* (On the Developmental History of Animals). Like Cuvier, von Baer rejected the linear animal series in favor of four distinct types. During embryogeny, a primary germ common to the whole animal kingdom differentiated into one of four ideal "archetypes," which governed ever more specialized development. An embryo did not pass through the permanent forms of other animals but diverged from shared embryonic forms. In the 1830s, von Baer's work was used in Britain and France to destroy or dilute an often anti-Establishment recapitulationism. Gentlemen of science prayed that the branching view of the animal kingdom would undermine the monad-to-man progressivism of radical lecturers in the London medical schools. In most hands, however, "von Baer's law" did not drive out the "Meckel–Serres law" of parallelism but rather coexisted or was conflated with it.[16]

Von Baer's studies provided a model for a wealth of further research. Between the 1820s and the 1850s, anatomists and physiologists, many of them students of Berlin physiologist Johannes Müller, added cells to the germ layers as a second fundamental unit of embryological analysis and began intensive efforts with the new achromatic microscopes to establish the relations between them. Vesicles surrounding a nucleus were found first as a unifying feature of vertebrate eggs, notably the mammalian ovum, which von Baer, following development to its origins, discovered in 1827. The cell theory of the late 1830s arose from the attempt to generalize the development of these fundamental organs to later structures, and to unify development across the living world. In the 1840s, Robert Remak, an unbaptized Jew forced to work on the margins of the University of Berlin, argued that all cells arise from preexisting cells, from the egg, through the germ layers, to the tissues (Figure 16.2). His doctrine of germ-layer specificity, the most powerful generalization of nineteenth-century embryology, taught that in all vertebrates each layer – endoderm, mesoderm, and ectoderm – gives rise to particular cell types; for example, liver, muscle, and nerve. The argument was expanded as germ layers and cells were investigated in animals with diverse life cycles and modes of reproduction.[17]

None of these men was employed as an embryologist; the science never achieved independent status in the German universities, its most important institutions. In the decades after 1800, it had mainly been the province of

[16] Adrian Desmond, *The Politics of Evolution: Morphology, Medicine, and Reform in Radical London* (Chicago: University of Chicago Press, 1989).

[17] Edwin Clarke and L. S. Jacyna, *Nineteenth-Century Origins of Neuroscientific Concepts* (Berkeley: University of California Press, 1987), pp. 1–100; Jane M. Oppenheimer, "The Non-specificity of the Germ-Layers," in her *Essays in the History of Embryology and Biology* (Cambridge, Mass.: MIT Press, 1967), pp. 256–94; Henry Harris, *The Birth of the Cell* (New Haven, Conn.: Yale University Press, 1999).

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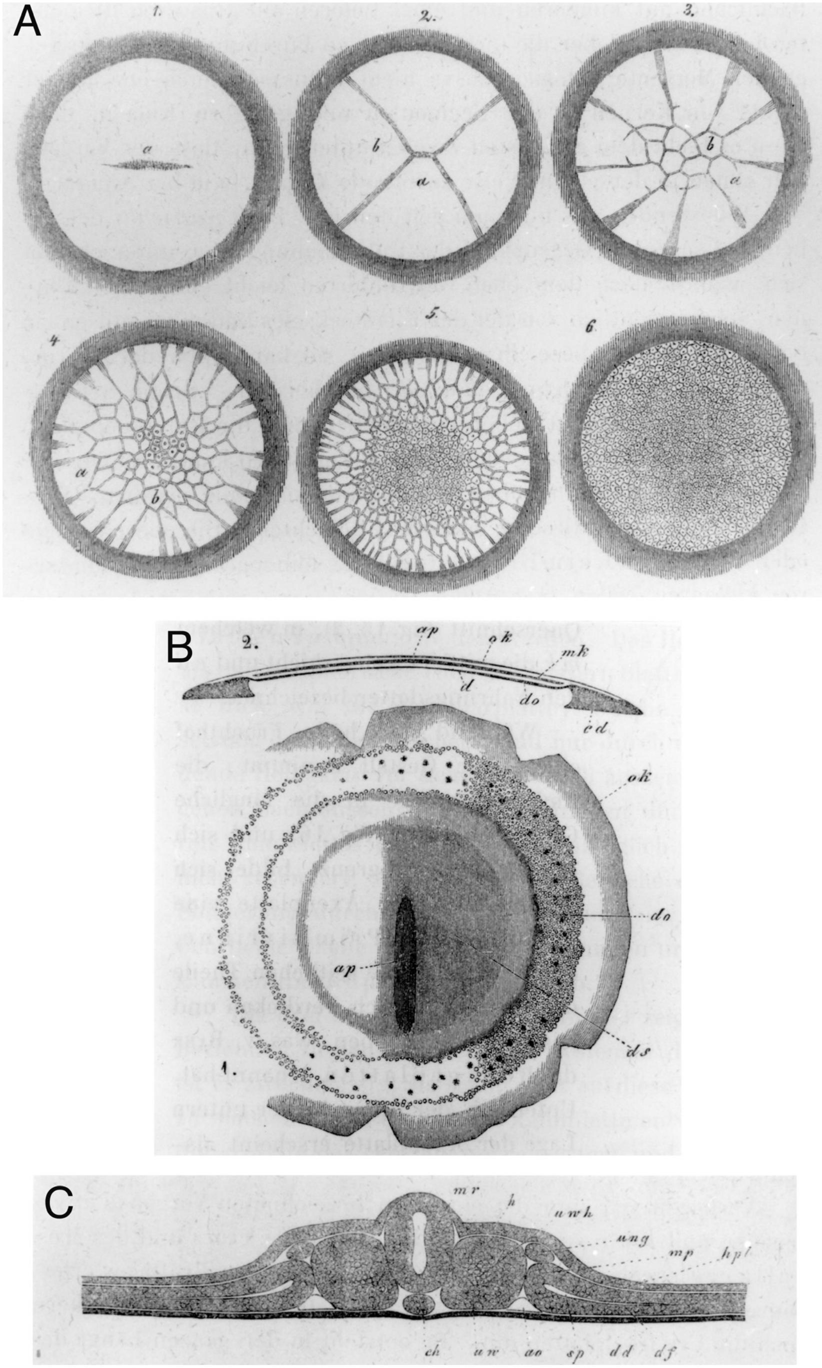


Figure 16.2. Cells and germ layers in chick development. (A) Earliest stages, when cleavage divides the egg's germinal layer into cells. (B) Germinal membrane early in incubation: (1) dorsal view and (2) cross section, showing three germ layers. (C) Cross section through second-day embryo, showing the typical vertebrate structure. Wood-engravings from Würzburg professor Albert Kölliker's successful textbook *Entwicklungsgeschichte des Menschen und der höheren Thiere* (Leipzig: Engelmann, 1861), pp. 41, 43, 48, by permission of the Syndics of Cambridge University Library.

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professors of anatomy and physiology, but around mid-century these chairs were divided, and physiology redefined as those topics amenable to experimental manipulation and physico-chemical analysis. Excluded as intractable, development was left to morphologists who were being pushed into independent anatomy institutes and new institutes of zoology. Henceforth embryology was largely split between professors of anatomy in the medical faculties, who tended to specialize in the vertebrates, and professors of zoology in the faculties of philosophy, who made the invertebrates their own.[18] In addition, human embryos were collected and studied in the obstetric clinics; and the discovery of the ovum helped the new discipline of gynecology organize itself around the ovary rather than the uterus.[19] Embryology would form part of fisheries biology, too. The embryology of plants was less clearly demarcated from the rest of botany than human and animal embryology were from general anatomy and zoology.

From the 1830s, enthusiastic teachers arranged special courses in embryology that became the backbone of the science and an important part of anatomical and zoological curricula; some embryology was taught also in obstetrics and gynecology, and to midwives and veterinarians. Medical lectures were oriented toward humans, but embryologists had only limited access to the bodies of pregnant women, and so they concentrated on the chick (Figure 16.2) and domestic mammals. Microscopy practicals were supposed to show students how to recognize in initially unprepossessing specimens the unfamiliar shapes through which bodies gained the structures that in other classes they dissected. Young men interested only in qualifying as doctors disliked these notoriously difficult courses, but embryology provoked great excitement and in the commercial anatomy museums laypeople paid to see preparations and models of the history of development.[20]

## EMBRYOS AS ANCESTORS

"Embryology rises greatly in interest, when we . . . look at the embryo as a picture, more or less obscured, of the common parent-form of each great class of animals," Charles Darwin argued in 1859.[21] Evolution made ideal archetypes into real ancestors, and embryonic resemblances into evidence of descent. Ironically, it was not von Baerian embryology that saw the greatest

[18] Lynn K. Nyhart, *Biology Takes Form: Animal Morphology and the German Universities, 1800–1900* (Chicago: University of Chicago Press, 1995), pp. 65–102.

[19] Claudia Honegger, *Die Ordnung der Geschlechter: Die Wissenschaften vom Menschen und das Weib, 1750–1850* (Frankfurt am Main: Campus, 1991), pp. 210–12.

[20] Nick Hopwood, *Embryos in Wax: Models from the Ziegler Studio, with a Reprint of "Embryological Wax Models" by Friedrich Ziegler* (Cambridge: Whipple Museum of the History of Science; Bern: Institute of the History of Medicine, 2002), pp. 12–13, 33–39.

[21] Quoted in Churchill, "Rise of Classical Descriptive Embryology," p. 18.

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rise in interest in the age of evolution but the recapitulationist version von Baer had attempted to refute. The extent to which Darwin himself held recapitulationist views is controversial.[22] But it was above all the Jena zoologist Ernst Haeckel (1834–1919) who taught that individuals repeat in the course of embryonic development the most important changes through which their adult ancestors passed during the evolutionary development of the species, or in the pithy formula of his "biogenetic law," "ontogeny recapitulates phylogeny." As a shortcut to otherwise poorly documented relationships, embryology enjoyed a heyday as a key to the history of life on earth and a matter of heated public debate.[23] (See the following chapters in this volume: Hodge, Chapter 14; Di Gregorio, Chapter 12.)

With a premium on investigating the development of a variety of animals, embryologists created new arrangements for collecting embryos that were distant from land-locked European laboratories. The priority was to exploit the sea, the "cradle of life" and home of the richest diversity of animal organization, bringing order to invertebrate embryology and attempting to establish the evolutionary origin of the vertebrates. In 1872, Haeckel's student Anton Dohrn founded the Naples Zoological Station, the most prestigious of a string of new marine laboratories. It played a crucial role as an international trading post for ideas, materials, and techniques.[24] In this age of empire, naturalists also took part in expanding Europe's biological dominion, making expeditions to observe embryos in the wild and bringing them home for collections.[25]

Embryologists revolutionized the analysis of the microscopic specimens they went to so much trouble to find. One line of work enlisted new fixatives and nuclear stains to elucidate the major events of fertilization and cell division. Another made a powerful tool for establishing phylogenies by generalizing the specificity of the germ layers from the vertebrates across the animal kingdom. Through the 1870s, embryologists increasingly used sectioning machines, or microtomes, to convert specimens into series of thin slices that showed more internal structure than a dissection.

Evolutionary hypotheses generally rested on, and inspired, laborious observations, but the bold deductions in Haeckel's semipopular works courted

[22] Robert J. Richards, *The Meaning of Evolution: The Morphological Construction and Ideological Reconstruction of Darwin's Theory* (Chicago: University of Chicago Press, 1992).

[23] Gould, *Ontogeny and Phylogeny*, pp. 69–114; Peter J. Bowler, *Life's Splendid Drama: Evolutionary Biology and the Reconstruction of Life's Ancestry, 1860–1940* (Chicago: University of Chicago Press, 1996).

[24] Christiane Groeben and Irmgard Müller, *The Naples Zoological Station at the Time of Anton Dohrn*, trans. Richard and Christl Ivell (Paris: Goethe-Institut, 1975).

[25] Roy MacLeod, "Embryology and Empire: The Balfour Students and the Quest for Intermediate Forms in the Laboratory of the Pacific," in *Darwin's Laboratory: Evolutionary Theory and Natural History in the Pacific*, ed. Roy MacLeod and Philip F. Rehbock (Honolulu: University of Hawaii Press, 1994), pp. 140–65; Rudolf A. Raff, *The Shape of Life: Genes, Development, and the Evolution of Animal Form* (Chicago: University of Chicago Press, 1996), pp. 1–4; Brian K. Hall, "John Samuel Budgett (1872–1904): In Pursuit of *Polypterus*," *BioScience*, 51 (2001), 399–407.

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controversy too. In passing from von Baer to Haeckel, we move from embryologists' patron saint to a man some treat as the evil genius of the science.[26] Although cemented by a revolt around 1900 against a style that to experimental biologists appeared long on speculation and short on substance, this mixed reputation went back to the 1870s. Courageously, cheered Haeckel's supporters, he not only opened up new topics for research but also made the biogenetic law the central principle of an evolutionary worldview. Outrageously, booed his opponents, he inflamed the general public with dogmatic answers to questions his scientific peers had yet to decide.

Within embryology, Haeckel's most effective adversary was the Basel, later Leipzig, anatomist Wilhelm His (1831–1904), who from the late 1860s combined promotion of the microtome and accurate reconstruction of wax models from the sections. He argued for a mechanical approach, joining the reductionist physiologists in claiming that no evolutionary series could explain development. His sought the mechanisms by which one stage transforms itself into the next, and found them in the bending and folding movements generated by unequal growth. For a long time, neither modeling nor this mechanical view of development caught on, but His's charge, that Haeckel's figures (Figure 16.3) tendentiously made the embryos look more similar than they really were, was very widely taken up.[27]

Evolutionary embryology was a much richer enterprise than one might suppose from the polemics directed against Haeckel, as the largest and softest target. Even his admirers, notably the British embryologists Francis Balfour and E. Ray Lankester, applied the biogenetic law more empirically and more flexibly. Comparative embryology also proved extraordinarily productive of lines of work that, like genetics, obscured their origins as they spun off. The notion of active host resistance to infection, for example, was developed out of the idea that phagocytosis characterizes the mesodermal lineage, which itself arose from a study of metazoan ancestry.[28]

It was mainly Haeckel, however, who took embryology out of medical courses, and the titillating world of the popular anatomy museum, and communicated it to the reading public. Recapitulationism was deployed in fields as diverse as anthropology, child study, and psychoanalysis. As European and North American men of science watched their own male offspring climb to the top of the evolutionary tree, it seemed as if everyone else – criminals, "primitives," and women – had arrested at some lower stage of

[26] For a psychopathologizing view, see Oppenheimer, *Essays in the History of Embryology and Biology*, pp. 150–4.

[27] Nick Hopwood, "'Giving Body' to Embryos: Modeling, Mechanism, and the Microtome in Late Nineteenth-Century Anatomy," *Isis*, 90 (1999), 462–96; Nick Hopwood, "Pictures of Evolution and Charges of Fraud: Ernst Haeckel's Embryological Illustrations," *Isis*, 97 (2006), 260–301.

[28] Alfred I. Tauber and Leon Chernyak, *Metchnikoff and the Origins of Immunology: From Metaphor to Theory* (Oxford: Oxford University Press, 1991).

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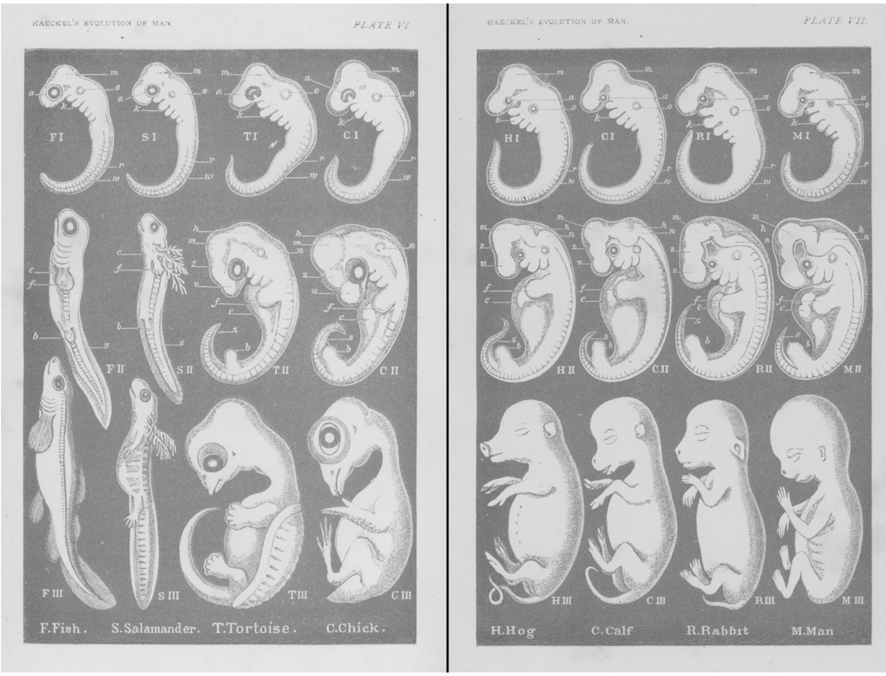


Figure 16.3. Embryology in the age of evolution. Controversial lithographs demonstrating to a wide public the agreement in the relations of form between human and other vertebrate embryos, more or less complete at very early stages (I) and retained longer in more nearly related animals (II, III). From Ernst Haeckel, *The Evolution of Man: A Popular Exposition of the Principal Points of Human Ontogeny and Phylogeny*, 2 vols. (London: Kegan Paul, 1879), vol. 1, plates VI, VII.

development.[29] But what happened to evolutionary embryology beyond the elite? No demonstration of evolution could match the vividness of embryos developing before one's very eyes. But who was encouraged to see the vertebrates' conquest of land in the development of frogspawn collected from a pond? Did Haeckel and his journalist allies persuade pregnant women that they carried first a fish, then a reptile, and only later a human being?

Haeckel reckoned it ridiculous to oppose aborting what he taught began as an animal. But physicians supported, and in the United States campaigned for, laws against abortion – unless performed by themselves. Alarmed at increasing medical intervention in pregnancy, in 1869 Pope Pius IX shifted the Catholic Church from the received distinction between the "inanimate" and the "animate" fetus to a hardline rejection of any abortion at all. Many women meanwhile still held to practices of menstrual regulation that were

[29] Gould, *Ontogeny and Phylogeny*, pp. 115–66. On uses of embryology in literature and art, see especially Evanghélia Stead, *Le monstre, le singe et le foetus: Tératogonie et Décadence dans l'Europe fin-de-siècle* (Geneva: Droz, 2004).

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justified by the notion that life was present only from quickening, little knowing or little caring that this had no more authority in an embryology that taught continuity of development than had the Catholic Church's embrace of ensoulment at conception.[30]

## EXPERIMENT AND DESCRIPTION

By the 1880s, academic embryology was in turmoil. The inability of teachers to agree, especially on the relative weighting of embryological and comparative anatomical evidence, turned influential students away from evolutionary morphology. They abandoned problems such as the origin of the vertebrates to focus on narrower questions, which they expected to answer using a more limited selection of materials, and many modeled their science on experimental physiology. Indeed, by opposing "experimental" to "descriptive" embryology, the more militant secured an identity as experimental biologists in a science they saw as overly descriptive and rife with unsupported speculation. In the 1970s and 1980s, historians of biology reinvestigated the changes in embryology between 1880 and World War I as exemplifying that wider transformation in the organization, problems, institutions, and methods of the life sciences by which biology as we know it was made. Experimental embryology and genetics were taken as model subdisciplines. Initial efforts to generalize tended to reinforce a one-dimensional view of a "revolt from morphology," but later studies worked to produce a more nuanced and inclusive history.[31] Yet the very agenda of searching for the origins of the new biology has underestimated continuities and excluded significant innovations in human and comparative embryological research.

The most successful iconoclast to emerge from the crisis of evolutionary morphology was Wilhelm Roux (1850–1924), an anatomist working in the 1880s at the University of Breslau (now Wrocław). Founding what he called *Entwicklungsmechanik* ("developmental mechanics"), he established himself over the next two decades as a tireless publicist for the new science and for Wilhelm Roux. By "mechanics" he did not mean the rather crude pressures

[30] McLaren, "Policing Pregnancies." For an opposing interpretation, see David Albert Jones, *The Soul of the Embryo: An Enquiry into the Status of the Human Embryo in the Christian Tradition* (London: Continuum, 2004). On the related debate over embryotomy versus Caesarean section, see Emmanuel Betta, *Animare la vita: Disciplina della nascita tra medicina e morale nell'Ottocento* (Bologna: Il Mulino, 2006).

[31] For surveys, see Jane Maienschein, *Transforming Traditions in American Biology, 1880–1915* (Baltimore: Johns Hopkins University Press, 1991); Nyhart, *Biology Takes Form*, pp. 243–361. See also Paul Julian Weindling, *Darwinism and Social Darwinism in Imperial Germany: The Contribution of the Cell Biologist Oscar Hertwig (1849–1922)* (Stuttgart: Gustav Fischer, 1991); Klaus Sander, "Von der Keimplasmatheorie zur synergetischen Musterbildung – Einhundert Jahre entwicklungsbiologischer Ideengeschichte," *Verhandlungen der Deutschen Zoologischen Gesellschaft*, 83 (1990), 133–77; Reinhard Mocek, *Die werdende Form: Eine Geschichte der Kausalen Morphologie* (Marburg: Basilisken-Presse, 1998).

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and pulls by which His had explained the form of the body, but rather signaled a Kantian commitment to causal explanation. Like His, Roux looked to factors in the here and now, but unlike His, he expected conclusive demonstrations of their actions and interactions from experiment alone. In 1888, Roux destroyed with a hot needle one of the two cells formed by cleavage of a fertilized frog egg, a maneuver he likened to throwing a bomb into a textile factory with a view to learning about its internal organization from the change in production. What would the undestroyed cell make? Obtaining a half-embryo (Figure 16.4A), Roux interpreted the result in terms of the "self-differentiation" as opposed to the "dependent differentiation" of the parts.

A year before Roux, Laurent Chabry (1855–1893) had reported a similar experiment on ascidian embryos and with similar results, yet because he worked in the French teratological tradition, these had a different significance. Etienne Geoffroy Saint-Hilaire had varnished hens' eggs in the 1820s, and from the 1850s Camille Dareste, aiming to produce new species by mimicking environmental change, made malformations by the same methods. These naturalists generated new forms to anatomize and taxonomize, or investigated the disturbances for their own sake, rather than using experiment to draw conclusions about normal development. With French zoology on the defensive against Claude Bernard's deterministic physiology, Chabry did not go decisively beyond this tradition, but Roux was oriented toward German physiologists' reductionism and embraced the Bernardian ideal of control.[32]

Roux may have had little to say about evolution, but he was open to phylogenetic questions and included an inherited determination complex among the causal factors of development. In the next generation, Haeckel's student Hans Driesch (1867–1941) accepted his teacher's stark opposition between mechanics and phylogeny, and – cushioned by a private fortune – vehemently rejected evolution. In 1891, shortly after his mathematical-mechanical approach had led Haeckel to recommend a spell in a psychiatric hospital, Driesch carried out a series of landmark experiments. Shaking apart the first two cells of a sea urchin embryo produced not two half-embryos, as Roux's frog results predicted, but two half-sized normal larvae (Figure 16.4B). This discovery of "regulation" set the agenda for a great deal of later work: How could an embryo possibly overcome such massive intervention and still develop into a harmonious whole? Although he had initially worked within a mechanistic framework, by 1900 Driesch had come to doubt that any machine could mimic the regulative ability of the embryo. He became

[32] Frederick B. Churchill, "Chabry, Roux, and the Experimental Method in Nineteenth-Century Embryology," in *Foundations of Scientific Method: The Nineteenth Century*, ed. Ronald N. Giere and Richard S. Westfall (Bloomington: Indiana University Press, 1973), pp. 161–205; Jean-Louis Fischer, *Leben und Werk von Camille Dareste, 1822–1899: Schöpfer der experimentellen Teratologie*, trans. Johannes Klapperstück, Acta historica Leopoldina, vol. 21 (Leipzig: Barth, 1994); Jean-Louis Fischer, "Laurent Chabry and the Beginnings of Experimental Embryology in France," in Gilbert, *Conceptual History of Modern Embryology*, pp. 31–41.

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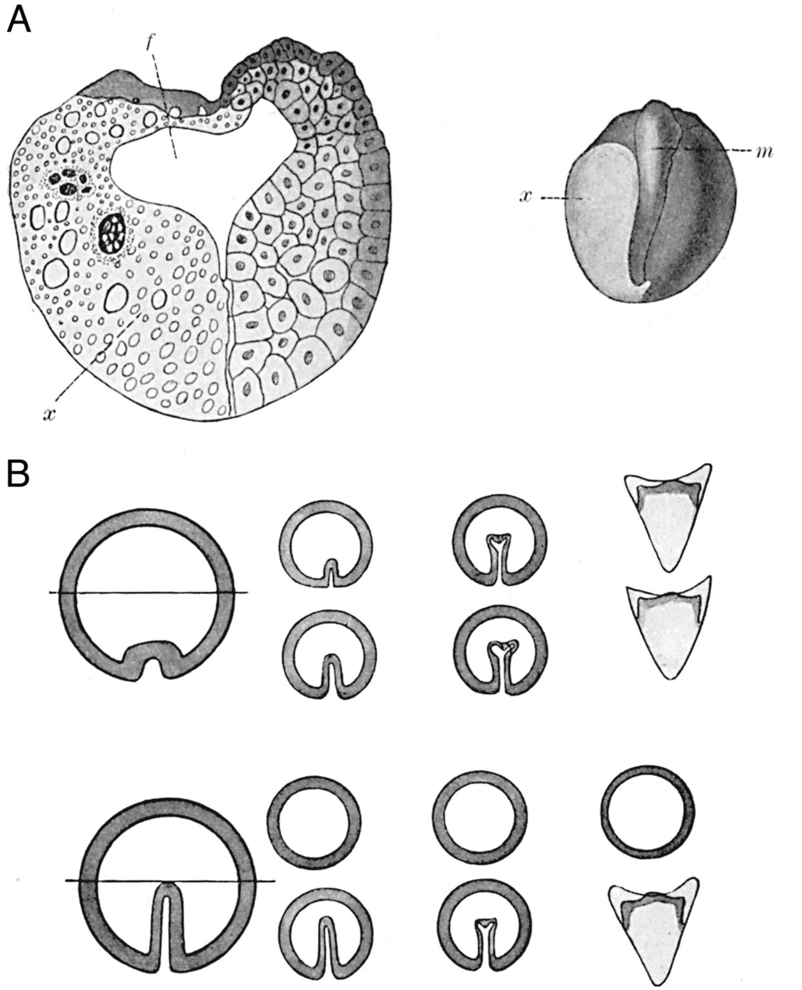


Figure 16.4. Classics of *Entwicklungsmechanik.* (A) Results of Wilhelm Roux's hot-needle experiment: section of frog embryo at blastula stage and dorsal view at neurula stage; "x" marks the damaged half. (B) Schematics of Hans Driesch–style experiments by Thomas H. Morgan showing which divisions of sea urchin embryos during and after gastrulation (upper and lower panels, respectively) regulate to produce whole larvae. From the early textbook by Otto Maas *Einführung in die experimentelle Entwickelungsgeschichte (Entwickelungsmechanik)* (Wiesbaden: Bergmann, 1903), pp. 33, 88.

a philosophy professor and espoused a vitalism that reached very wide audiences but was unpopular among biologists (and philosophers).

Roux found it difficult to establish his new science in the stagnating German university system that militated against specialization; experiments tended merely to embellish rather stable medical courses. So marine

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stations and independent research institutes became the key European sites of *Entwicklungsmechanik*. In the United States, the field flourished at the Marine Biological Laboratory at Woods Hole on Cape Cod and was also more readily integrated into departments of biology. German emigré Jacques Loeb's artificial parthenogenesis of sea urchin eggs, performed at Woods Hole in 1899, was interpreted in the newspapers as "a long step towards . . . creat[ing] life in a test tube"; feminists were intrigued by the prospect of male redundancy. But *Entwicklungsmechanik* was far from the only game in town: Cell lineage work on invertebrates was neither primarily experimental nor modeled on physiology; painstaking morphological investigations followed cell divisions and colored plasms through the generations.[33]

More generally, descriptive work not only persisted but also changed. Indeed, as the field against which experimentalists demarcated their own endeavors, "descriptive embryology" was arguably only created around 1900. The term "descriptive" had long been used in embryology, but in the early nineteenth century it meant anatomical as opposed to comparative studies, whereas Haeckel used it to disparage work not informed by phylogenetic concerns. Now, as the worthy but dull counterpart of experimental embryology, "descriptive embryology" was reframed to include both comparative and phylogenetic work. Yet though increasingly thrown onto the defensive, and in crisis at the grand theoretical level, "descriptive embryology," especially of the vertebrates, was being transformed technically and institutionally in ways that lastingly shaped embryo science.[34]

The technical transformation was modeled on the monumental study with which in the early 1880s Wilhelm His reformed research on human embryos; though experimentally inaccessible these were the prime medical and anthropological concern. He disciplined physicians to collect rare aborted material, rendered this into embryological drawings, arranged the pictures in developmental order, and selected those most likely to represent normal development for inclusion in a "normal plate" depicting a series of standard images from the end of the first two weeks through the first two months of pregnancy. This was far from trivial: Modern human embryology was founded on the exclusion, after a seven-year controversy, of an embryo described as human – and supporting Haeckel's views – that His eventually persuaded anatomists was in fact that of a bird. The normal plate provided a framework for a wealth of research on human embryos and was used as a model for formalizing developmental sequences in other species.[35]

33 Philip J. Pauly, *Controlling Life: Jacques Loeb and the Engineering Ideal in Biology* (Berkeley: University of California Press, 1987), fig. 10 and pp. 100–1; Jane Maienschein, *100 Years Exploring Life, 1888–1988: The Marine Biological Laboratory at Woods Hole* (Boston: Jones and Bartlett, 1989).

34 Nick Hopwood, "Visual Standards and Disciplinary Change: Normal Plates, Tables and Stages in Embryology," *History of Science*, 43 (2005), 239–303, especially p. 244.

35 Hopwood, "Producing Development"; Hopwood, "Visual Standards and Disciplinary Change."

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His also insisted that to grasp complex microscopic structures it was necessary to reconstruct wax models from serial sections. As modeling became a crucial method of research, monographs and articles described models, which Adolf and Friedrich Ziegler of Freiburg in Baden "published" in parallel and sold to institutes all over the world (Figure 16.5C).[36] Scientists who published with the Ziegler studio did not abandon evolutionary interests; they rather used normal plates and plastic reconstruction to reinvestigate Haeckel's questions in exquisitely detailed analyses, especially of scarce and complex mammalian embryos. But embryology's independence was promoted by the perceived need to gain a much stronger empirical basis if the science was to continue to contribute to a phylogeny increasingly dominated by comparative anatomy and paleontology.

This "descriptive" vertebrate embryology was not simply entrenched in old institutes; it took three major institutional initiatives. First, from 1897, the German anatomist Franz Keibel edited an international series of normal plates to provide a basis for reinvestigating the relations of ontogeny and phylogeny. The subsidiary goal, in a science that is often said to have been mired in "typological" thinking, was to study variation between individual embryos.[37] Second, the International Institute of Embryology was founded in 1911 as a club devoted to comparative vertebrate embryology and specifically to promoting the collection and study of the embryos of endangered colonial mammals. Its monument is the Central Embryological Collection of the Hubrecht Laboratory; Figure 16.5A shows jars of whole embryos in alcohol and Figure 16.5B cabinets of sectioned embryos on slides.[38] Third, in 1914, His's student Franklin Paine Mall obtained funds from the Carnegie Institution of Washington for a Department of Embryology at the Johns Hopkins

[36] Hopwood, *Embryos in Wax*.
[37] Hopwood, "Visual Standards and Disciplinary Change."
[38] P. D. Nieuwkoop, "'L'Institut International d'Embryologie' (1911–1961)," *General Embryological Information Service*, 9 (1961), 265–9; Patricia Faasse, Job Faber, and Jenny Narraway, "A Brief History of the Hubrecht Laboratory," *International Journal of Developmental Biology*, 43 (1999), 583–90; Michael K. Richardson and Jennifer Narraway, "A Treasure House of Comparative Embryology," *International Journal of Developmental Biology*, 43 (1999), 591–602.

Figure 16.5 (opposite). Collections of embryos. (A) Whole embryos of the macaque monkey *Macaca irus*, and (B) sections of the embryos on microscope slides at the Central Embryological Collection, Hubrecht Laboratory, Utrecht, in the 1990s. (In 2004, the collection was moved to the Museum für Naturkunde, Berlin.) (C) Friedrich Ziegler's prizewinning display of embryological wax models, many reproduced from plastic reconstructions of serial sections, at the 1893 World's Columbian Exposition in Chicago. From *Prospectus über die zu Unterrichtszwecken hergestellten Embryologischen Wachsmodelle von Friedrich Ziegler (vormals Dr. Adolph Ziegler)* (Freiburg in Baden: Atelier für wissenschaftliche Plastik, 1893). (A–B) courtesy of the Hubrecht Laboratory and (C) Cornell University Library, Rare and Manuscript Collections.

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University in Baltimore; it became a "bureau of standards" for human embryos.[39] The explorers of the gravid uterus compared ever-younger human specimens – increasingly obtained during gynecological operations – with the embryos of "out-of-the-way species" that they "ransacked" from the "jungles and hillsides of the world" or took from colonies, including of primates, at home. Moderating Haeckel's evolutionary zeal, they concluded that the human embryo is an archive in which is written evidence of descent, but it is also a germ, which must live and so is "open for business during construction."[40]

Experiment, we can conclude, worked in two ways: as a practice and as a rhetoric, even an ideology.[41] As a practice, experiment became the method of highest status. As a rhetoric, experimentalism associated its practitioners with modern rigor and control and simultaneously created "descriptive embryology" as its unglamorous other, ideally relegated to a "classical" past. Experiment did not in fact replace analysis but was added to it. Experimentalists sought to reveal the potentialities of parts and analyzed operated embryos for the presence or absence of tissues, cells, or molecules; they also invested time in making standards, "normal stages" adapted from Keibel's plates and "fate maps," against which to assess the effects of their interventions. Nor did "descriptive embryology" just fade slowly into the background; in the years before World War I, when most histories have experimenters making all the running, "descriptive" embryologists founded both the first specifically embryological society and the first research institution dedicated to the science. And though the war seriously disrupted the European initiatives, comparative work continued.

## ORGANIZERS, GRADIENTS, AND FIELDS

Building new experimental sciences onto the dilapidated but still inhabited evolutionist mansion threatened its very foundations. And, as debates over mechanism and vitalism engaged wide audiences, it was hard to contain fundamental metaphysical and methodological disputes – especially during war, revolution, and slump. Some biologists and clinicians were more interested

39 Ronan O'Rahilly, "One Hundred Years of Human Embryology," *Issues and Reviews in Teratology*, 4 (1988), 81–128, at p. 93; Lynn Morgan, "Embryo Tales," in *Remaking Life and Death: Toward an Anthropology of the Biosciences*, ed. Sarah Franklin and Margaret Lock (Santa Fe, N.M.: School of American Research Press, 2003), pp. 261–91; Jane Maienschein, Marie Glitz, and Garland Allen, eds., *Centennial History of the Carnegie Institution of Washington*, vol. 5: *The Department of Embryology* (Cambridge: Cambridge University Press, 2004); Hopwood, "Visual Standards and Disciplinary Change," pp. 281–4.

40 Quotations from George W. Corner, *Ourselves Unborn: An Embryologist's Essay on Man* (New Haven, Conn.: Yale University Press, 1944), p. 28; O'Rahilly, "One Hundred Years of Human Embryology," p. 99.

41 Oppenheimer, *Essays in the History of Embryology and Biology*, pp. 4–10; Maienschein, *Transforming Traditions in American Biology*; Pickstone, "Museological Science?"

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in what, from organ transplantation to limb regeneration, they might make Roux's science do. Others, seeking to unify the life sciences, warned of a crisis and searched for a synthesis. Those working in "*Entwicklungsmechanik*," "developmental physiology," or "experimental morphology," as it was variously called, adopted two strategies for bringing order. In 1919, Haeckel's last student, Julius Schaxel, argued that only wholesale theoretical clarification could overcome fragmentation, discipline speculation, and guide experiment, and he founded the first journal of "theoretical biology."[42] Others pursued experimental programs using highly productive systems to define "organizers," "gradients," and "fields," organicist entities designed to avoid both the mechanist Scylla and the vitalist Charybdis.[43]

Early twentieth-century experimentalists refined the tools for answering the questions raised by Roux and Driesch. Under low-power stereomicroscopes, they exploited especially the remarkable healing powers of amphibian embryos, transplanting tissue from one embryo to another or removing it for culture in isolation. Some manipulations focused on cells. In 1907, American zoologist Ross Harrison (1870–1959) explanted parts of the larval neural tube into clotted lymph; by watching living neuroblasts send out fibers, he decisively supported the neuron doctrine and pioneered modern cell culture. Other experiments asked whether a graft would develop according to its origin or its new location, or whether an explant had become self-sufficient or still needed further interactions. In this way, Harrison defined the mesodermal cells of the limb rudiment as what would be called a "field," a physically bounded area of interaction within which state of determination is a function of position.[44]

Chicago biologist Charles Manning Child (1869–1954) cut a piece out of the middle of a flatworm and found that anterior structures regenerated anteriorly and posterior structures at its posterior end. Each cell could form any structure; what it made appeared to be determined by an original polarity. Some saw here gradients of a formative substance, but shortly before World War I, Child articulated a dynamic view of a polarity of activity. Flatworms placed in a cyanide solution died from the head backward, indicating an anteroposterior gradient in metabolic rate, which, he argued, was expressed in the structure of the worm. Developmental plasticity supported an antihereditarian social philosophy but also a disciplinary politics: As carriers of developmental memory, gradients competed with the genes to explain inheritance.[45]

[42] Nick Hopwood, "Biology between University and Proletariat: The Making of a Red Professor," *History of Science*, 35 (1997), 367–424.

[43] Donna Jeanne Haraway, *Crystals, Fabrics, and Fields: Metaphors of Organicism in Twentieth-Century Developmental Biology* (New Haven, Conn.: Yale University Press, 1976).

[44] Maienschein, *Transforming Traditions in American Biology*, pp. 261–89; Klaus Sander, "An American in Paris and the Origins of the Stereomicroscope," *Roux's Archives of Developmental Biology*, 203 (1994), 235–42.

[45] Gregg Mitman and Anne Fausto-Sterling, "Whatever Happened to *Planaria*? C. M. Child and the Physiology of Inheritance," in *The Right Tools for the Job: At Work in Twentieth-Century Life Sciences*,

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Just as axial gradients originated in ideas of polarity and postulated a privileged region, so did the "organizers" of German zoologist Hans Spemann (1869–1941). He led the dominant school of interwar embryologists in microsurgery with fine glass instruments on cultures of amphibian spawn (Figure 16.6A). At Freiburg in the early 1920s, Spemann's student Hilde Pröscholdt (later Mangold) carried out the most exciting biological experiment of the age. She transplanted the "dorsal lip" of a newt gastrula into the belly of a host embryo of a more darkly pigmented species and found that it induced the host tissues to participate in the formation of a secondary axis, including a central nervous system (Figure 16.6B). Spemann called the dorsal lip the "organizer," which in Germany on the brink of civil war was a metaphor for the restoration of social order. He envisaged development as a sequence of inductive interactions following this "primary" induction and in 1936 won a Nobel Prize.[46]

The productivity of these experimental systems stimulated even such polymaths as Julian Huxley to concentrate their laboratory work in embryology and fueled his and his brother Aldous's science fiction. Julian's "amazing story" of 1927 had a British researcher become religious adviser to an African king and mass-produce "living fetishes," including double-headed toads and three-headed snakes, by applying the "methods of Mr. Ford" to some of Spemann's and Harrison's artisanal experiments. Excitement reached fever pitch with the discovery in 1932, mainly by Spemann's student Johannes Holtfreter (1901–1992), that fixed, boiled, or otherwise mistreated organizers induced normal structures. The suggestion that the active principle could be isolated chemically captivated a group of Cambridge radicals who had started to meet in an informal "Theoretical Biology Club." Joseph Needham (1900–1995) and comrades took from Germany both Schaxel's vision of a theoretical biology and Holtfreter's embryological techniques, and combined them with the local biochemistry. The team prepared cell-free inducing extracts, but their organicist molecular models respected different hierarchical levels in the whole embryo.[47]

In practice, the organizer proved biochemically intractable, and attempts to marry organizers and gradients or gradients and fields were short-lived. Instead of leading a grand embryological synthesis, by the mid-1940s

ed. Adele E. Clarke and Joan H. Fujimura (Princeton, N.J.: Princeton University Press, 1992), pp. 172–97.

[46] Peter E. Fäßler, *Hans Spemann, 1869–1941: Experimentelle Forschung im Spannungsfeld von Empirie und Theorie. Ein Beitrag zur Geschichte der Entwicklungsphysiologie zu Beginn des 20. Jahrhunderts* (Berlin: Springer, 1997).

[47] C. Kenneth Waters and Albert Van Helden, *Julian Huxley: Biologist and Statesman of Science* (Houston, Tex.: Rice University Press, 1992); Julian Huxley, "The Tissue-Culture King," *Yale Review*, 15 (1926), 479–504; Susan Merrill Squier, *Babies in Bottles: Twentieth-Century Visions of Reproductive Technology* (New Brunswick, N.J.: Rutgers University Press, 1994), pp. 24–62; P. G. Abir-Am, "The Philosophical Background of Joseph Needham's Work in Chemical Embryology," in Gilbert, *Conceptual History of Modern Embryology*, pp. 159–80.

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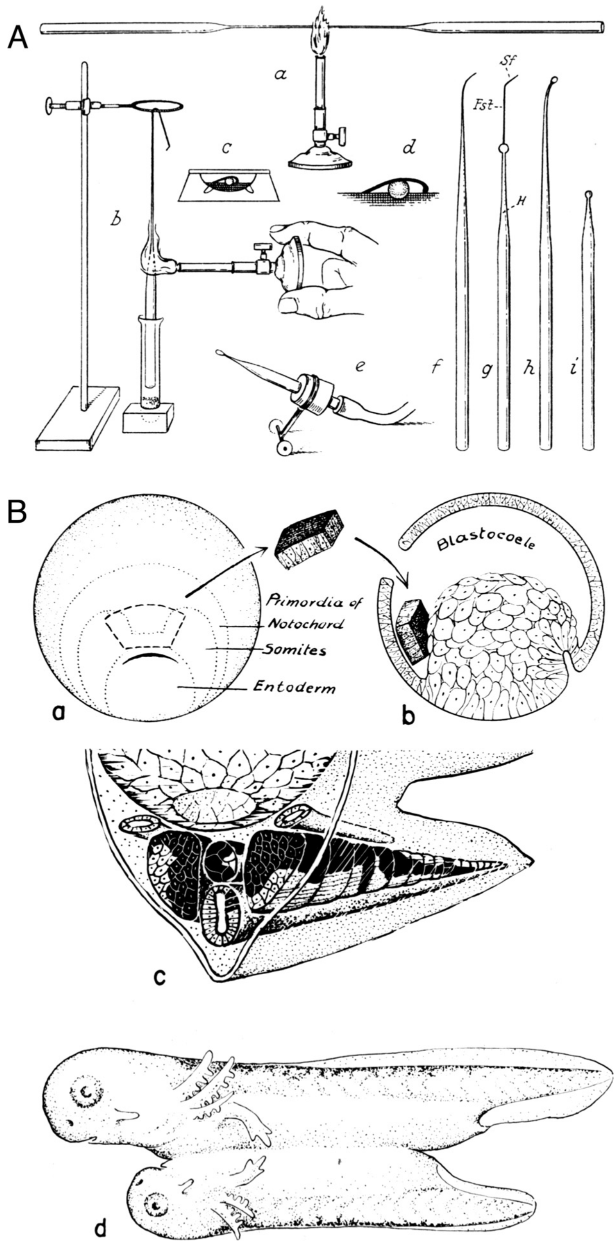


Figure 16.6. Hans Spemann's developmental physiology. (A) How to make the microsurgical instruments; from Otto Mangold, *Hans Spemann, ein Meister der Entwicklungsphysiologie: Sein Leben und sein Werk* (Stuttgart: Wissenschaftliche Verlagsgesellschaft, 1953), p. 111. (B) Simplified design of the organizer graft, in which (a) the dorsal lip from a darkly pigmented donor is (b) inserted into the blastocoelic cavity of a lightly pigmented host, where it (c, d) induces host tissues to participate in forming a secondary axis. From J. Holtfreter and V. Hamburger, "Amphibians," in *Analysis of Development*, ed. Benjamin H. Willier, Paul A. Weiss, and Viktor Hamburger (Philadelphia: Saunders, 1955), pp. 230–96, at p. 244.

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embryologists found themselves on the sidelines of the distinctly nonembryological Modern Evolutionary Synthesis. Anachronistically, "chemical embryology" is often reckoned to have run into the sands because the problem needed the molecular biological techniques that were not applied to it until the 1980s. Historically, we can understand the fate of embryology only in relation to other sciences with alternative programs. We should also remember that in many departments embryology remained a comparative science well into the postwar era.

## EMBRYOS, CELLS, GENES, AND MOLECULES

A major drive in twentieth-century biology was to gain access to, and perhaps to explain, mechanisms of development through the properties of cells, molecules, and genes. But other sciences – especially biochemistry, molecular biology, and genetics – claimed embryonic components for themselves. What were their relations to embryology, and what was their relative status and success? Embryology appears in the decades around 1900 as the powerhouse of the new biology, generating such key innovations as cell culture and the gene. After World War II, by contrast, it struck biochemists, geneticists, and the new molecular biologists as a field of great problems but no progress. Recast by the 1960s as "developmental biology," in the last quarter of the twentieth century it was a very active area of research.

The first geneticists drove a wedge between genetic transmission and embryonic development, between nucleus and cytoplasm. Genetics has been better funded, of higher status, and, until recently, more successful; developmental biology has been championed as a more holistic, feminine, embodied, and European alternative to the dominantly reductionist, masculine, abstract, and American style of genetics.[48] American genetics in part came out of an embryological debate in the early 1900s about the relative importance of the nucleus and cytoplasm in development; Thomas H. Morgan's work on the fruit fly *Drosophila* convinced him of the importance of the nucleus and the chromosomes on which he began to localize genes. Interwar embryologists, conversely, came to regard the cytoplasm as the more interesting part of the cell; genes that affected only eye color struck them as too trivial to explain how the eye itself formed. Greater pragmatism, and funding by agricultural and eugenic interests, allowed geneticists to claim pride of place in an evolutionary synthesis organized around quantitative change in gene frequencies. Whereas in the late nineteenth century the mechanism of evolution was development, by shunning evolutionary questions, experimental

[48] Evelyn Fox Keller, *Refiguring Life: Metaphors of Twentieth-Century Biology* (New York: Columbia University Press, 1995), pp. 3–42.

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embryology had left a vacuum for genetics to fill.[49] (See Burian and Zallen, Chapter 23, this volume.)

After World War II, massive investment in other biomedical sciences pushed embryology to the margins. Research continued, however, on a series of levels, from the whole embryo through cells to molecules, until in the 1960s the science was reformed. In the United States, from 1939, the Society for the Study of Growth, later the Society for Developmental Biology, brought embryologists and other scientists together. In the early 1930s, the International Institute of Embryology had made a modest overture to experiment, and in 1968 it was renamed the International Society of Developmental Biologists. Developmental biology was a joint initiative of self-consciously "modern" embryologists and geneticists, biochemists, cell biologists, and molecular biologists who saw a field ripe for their skills. It took over the problems and practices of experimental embryology but drew on other resources to claim a universal role in explaining development and differentiation throughout the living world.[50]

The new field's key generalization was development as differential gene expression. From the late 1930s, when several influential embryologists and geneticists converted to "developmental genetics," it became clear that mutations could have embryologically interesting effects. Transplantations of nuclei from differentiated frog cells into enucleated eggs suggested from the late 1950s that they still had all the genes to make at least a tadpole, perhaps a frog – and sparked a public debate about cloning. At the Pasteur Institute in Paris, molecular biologists presented bacterial genes turning on and off in response to environmental stimuli as a model for multicellular differentiation. Did the ensuing drive to investigate "gene activity in early development" represent a long takeover of embryology by genetics and molecular biology or is it better seen as an updated version of (bio)chemical embryology? Tools were increasingly imported from outside, but the traffic was not all one way. There remained among embryologists a powerful impetus toward molecular analysis: Around 1960, Jean Brachet's nucleic acid cytochemistry had a hand in the notion of mRNA, and in the 1970s frog oocytes became a favored system for testing the expression of eukaryotic genes.[51]

[49] Reviewed in Scott F. Gilbert, John M. Opitz, and Rudolf A. Raff, "Resynthesizing Evolutionary and Developmental Biology," *Developmental Biology*, 173 (1996), 357–72.

[50] Jane M. Oppenheimer, "The Growth and Development of Developmental Biology," in *Major Problems in Developmental Biology*, ed. Michael Locke, Symposia of the Society for Developmental Biology, vol. 25 (New York: Academic Press, 1966), pp. 1–27; Oppenheimer, *Essays in the History of Embryology and Biology*, pp. 1–61; Keller, *Refiguring Life*.

[51] Scott F. Gilbert, "Induction and the Origins of Developmental Genetics," in Gilbert, *Conceptual History of Modern Embryology*, pp. 181–206; Richard M. Burian, "Underappreciated Pathways toward Molecular Genetics as Illustrated by Jean Brachet's Chemical Embryology"; Scott F. Gilbert, "Enzymatic Adaptation and the Entrance of Molecular Biology into Embryology," in *The Philosophy and History of Molecular Biology: New Perspectives*, ed. Sahotra Sarkar, Boston Studies in the Philosophy of Science, vol. 183 (Dordrecht: Kluwer, 1996), pp. 67–85, 101–23. For contrasting views, see

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As studies of cell differentiation continued, some developmental biologists insisted that there was more to development than that. How did the embryo make not just skin, muscle, and bone – but a hand? One answer was morphogenesis, a term used in this period to refer specifically to changes in embryonic form in early development, notably gastrulation and neurulation. In the mid-1950s, attention was focused on the cell surface by experiments showing that if cells from different germ layers were disaggregated, mixed, and reaggregated, they could re-sort. Scientists in the borderlands of embryology and cell biology explored cell adhesion and locomotion, attempting to understand their coordination and searching for the subcellular components responsible for their specificity. But from the late 1960s, "pattern formation" was promoted as deeper than either differentiation or morphogenesis. The concept of "positional information" sought to specify how cells "know" their relative positions in a field and regulate by recognizing discontinuities. With experiments on insect embryos, this boosted gradients back into the mainstream – but now of "morphogens" activating batteries of genes in patterns.[52]

Developmental biology was made in part by reinventing experimental embryology, in part by biochemists and molecular biologists who despised the embryological tradition but saw in its failure a challenge. Embryology appeared to one biochemist as "a field so primitive that no modern research was being done in it. And yet it had this huge, incredible problem – how an egg develops into a multicelled organism."[53] In the mid-1970s, Christiane Nüsslein-Volhard (b. 1942) went from molecular biology to learn methods for working with mutations that affected early *Drosophila* embryos – and then increased their productivity a hundredfold. At the European Molecular Biology Laboratory in Heidelberg, she and Eric Wieschaus screened not for a couple of genes but for all those controlling segmentation.[54] This was not "molecular" work but classical developmental genetics pursued in an unusually aggressive style; combining it with experimental embryology and new technology for cloning the genes won them a Nobel Prize. Nüsslein-Volhard's colleagues did not just represent the progressive specification of the axes in terms of a hierarchy of interacting genes, mRNAs, and proteins (Figure 16.7); they visualized a gradient of the anterior morphogen and watched how changing its concentration altered the body plan. In the 1980s, some

J. B. Gurdon, "Introductory Comments," and G. M. Rubin, "Summary," in "Molecular Biology of Development," *Cold Spring Harbor Symposia on Quantitative Biology*, 50 (1985), 1–10 and 905–8, respectively.

[52] Sander, "Von der Keimplasmatheorie zur synergetischen Musterbildung," pp. 162–72; L. Wolpert, "Gradients, Position and Pattern: A History," in *A History of Embryology*, ed. T. J. Horder, J. A. Witkowski, and C. C. Wylie (Cambridge: Cambridge University Press, 1985), pp. 347–62.

[53] Donald D. Brown, quoted in Patricia Parratt, *One Scientist's Story*, Perspectives in Science, vol. 4 (Washington, D.C.: Carnegie Institution, 1988), p. 6.

[54] Evelyn Fox Keller, "*Drosophila* Embryos as Transitional Objects: The Work of Donald Poulson and Christiane Nüsslein-Volhard," *Historical Studies in the Physical and Biological Sciences*, 26 (1996), 313–46.

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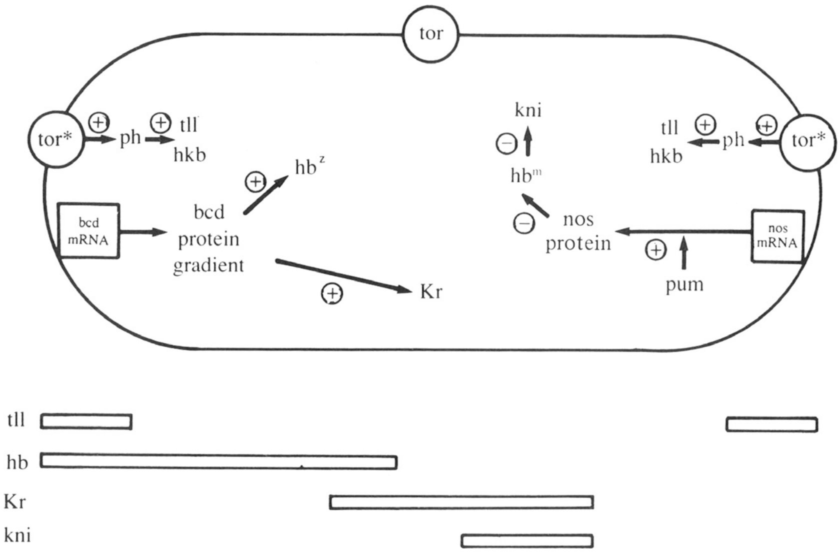


Figure 16.7. Roles of the maternal genes that control the anteroposterior pattern in *Drosophila* in activating (+) or repressing (−) expression of the first zygotic developmental genes (bars below). From J. M. W. Slack, *From Egg to Embryo: Regional Specification in Early Development*, 2nd ed. (Cambridge: Cambridge University Press, 1991), p. 238.

embryologists saw molecular cloning as finally reducing development to gene expression and developmental biology to an anonymous grind in the sweatshops of molecular cell biology. By the 1990s, however, they were analyzing complex phenomena, including Spemann's organizer and flower formation, with techniques of unprecedented sophistication and depth, displaying and manipulating embryos as never before.

Developmental biology focused on principles it claimed would be universal and so could be studied in whatever species was most convenient. By the late 1980s, most work used one of only a half dozen "model organisms": *Drosophila*, the frog *Xenopus*, mouse, chick, a nematode worm, and zebrafish, plus the mustard *Arabidopsis* as a model flowering plant.[55] Frog embryologists transplanted, microinjected, and did biochemistry but almost no classical genetics; drosophilists mutated and crossed but struggled to manipulate the embryo directly. As the field expanded, they formed distinct communities specializing in different phenomena and techniques and attending organism-specific meetings. Textbooks had long presented a composite view,

[55] Jessica A. Bolker, "Model Systems in Developmental Biology," *BioEssays*, 17 (1995), 451–5; Soraya de Chadarevian, "Of Worms and Programmes: *Caenorhabditis elegans* and the Study of Development," *Studies in History and Philosophy of Biological and Biomedical Sciences*, 29 (1998), 81–105; John B. Gurdon and Nick Hopwood, "The Introduction of *Xenopus laevis* into Developmental Biology: Of Empire, Pregnancy Testing and Ribosomal Genes," *International Journal of Developmental Biology*, 44 (2000), 43–50.

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exemplifying different developmental mechanisms in whatever systems had been engineered for a particular job. The investment necessary to make a species accessible to genetics and/or molecular biology reinforced a paradoxical ghettoization. But searches for homologous DNA sequences across the animal kingdom showed, for example, that flies and mice have a similar set of genes, arranged in the same chromosomal order, which they use to identify the same relative positions along the anteroposterior axis. This breathed new life into once-scorned evolutionary studies; it again appeared rewarding to work not just on fruit flies but on bugs, spiders, and lobsters as well. Promoters of "evo-devo" hold out the prospect of a new synthesis organized around macroevolution, homology, and embryology, the very problems that the Modern Synthesis excluded.[56]

## EMBRYOLOGY AND REPRODUCTION

Although many experimental embryologists and developmental biologists took biomedical funding but valued independence from medical service roles, much embryology in the twentieth century was oriented primarily toward medicine and agriculture. Those studying mammals especially have been engaged not only in academic biology but also in rationalizing human and animal reproduction. Promoting its scientific representation, planning, and control has begun to achieve long-term goals but also galvanized a wide range of critics, from antiabortionists to feminists, from conservative defenders of "traditional" families to red-green opponents of commodifying life.

Anatomist-embryologists continued to be responsible for teaching human development to medical students from textbooks that Carnegie Institution scientists revised with ever more complete embryonic series. In Boston, between 1938 and 1953, gynecologist John Rock and pathologist Arthur T. Hertig recovered fertilized eggs in the first two weeks of development from women scheduled for hysterectomies. The doctors increased the chances and value of a successful "egg hunt," as they called it, by asking their patients to keep rhythm charts in the months before surgery and to note if and when they had sex during their last fertile period. The operation was set for shortly after ovulation, and Hertig took embryos to the Carnegie Department for sectioning.[57]

In the early twentieth century, human embryology's association with sex and evolution still tended to keep it out of the schools. So it was adult educators and sex reformers who first made series of human embryos part

[56] Gilbert, Opitz, and Raff, "Resynthesizing Evolutionary and Developmental Biology"; Walter J. Gehring, *Master Control Genes in Development and Evolution: The Homeobox Story* (New Haven, Conn.: Yale University Press, 1998).

[57] O'Rahilly, "One Hundred Years"; Loretta McLaughlin, *The Pill, John Rock, and the Church: The Biography of a Revolution* (Boston: Little, Brown, 1982), pp. 58–92.

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Figure 16.8. Communicating the embryological vision of pregnancy with a Schick anatomical chart at a maternity welfare center in Paddington, London, around 1950 (London Metropolitan Archives, photograph 80/7364).

of the scientific facts of life (Figure 16.8). Having encountered social worlds in which eggs, sperm, and developing embryos were by no means taken for granted, they deplored what they presented as women's "ignorance" of their own bodies. Working-class women who sought abortions because a missed period indicated that "clotted blood" needed "tipping out" were ignorant by the standards of the relatively new medical knowledge, but their practical understandings of how babies were (not) made often worked.[58]

Embryologists' claims to provide physicians and midwives with knowledge relevant to obstetrics and gynecology had always been strained. But as pregnancy became hospitalized after World War II, obstetric technologies – as x-rays gave way to ultrasound – increasingly visualized inside the womb what embryologists had described only postmortem. Obstetricians, whose primary charge had been the pregnant woman, became advocates for a "fetal patient" that physiologists represented as active and in control. But the unborn may be constructed in diametrically opposed ways. While a fetus is the subject of surgical intervention inside the body of a pregnant woman, in the same hospital, aborted material may be used as a tool for transplantation or research. Embryos and fetuses are now supercharged with controversy. Since the early 1980s, antiabortion activists have deployed embryonic and

[58] Cornelie Usborne, "Rhetoric and Resistance: Rationalization of Reproduction in Weimar Germany," *Social Politics*, 4 (1997), 65–89, at pp. 80–1.

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fetal images as weapons against the reforms of the late 1960s and early 1970s. Feminists critiqued icons of the "unborn child" for blurring the distinction between embryo and baby, and – like much human embryology – for constructing an illusion of autonomous fetal development only by effacing pregnant women.[59]

In the early twentieth century, reproductive scientists carved out from embryology a new field of research on sex, but attempts to control reproduction by manipulating gametes and early embryos continued to overlap with embryology. After World War II, in vitro fertilization and embryo transfer were presented as offering the potential for livestock to be bred more intensively from valuable females and for women to overcome infertility caused by blocked Fallopian tubes. During the 1950s, earlier reports of in vitro fertilization became so mistrusted that it was very hard to make claims stick. The 1969 announcement that Robert Edwards, a physiologist at the University of Cambridge, had managed it for humans was universally accepted only a decade later, when he collaborated with Oldham gynecologist Patrick Steptoe and technical assistant Jean Purdy to help Lesley Brown have a baby by laparoscopically removing a mature oocyte, fertilizing it in vitro, and replacing the embryo in her uterus. Meanwhile, culture techniques had begun to overcome the obstacles to experimental analysis of the small and inaccessible mammalian embryos, and in the 1970s cattle embryo transfer was made a major international business. The 1997 report of the cloning of a sheep by nuclear transplantation from an adult udder realized developmental biologists' long-standing ambition to show that the nucleus of a differentiated mammalian cell is totipotent. This technique, combined with advances in stem-cell culture, is also opening up new markets in agriculture, pharmaceuticals, and "regenerative medicine."[60]

The second wave of feminism brought radical broadsides against embryology's complicity in a male takeover of female procreative powers. Feminists in and around developmental biology – where women were by the 1980s and 1990s unusually well represented – led a more conciliatory and more successful campaign against, for example, mapping stereotypes of active male

[59] Ann Oakley, *The Captured Womb: A History of the Medical Care of Pregnant Women* (Oxford: Blackwell, 1984); Monica J. Casper, *The Making of the Unborn Patient: A Social Anatomy of Fetal Surgery* (New Brunswick, N.J.: Rutgers University Press, 1998); Lynn M. Morgan and Meredith W. Michaels, eds., *Fetal Subjects, Feminist Positions* (Philadelphia: University of Pennsylvania Press, 1999).

[60] Adele E. Clarke, *Disciplining Reproduction: American Life Sciences and "The Problems of Sex"* (Berkeley: University of California Press, 1998); Robert Edwards and Patrick Steptoe, *A Matter of Life: The Story of a Medical Breakthrough* (London: Hutchinson, 1980); C. E. Adams, "Egg Transfer: Historical Aspects," in *Mammalian Egg Transfer*, ed. C. E. Adams (Boca Raton, Fla.: CRC Press, 1982), pp. 1–17; John D. Biggers, "*In vitro* Fertilization and Embryo Transfer in Historical Perspective," in *In vitro Fertilization and Embryo Transfer*, ed. Alan Trounson and Carl Wood (London: Churchill Livingstone, 1984), pp. 3–15; Gina Kolata, *Clone: The Road to Dolly and the Path Ahead* (London: Allen Lane, 1997); Sarah Franklin, "Ethical Biocapital," in Franklin and Lock, *Remaking Life and Death*, pp. 97–127.

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and passive female onto sperm/nucleus and egg/cytoplasm.[61] Since the birth of Louise Brown, many have been assisted to have much-wanted children. In spite of criticism of the heavy emotional, physical, and financial costs to women of a procedure that usually failed, the discussion quickly moved on to the legal regulation of the market in reproductive services and the ethics of experimentation on surplus embryos. In response to a backlash from antiabortion groups, British scientists lobbied to be allowed to continue embryo research. Although in clinics eggs may be represented as children as little as an hour after fertilization, scientists argued that research should be permitted until the appearance of the primitive streak, an early sign of gastrulation. Whereas in the United States a ban on federal funding pushed the work into an unregulated private sector, in 1990 the U.K. Parliament recognized it as legitimate up to fourteen days but as requiring strict regulation by a Human Fertilization and Embryology Authority.[62]

At the start of the twenty-first century, embryology is again a high-profile science, as it was at the beginnings of the nineteenth and twentieth. But embryologists no longer just analyze embryos or even intervene experimentally in development; cloning companies and fertility clinics are creating new organisms. The identities and relations of embryology have also been transformed. Most dramatically and controversially, embryological practices and products have been powerfully extended into medicine, agriculture, and everyday life.

[61] Gena Corea, *The Mother Machine: Reproductive Technologies from Artificial Insemination to Artificial Wombs* (London: Women's Press, 1985); Scott F. Gilbert and Karen A. Rader, "Revisiting Women, Gender, and Feminism in Developmental Biology," in *Feminism in Twentieth-Century Science, Technology, and Medicine*, ed. Angela N. H. Creager, Elizabeth Lunbeck, and Londa Schiebinger (Chicago: University of Chicago Press, 2001), pp. 73–97.

[62] Sarah Franklin, *Embodied Progress: A Cultural Account of Assisted Conception* (London: Routledge, 1997); Michael Mulkay, *The Embryo Research Debate: Science and the Politics of Reproduction* (Cambridge: Cambridge University Press, 1997).

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# 17

# MICROBIOLOGY

*Olga Amsterdamska*

The only constant characteristics of a research area that we can, anachronistically, describe as microbiology might be the minute size of the organisms it studies and its reliance on instruments and a set of techniques that allow us to see beyond the range of what is visible to the naked eye. Stability or continuity are difficult to find elsewhere – either in the range and classification of microorganisms, in the types of questions asked about them, in the theoretical or practical goals of research, in the institutions in which investigations were conducted, or in the composition of the group of scientists to whom these microscopic organisms were of interest.

The range of organisms encompassed by these investigations has changed many times during the last two centuries. Relatively undifferentiated infusoria gave place to protists and schizomycetes, and later to protozoa, bacteria, fungi, and algae; the invisible filterable viruses, obligate parasites, and lytic principles appeared only temporarily, to be replaced by rickettsia and viruses. These microorganisms were investigated by a heterogeneous assembly of amateurs, botanists, zoologists, biologists, pathologists, biochemists, geneticists, medical doctors, sanitary engineers, agricultural scientists, veterinarians, public health investigators, biotechnologists, and so on. Specialisms and disciplines devoted to specific groups of microorganisms – bacteriology, virology, protozoology, and mycology – have disparate though often overlapping institutional and intellectual histories, and although the term "microbiology" dates from the last decades of the nineteenth century, it did not come to designate a discipline that could claim its own sphere of concern until after the Second World War. Even then, the discipline remained both intellectually and institutionally heterogeneous.

Since the late nineteenth century, the study of microorganisms has been dominated by practical concerns such as the protection of public health and the struggle against human or animal disease, or the production of wine, beer, foodstuffs, or industrial chemicals. Today, genetic manipulation of microorganisms plays an important role in biotechnological innovations.



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Microbiological research also played a pivotal role in many fundamental theoretical controversies within biology – in the debates on spontaneous generation, cell theory, the nature of life, classification, speciation, heredity and its mechanisms, and others – and in some of its more revolutionary transitions, such as those from descriptive, morphological, or "natural history" styles of research to experimental biology, or in the development of general biochemistry and, later, molecular biology.

Microorganisms were only rarely studied for their own sake. As the smallest living creatures, they were expected to yield a critical understanding of macroscopic life. They provided a limit – as the simplest organisms, the most bountiful and ingenious ones, the earliest and most primitive, the most adaptable, the most varied and changeable, the most quickly reproducing, or the easiest to transform – against which a variety of general biological claims were tested. As organisms responsible for various fermentations, microorganisms could be used to probe and manipulate the production of many foodstuffs and chemicals, but also to study general biochemistry. As little metabolic factories, they could be harnessed – and today also changed and manipulated – to produce useful substances. When regarded as pathogens, their study promised a means to understand and control human, animal, or plant disease. In genetic or biochemical research, microorganisms were likely to be used as laboratory tools or instruments and to provide insight into metabolic pathways or mechanisms of hereditary transmission.

Given this variety of contexts and concerns, it is difficult to write a history of microbiology that does justice to both the intellectual and institutional complexity of the field's development.[1] In the discussion here, an attempt is made to emphasize the changing interactions between studies of microorganisms conducted in different intellectual and institutional contexts, between research in which microorganisms were used as tools and research that focused on microbes as distinct organisms, and between studies that aim to answer basic biological questions and those devoted to the practical applications of microbiological knowledge.

## SPECIATION, CLASSIFICATION, AND THE INFUSORIA

Throughout the eighteenth century, microorganisms were difficult to see and even harder to understand. The use of microscopes – especially of single-lens

[1] All existing general histories of microbiology (and bacteriology) are by now fairly dated, but the most comprehensive ones are Hubert A. Lechevalier and Morris Solotorovsky, *Three Centuries of Microbiology* (New York: McGraw-Hill, 1965), and Patrick Collard, *The Development of Microbiology* (Cambridge: Cambridge University Press, 1976). The history of medical bacteriology until World War II is discussed in William Bulloch, *The History of Bacteriology* (Oxford: Oxford University Press, 1938), and W. D. Foster, *The History of Medical Bacteriology and Immunology* (London: Heinemann, 1970).

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instruments – required a substantial degree of skill and patience, while the images produced by compound microscopes were often blurred, with each object surrounded by a fringe of colors. Working with microscopes that produced as much doubt as conviction, and without a framework into which to fit their observations of the "infusoria" – as microscopic organisms have been called since the 1760s – only a few eighteenth-century naturalists attempted more than detailed descriptions of the individual miniscule creatures first observed by the Dutch draper Antoni van Leeuwenhoek (1632–1723). Carl Linnaeus himself placed all microorganisms in an order "Vermes" in a class he referred to as "Chaos."

In the course of the nineteenth century, however, studies of microorganisms became more important, and disputes about them came to reflect a number of interrelated controversies in biology. After the 1820s, new achromatic microscopes became available, increasing both the magnification and the sharpness of the images. But the steadily increasing ability to differentiate the organization of the various microorganisms and to see infusoria that had previously been invisible did not settle the existing disagreements among experts. Learning to prepare the specimens and to see through a microscope was a complex process, and it often generated new foci of opposition and controversy among the microscopists.

One such controversy surrounded the theories of the German naturalist Christian Gottfried Ehrenberg (1795–1876), whose classification of infusoria was an elaboration and extension of an eighteenth-century classification of the Danish naturalist Otto Friedrich Müller (1730–1784). In *Die Infusionsthierchen als vollkommene Organismen* (1838), Ehrenberg described and classified hundreds of microorganisms and attempted to show that despite the great variety of their forms, all infusoria had a full set of organ systems and functions. Ehrenberg particularly emphasized the ubiquity and complexity of their digestive system, which his new achromatic microscopes allowed him to see.[2] In 1841, this "polygastric theory" of infusoria was vehemently criticized by the French zoologist Felix Dujardin (1801–1860). Although Dujardin used an admittedly inferior microscope, he concluded that Ehrenberg "has yielded too easily to the rapture of his imagination."[3] The disagreements between Ehrenberg and Dujardin were related less to the differences between what they could or could not see through their respective microscopes than to their more general beliefs about the nature of the organic world.

Questions of general biological interest – how, if at all, infusoria were to be classified, the nature of their morphological structure, how they develop and

[2] Frederick B. Churchill, "The Guts of the Matter – Infusoria from Ehrenberg to Bütschli: 1838–1876," *Journal of the History of Biology*, 22 (1989), 189–213.

[3] John Farley, *The Spontaneous Generation Controversy from Descartes to Oparin* (Baltimore: Johns Hopkins University Press, 1974), p. 55.

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evolve, and how they fit into the rest of the plant and animal world – were the basis of botanists' and zoologists' interest in microorganisms from the 1840s through the 1870s. In 1845, Karl Theodore von Siebold (1804–1885), a zoologist at the University of Freiburg, reclassified the infusoria by drawing a distinction between bacteria and other microorganisms, which he called protozoa. A proponent of the cell theory, he removed bacteria to the plant kingdom (because their motions were involuntary rather than directed) and installed "unicellular" microorganisms as the simplest forms of organisms both in the plant and in the animal kingdoms. The precise relevance of the cell theory and of evolutionary theory to the understanding of protozoa continued to be debated in the work of zoologists such as Friedrich Stein, Ernst Haeckel, and Otto Bütschli.[4]

Concerns about classification and the nature of life were also central in the disputes between the botanist Carl von Nägeli (1817–1891), professor at Zurich and then Munich, and his Breslau counterpart Ferdinand Cohn (1828–1898). At a philosophical level, this dispute opposed materialist and mechanist biologists, who believed in some form of evolutionary transformation of species and emphasized the unity of laws governing both inanimate and animate nature, against others who argued for the unique character of life, strict natural classifications, and division among and fixity of species. Pauline Mazumdar describes this controversy as the long-standing debate between "the Unitarians" and "the Linnaeans," with von Nägeli representing the first group and Cohn the second. Von Nägeli not only developed his own theory of evolution and claimed the possibility of spontaneous generation, but he also believed that bacteria (or schizomycetes, as he called them) are pleomorphic – subject to a variety of transformations – so that it makes little sense to divide them into species. Ferdinand Cohn, in contrast, spent much of his life cataloging and classifying the variety and distinctiveness of bacterial species. Although initially not anti-Darwinian, Cohn basically was not interested in evolutionary ideas and emphasized the fixity and stability of the species he identified. As opposed to von Nägeli, Cohn saw no need for spontaneous generation. These debates between Cohn and von Nägeli were also of central importance in the bacteriological revolution.[5]

In the context of German academic botany, the differences between von Nägeli and Cohn appeared primarily as issues in the philosophy of biology. Because they concerned such fundamental questions as the nature of life and its evolution, the debates often also had religious and political components. Gerald Geison has argued that such fundamental philosophical issues (linked to religious and political views), rather than the more immediate and

[4] Natasha X. Jacobs, "From Unit to Unity: Protozoology, Cell Theory, and the New Concept of Life," *Journal of the History of Biology*, 22 (1989), 215–42.

[5] Pauline M. H. Mazumdar, *Species and Specificity: An Interpretation of the History of Immunology* (Cambridge: Cambridge University Press, 1995), pp. 15–67.

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pragmatic concerns, were centrally important in the initiation of microbiological research by Louis Pasteur.[6]

## WINE, LIFE, AND POLITICS: PASTEUR'S STUDIES OF FERMENTATION

Louis Pasteur (1822–1895), a chemist by training, turned to the study of microorganisms in the 1850s when he became interested in processes of fermentation.[7] According to the traditional account, Pasteur's initial interest in the study of alcoholic fermentations was aroused when he was approached by the Lille industrialist Mourier Bigo, who was experiencing difficulties with the production of alcohol from beets. Geison has questioned the significance of this practical incentive for the change in Pasteur's research focus by pointing to the continuities between Pasteur's chemical studies and his early microbiological work, both of which reflect his abiding interest in the distinction between life and nonliving matter.

Pasteur was not the first scientist to argue that fermentation was a result of the vital activities of living vegetable matter (yeast). Such arguments had been made since the late 1830s by scientists such as Charles Cagniard de Latour (1777–1859), Theodor Schwann (1810–1882), and Friedrich Kützing (1807–1893). But Justus von Liebig (1803–1873), the preeminent organic chemist, regarded fermentation as an act of chemical decomposition analogous to the digestion of food by pepsin, and he ridiculed the idea that live microorganisms are involved in the process. (Pasteur's conclusions were later modified by Edouard Buchner (1860–1907), who demonstrated in 1897 that fermentation can be accomplished by a cell-free extract from yeast that he called "zymase").[8]

Pasteur identified yeast "globules" as living organisms necessary for fermentation to take place, and he demonstrated that they could grow and multiply in the absence of free oxygen, and that each specific type of fermentation (lactic, alcoholic, acetic, butyric, etc.) was caused by a characteristic living ferment. Defining fermentation as "life without air," Pasteur emphasized the importance of the process of fermentation and putrefaction in the general "economy of nature."[9]

[6] Gerald L. Geison, *The Private Science of Louis Pasteur* (Princeton, N.J.: Princeton University Press, 1995).

[7] The literature on Pasteur is voluminous. See, among others, René Dubos, *Louis Pasteur: Free Lance of Science* (Boston: Little, Brown, 1950); Emile Duclaux, *Pasteur: The History of a Mind*, trans. Erwin F. Smith and Florence Hedges (Philadelphia: Saunders, 1920); René Valery-Radot, *La vie de Pasteur*, 2 vols. (Paris: Flammarion, 1900); Claire Salomon-Bayet, ed., *Pasteur et la révolution pastorienne* (Paris: Payot, 1986); Geison, *Private Science of Louis Pasteur*; Gerald L. Geison, "Louis Pasteur," *Dictionary of Scientific Biography*, vol. 10, 350–416.

[8] On Buchner's discovery and its implications, see Robert E. Kohler, "The Background to Edouard Buchner's Discovery of Cell-Free Fermentation," *Journal of the History of Biology*, 4 (1971), 35–61.

[9] James Bryant Conant, "Pasteur's Study of Fermentation," in *Harvard Case Histories in Experimental Science* (Cambridge, Mass.: Harvard University Press, 1957), vol. 2, pp. 437–85.

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Pasteur's work on fermentation permanently shifted his own research toward the study of microorganisms. He showed that the "infinitely small" organisms are not only biologically interesting but also of practical importance in human activities (as in the production of wine, beer, vinegar, and cheese) and that the scientific study of microbes can be used to address both practical problems and biological quandaries. Pasteur's attempts to contribute to the solution of practical problems in the agricultural-industrial world of nineteenth-century France initiated a long tradition of industrially, and later more specifically biochemically, oriented studies of the metabolic processes performed by microorganisms.

The role of microbes in fermentation also offered a compelling analogy for the study of human and animal disease at a time when diseases were often seen as kinds of fermentation or putrefaction. The demonstration that different microbial organisms were involved in different types of fermentation helped to establish a notion of specificity that later played an important role in the debates on the germ theory of disease. Finally, in the course of his research on fermentation, Pasteur developed a number of procedures and techniques for the isolation and purification of microbes that were to prove central in later research. This experimental manipulation of microbial organisms moved the study of microorganisms away from natural-historical observation and into the forefront of experimental biology.

The difference between life and inanimate nature, and the associated philosophical, religious, and political issues, also figured in Pasteur's debate with Félix Archimède Pouchet (1800–1872) on spontaneous generation.

Debates about spontaneous generation punctuated nineteenth-century biology. As John Farley has argued, all these debates – whether opposing Georges Cuvier and Etienne Geoffroy Saint-Hilaire to Jean-Baptiste Lamarck, or the Naturphilosophen to the Physicalists, or H. Charlton Bastian to John Tyndall – were religious and political as well as biological, though their significance changed in the course of the century and was different in the different countries. In Germany, the idea of spontaneous generation was associated with the Naturphilosophen who posited a unifying vital force suffusing all of nature. When, in the mid-nineteenth century, the vitalism of the Naturphilosophen was rejected by physicalists such as Hermann von Helmholtz, Emil du Bois Raymond, Carl Ludwig, and Ernst Brücke, the idea of spontaneous generation lost popularity, only to be invoked again by the materialist and unitarian evolutionists (e.g., Ernst Haeckel) who believed spontaneous generation to be a prerequisite for a fully materialist explanation of life. In France, the view that organisms (or their precursors) can develop spontaneously, from either organic or inorganic materials, had been associated with materialist evolutionary philosophies of nature at the beginning of the nineteenth century, and, by extension, it also came to be associated with antireligious and republican views. Thus, the doctrine of spontaneous generation was linked with political ideas, materialism, and atheism, forces

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from which Pasteur – a lifelong defender of the forces of order – might have wished to dissociate himself. At the same time, Pasteur's opposition to spontaneous generation was directly related to his theory of fermentation. Not only was heterogenesis "politically suspect," but it also failed to accord with Pasteur's insistence that microorganisms are the cause (and not the result) of fermentation, or with his notion of microbial specificity, which rested on a belief that "like begets like."[10]

Much has been written about the ingenuity and, recently, about the logical insufficiency and circularity of Pasteur's experiments designed to show that microbial life is generated not from the interaction between organic matter and air (or oxygen) but from the germination and reproduction of microorganisms always present in the air or contaminating the experimental apparatus (e.g., the mercury bath of Pouchet). Thus, some historians have celebrated the inventiveness of Pasteur's swan-necked retorts (which allowed air but not microorganisms to enter the vessels in which various fluids were kept sterile), the discipline of his experimental procedures, and the logical rigor of his demonstrations, all of which enabled him to show that germs suspended in the air, and not some invisible "vital" forces, were responsible for the introduction of microscopic life into organic infusions.[11] Other historians dispute the decisive nature of these experiments by showing that Pasteur never replicated Pouchet's experiments exactly, and that the preconceived ideas of Pasteur and his allies were more important than experimental evidence. They argue that Pasteur, like other critics of spontaneous generation, could never prove that spontaneous generation is impossible; at best, he could show only that individual experiments purporting to prove the phenomenon were flawed. In his attempts to disprove spontaneous generation, Pasteur in effect is alleged to have presupposed its impossibility and used it as a criterion for evaluating the success or failure of individual experiments.[12] Based as they are on two opposing philosophies of science, these two kinds of accounts cannot be fully reconciled. But the impossibility of achieving absolute "logical" sufficiency in an experimental demonstration is no warrant for according it a subordinate "historical" role in the creation of scientific consensus, or for reducing Pasteur's experimental skill to that of a "prestidigitator who could produce the desired results 'at will.'"[13]

[10] Farley, *Spontaneous Generation Controversy from Descartes to Oparin*; John Farley and Gerald L. Geison, "Science, Politics and Spontaneous Generation in Nineteenth Century France: The Pasteur-Pouchet Debate," *Bulletin of the History of Medicine*, 48 (1974), 161–98; Geison, *Private Science of Louis Pasteur*, chap. 5, pp. 110–42; Glenn Vandervliet, *Microbiology and the Spontaneous Generation Debate during the 1870's* (Lawrence, Kans.: Coronado Press, 1971).

[11] See, for example, N. Roll-Hansen, "Experimental Method and Spontaneous Generation: The Controversy between Pasteur and Pouchet, 1859–64," *Journal of the History of Medicine and Allied Sciences*, 34 (1979), 273–92.

[12] Farley, *Spontaneous Generation Controversy from Descartes to Oparin*; Geison, *Private Science of Louis Pasteur*.

[13] Geison, *Private Science of Louis Pasteur*, p. 133. See also Bruno Latour, "Le théâtre de la preuve," in Salamon-Bayet, *Pasteur et la révolution pastorienne*, pp. 335–84.

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Pasteur's demonstrations ended the debates about spontaneous generation in France in the 1860s, but such debates continued elsewhere until the 1880s. The rejection of the possibility of spontaneous generation, just like the principle of specificity established in fermentation studies and through the classification studies of Ferdinand Cohn, created a biological context in which what we now consider "the bacteriological revolution" could take place.

## THE BACTERIOLOGICAL REVOLUTION

Although the "bacteriological revolution" is most commonly associated with the work of Louis Pasteur, Robert Koch (1843–1910), and Joseph Lister (1827–1912), it was a complicated and lengthy process that involved changes in ideas of disease causation and specificity,[14] a new biological understanding of microorganisms, and radical innovations in the manner in which microorganisms (and diseases) were to be studied in the laboratory. Even if the confluence of innovations in these three areas is most clearly found in the work of Pasteur and Koch, various elements were being articulated by a number of researchers from the middle of the century.

What is today understood as "the" germ theory of disease – the notion that specific microorganisms cause specific diseases – was far from a single unified theory. A variety of such theories of infection and contagion were being articulated by scientists as well as practicing physicians, veterinarians, and epidemiologists in the middle decades of the nineteenth century.[15] The germ theory was also formulated differently by the Pastorians and by the "German school" of Robert Koch. Andrew Mendelsohn has argued that Pasteur's contribution to medicine was driven by a distinct set of beliefs about the nature of life and the place of microbes in the general economy of nature, including their role in disease. Pasteur, who had studied beneficial uses of microbes in the agricultural industries of France, saw microbial life in more physiological and ecological terms, as more pliable and subject to environmental modification. Koch, a physician who had served in the Franco-Prussian War and later surrounded himself with other army doctors, had a strictly medical – and, one might say, almost military – view of bacteria

[14] K. Codell Carter, "The Development of Pasteur's Concept of Disease Causation and the Emergence of Specific Causes in Nineteenth Century Medicine," *Bulletin of the History of Medicine*, 65 (1991), 528–48.

[15] The history of these theories in the United Kingdom is traced in Michael Worboys, *Spreading Germs: Disease Theories and Medical Practice in Britain, 1865–1900* (Cambridge: Cambridge University Press, 2000). See also Nancy J. Tomes and John Harley Warner, "Introduction to Special Issue on Rethinking the Reception of the Germ Theory of Disease: Comparative Perspectives," *Journal of the History of Medicine and Allied Sciences*, 52 (1997), 7–16; Christopher Lawrence and Richard Dixey, "Practicing on Principle: Joseph Lister and the Germ Theories of Disease," in *Medical Theory, Surgical Practice: Studies in the History of Surgery*, ed. Christopher Lawrence (London: Routledge, 1992), pp. 153–215.

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as deadly invaders to be eradicated.[16] These differences clearly affected the ways in which their respective research programs developed. Pasteur quickly turned his attention to attenuation of virulence and immunity, while Koch identified the invisible enemies in all nooks and crannies in order to direct their eradication. Nevertheless, the two founders of medical bacteriology did share a belief that infectious diseases were specific entities and that specific (though, according to Pasteur, physiologically complex and environmentally labile) microorganisms were causally implicated in their origin and spread.

By the mid-nineteenth century, clinical and pathological studies had led to the delineation of individual specific diseases such as typhus and typhoid, tuberculosis, and diphtheria. But the differentiation of diseases in terms of their pathology or symptomatology was quite separate from either the etiological classification of diseases in terms of their causes or the epidemiological grouping of diseases into classes that originated from similar sources or spread in a similar manner. Thus, while the specific identity of many individual diseases had been elaborated prior to (and independently of) their association with specific microorganisms, they were not seen as belonging to a single class, nor were they all believed to result from a unique set of causes.[17] In the mid-nineteenth century, theories of how the contagious and/or miasmatic diseases are transmitted often involved the invocation of some particulate, usually organic and occasionally even living, matter (variously described as a poison, a germ of disease, a microzyme, a fungus, a bacillus, or a vibrio) that was either generated in particular locations (for example, from decomposing organic matter), transmitted directly from person to person, or passed on indirectly via water or "fomites." Many of the contagious diseases were seen as forms of putrefaction and fermentation, and given the popularity of the chemical theory of fermentation, the pathological process was believed to be initiated by some chemical change affecting organic matter that made it "zymotic" and allowed for the transmission of its fermentative abilities to new sufferers.[18] This analogy between disease process and fermentation had led Pasteur to claim already in 1859, before he actually turned his attention to the study of infectious diseases, that contagious diseases are likely "to owe

[16] John Andrew Mendelsohn, "Cultures of Bacteriology: Formation and Transformation of a Science in France and Germany, 1870–1914" (PhD diss., Princeton University, 1996); K. Codell Carter, "The Koch-Pasteur Debate on Establishing the Cause of Anthrax," *Bulletin of the History of Medicine*, 62 (1988), 42–57; Henri H. Mollaret, "Contribution à la connaissance des relations entre Koch et Pasteur," *Schriftenreihe für Geschichte der Naturwissenschaften, Technik und Medizin*, 20 (1983), 57–65.

[17] Margaret Pelling, *Cholera, Fever and English Medicine, 1825–1865* (Oxford: Oxford University Press, 1978); Margaret Pelling, "Contagion/Germ Theory/Specificity," in *Companion Encyclopedia to the History of Medicine*, ed. William Bynum and Roy Porter, 2 vols. (London: Routledge, 1997), vol. 1, pp. 309–33; Owsei Temkin, "A Historical Analysis of the Concept of Infection," in Owsei Temkin, *The Double Face of Janus and Other Essays in the History of Medicine* (Baltimore: Johns Hopkins University Press, 1977), pp. 456–71.

[18] Christopher Hamlin, *The Science of Impurity: Water Analysis in Nineteenth Century Britain* (Berkeley: University of California Press, 1990).

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their existence to similar causes"[19] as specific fermentations. The association of fermentation and putrefaction with the action of "germs" in suppurating wounds was also taken up in the initial work of Joseph Lister, who promoted antiseptic methods in surgery as means of preventing inflammation.[20]

The idea that living organisms are causally implicated in infectious diseases was also supported by experimental studies and microscopic observations conducted by epidemiologists, veterinary doctors, anatomical pathologists, and physicians. One might mention here the work on anthrax by the French veterinarian Casimir Davaine (1812–1882), the inoculation experiments on cattle plague performed in England by John Scott Burdon Sanderson (1828–1905),[21] the research on the fungal etiology of muscardine by the Italian civil servant Augusto Bassi (1773–1856), and the claims of the British epidemiologist William Budd (1811–1880), who reported the presence of fungi in the discharges of cholera victims or water contaminated with the choleraic excreta.[22]

It is somewhat misleading to place all these observations and claims side by side and discuss them as precursors of the germ theory. They were made in a variety of independent contexts, by researchers working in different fields, using a variety of different terminologies to describe the *contagium vivum*, and differing in their understanding of how the living "contagia" they observed caused or transmitted disease. The historical interest of these early observations and inoculation experiments on animals lies perhaps not so much in how much they anticipated the later demonstrations of the bacterial etiology of any specific disease, as in their functioning as catalysts for discussions of what kind of evidence would be considered necessary to establish a causal relation between a microorganism and a disease.

The kinds of conditions required to demonstrate that live microorganisms cause diseases had been explicitly spelled out already in 1840 by the German pathologist Jacob Henle (1809–1885), who argued that in order to show that a given agent is causally implicated in disease it must be shown to be always present in the diseased organism, and it must be isolated and tested in its isolated state to see whether it can reproduce the disease. In 1872, a similar set of criteria for establishing a causal relation was articulated by another anatomical pathologist, Edwin Klebs (1834–1913), in his study of gunshot wounds.[23] Klebs's formulation of the ideal experimental strategy, as well as Koch's later refinement and use of this strategy in his studies of anthrax, wound

[19] Quoted in Nancy Tomes, *The Gospel of Germs: Men, Women, and the Microbe in American Life* (Cambridge, Mass.: Harvard University Press, 1998), p. 31.

[20] Lawrence and Dixey, *Medical Theory, Surgical Practice.*

[21] Terrie M. Romano, "The Cattle Plague of 1865 and the Reception of 'The Germ Theory' in Mid-Victorian Britain," *Journal of the History of Medicine and the Allied Sciences*, 52 (1997), 51–80.

[22] K. Codell Carter, "Ignaz Semmelweis, Carl Mayrhoffer, and the Rise of Germ Theory," *Medical History*, 29 (1985), 33–53.

[23] K. Codell Carter, "Koch's Postulates in Relation to the Work of Jacob Henle and Edwin Klebs," *Medical History*, 29 (1985), 353–74.

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infection, cholera, and tuberculosis, took place in the context of debates among medical researchers, physicians, veterinarians, hygienists, and public health workers as to whether microorganisms were not just "innocent" contaminants, accompanying rather than causing disease, whether they followed rather than initiated the disease process, or whether they could be generated de novo or change from saprophytic to pathological under certain as yet unknown environmental conditions. How important are microbes in the complicated causal nexus that leads to a person becoming sick? Is the presence of microbes a sufficient cause or only a necessary precondition? Why do epidemics come and go? If microbes are everywhere, why doesn't everybody become sick? The elaboration of what later came to be known as "Koch's postulates" thus came in a context of controversies between those skeptical of the germ theory and proponents seeking to demonstrate the causal link between microbes and disease. The issue was not only whether (specific) germs cause (specific) diseases, it also concerned the nature of the causal connection and its practical – medical and epidemiological – ramifications.

The reception of Koch's famous 1876 demonstration that anthrax bacilli produce heat-resistant spores has to be seen in this context. Koch's presentation of his experiments in the Breslau laboratories of Ferdinand Cohn generated enthusiasm not only because it posited a credible mode of transmission of anthrax and explained the known epidemiological facts about this disease, but also because Koch's new experimental techniques advanced the researchers' ability to manipulate microorganisms in the laboratory and suggested new ways of conducting etiological investigations.[24]

Koch's commitment to the idea of the specificity, stability, and distinctiveness of bacterial species played a key role in his research program. This idea was a central theoretical foundation for his etiological investigations that associated specific and stable bacterial species with specific diseases. It served as an important methodological regulator, one of the criteria for deciding whether a culture has been kept pure, and it assured the medical and epidemiological relevance of Koch's studies of the etiology of infectious diseases. Both the medical and the biological ramifications of Koch's position were brought out in his disputes with botanists such as von Nägeli and his student Hans Buchner (1850–1902), and hygienists such as Max von Pettenkoffer (1818–1901), who argued that microorganisms were not the most significant causes of disease because under different local conditions the same microorganism (or schizomycete) could be either pathogenic (like the anthrax bacillus) or saprophytic (like the hay bacillus). Koch returned to this issue repeatedly – in his study of wound infections, in his further papers on anthrax, and, later, in his work on tuberculosis and cholera. Aligning himself with the "Linnaean" botanist Cohn, Koch cited his own experimental results as evidence that

[24] On Koch, see Thomas Brock, *Robert Koch: A Life in Medicine and Bacteriology* (Madison, Wis.: Science Tech, 1988); Mendelsohn, "Cultures of Bacteriology"; Mazumdar, *Species and Specificity.*

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bacterial species bred true and did not undergo any significant morphological or physiological variation. At the same time, he attributed contrary observations or claims of variability to unrecognized errors in the experimental procedures of his opponents such as contamination, lack of purity, or the use of mixed cultures.[25]

Koch's insistence on specificity, as well as a series of technical innovations such as the development of pure culture methods, solid and selective culture media, differential staining, and microphotography, led to a long series of demonstrations of the specific etiologies of a number of infectious diseases. During the decade following Koch's famous 1882 demonstration that the tubercle bacillus was the bacterial agent responsible for tuberculosis, Koch and his students and collaborators identified the specific microorganisms responsible for diphtheria (by Loeffler in 1884), glanders (Loeffler in 1882), typhoid (Gaffky in 1884), cholera (Koch in 1884), and tetanus (Kitasato in 1889). Others, using Koch's methods of isolation, pure culture, and inoculation, identified and described the specific organisms responsible for dysentery, gonorrhea, meningitis, and pneumonia, among other diseases. This simple list does not do justice to the complexity of the research and technical ingenuity required to isolate and identify the various bacterial strains and to transmit the disease to experimental animals reproducing specific disease symptoms. Many of the claims made by the students of bacteria and disease in this early heroic period were disputed at the time, some bacterial etiologies were later rejected or drastically modified, and many of the "proofs" were completed gradually by a number of bacteriologists, pathologists, or physicians working in laboratories around the world. In some cases, the difficulty of transmitting the disease to an experimental animal (cholera is the most famous example) or of reproducing the specific pathology – and thus the impossibility of fulfilling all of Koch's postulates – meant that the debates on the causative role of particular bacteria continued for decades.[26] These etiological debates about infectious diseases continued through the 1920s and 1930s, when attempts to identify specific pathogenic viruses (known then as "filterable viruses") were subjects of lively dispute.[27]

While Koch and his students in Germany were busy with studies of etiology, Pasteur and his collaborators turned their attention to the attenuation

[25] Olga Amsterdamska, "Medical and Biological Constraints: Early Research on Variation in Bacteriology," *Social Studies of Science*, 17 (1987), 657–87. See also Mazumdar, *Species and Specificity*; Mendelsohn, "Cultures of Bacteriology."

[26] See, for example, William Coleman, "Koch's Comma Bacillus: The First Year," *Bulletin of the History of Medicine*, 61 (1987), 315–42; Ilana Löwy, "From Guinea Pigs to Man: The Development of Haffkine's Anticholera Vaccine," *Journal of the History of Medicine and the Allied Sciences*, 47 (1992), 270–309.

[27] On debates about the etiology of one such disease, influenza, see Ton Van Helvoort, "A Bacteriological Paradigm in Influenza Research in the First Half of the Twentieth Century," *History and Philosophy of the Life Sciences*, 15 (1993), 3–21. On the history of virology more generally, see S. S. Hughes, *The Virus: A History of the Concept* (London: Heinemann, 1977); A. P. Waterson and L. Wilkinson, *An Introduction to the History of Virology* (Cambridge: Cambridge University Press, 1978).

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of microbes and the development of specific vaccines. The initial success in producing a vaccine against chicken cholera (1880) was followed by a vaccine against anthrax, which was tested in famous trials in Pouilly-le-Fort in 1881, and a vaccine to treat rabies, first used on humans in 1885.[28] Pasteur's research on immunity and attenuation inaugurated a variety of efforts to develop vaccines and attempts to explain the phenomenon of immunity to infection. By the mid-1890s, immunological research also led to the development of new techniques of bacterial identification and diagnosis (such as the Widal reaction for the diagnosis of typhoid, based on the 1896 work of Max Gruber and Herbert Durham on the clumping or agglutination of typhoid bacilli in immune serum). In the following decades, the development of immunological theories and techniques was closely linked to developments in medical bacteriology, and a strict separation of the two areas prior to World War II is hardly possible.

By 1900, despite the development of Pastorian vaccines and a serum treatment for diphtheria (by Behring in 1890), the hopes that bacteriological discoveries would quickly lead to efficient therapies had been moderated. The germ theory, however, penetrated deeply not only into the physicians' and scientists' understanding of diseases but also into the public consciousness and daily sanitary practices.[29] The germ theory was a prime example of what laboratory research in medicine could accomplish, even if some epidemiologists or physicians had doubts about particular etiologies, the sufficiency of bacteriological explanation, or the relevance of specific laboratory findings for the management of infectious diseases.[30]

## INSTITUTIONALIZATION OF BACTERIOLOGY

The bacteriological revolution profoundly altered the organization of medical research and the manner in which studies of microorganisms were institutionalized. In the course of a few decades, microorganisms became the central preoccupation of a variety of medical researchers and physicians working in hospitals, public health laboratories, and medical schools and faculties. This

[28] Geison, *Private Science of Louis Pasteur.*

[29] See Tomes, *Gospel of Germs.*

[30] On some aspects of opposition to bacteriology, see Russell C. Maulitz, "'Physician vs. Bacteriologist': The Ideology of Science in Clinical Medicine," in *The Therapeutic Revolution: Essays in the Social History of American Medicine*, ed. Morris J. Vogel and Charles E. Rosenberg (Philadelphia: University of Pennsylvania Press, 1979), pp. 91–107; Michael Worboys, "Treatments for Pneumonia in Britain, 1910–1940," in *Medicine and Change: Historical and Sociology Studies of Medical Innovation*, ed. Ilana Löwy et al. (Paris: Libbey, 1993); Anne Hardy, "On the Cusp: Epidemiology and Bacteriology at the Local Government Board, 1890–1905," *Medical History*, 42 (1998), 328–46; Anna Greenwood, "Lawson Tait and Opposition to Germ Theory: Defining Science in Surgical Practice," *Journal of the History of Medicine and the Allied Sciences*, 53 (1998), 99–131; Nancy J. Tomes, "American Attitudes towards the Germ Theory of Disease: Phyllis Allen Richmond Revisited," *Journal of the History of Medicine and the Allied Sciences*, 52 (1997), 17–50.

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process of institutionalization proceeded somewhat differently in various countries: In Germany, for example, bacteriologists tended to be appointed to chairs of hygiene, many of which were created in the wake of Koch's discoveries and occupied by his students and collaborators.[31] In the United States and the United Kingdom, bacteriology was more often regarded as a specialization within pathology. Independent university departments of bacteriology were established in the United States in the first decades of the twentieth century. In the United Kingdom, this process of institutionalization proceeded more slowly. Pathological and bacteriological research was conducted mainly in teaching hospitals and a few of the newer urban universities, and laboratory diagnostic work had relatively low status in a system dominated by elite clinicians. Moreover, bacteriological laboratories in British teaching hospitals and universities were often swamped with routine diagnostic work performed at the request of physicians or for public health authorities.[32] A similar domination of clinicians in the teaching hospitals of Paris also tended to marginalize laboratory research, and the few chairs of microbiology established before World War I "were often 'waiting chairs', occupied by physicians who aspired to a clinical chair and were not interested in microbiological research."[33]

But even if the existing institutional and disciplinary structures were relatively slow to make room for bacteriology as an independent discipline, the bacteriological revolution was followed by a significant innovation in the organization of (medical) research: the establishment of major research institutes devoted to medical, and especially bacteriological, research. In 1888, following Pasteur's successful treatment of rabies, a public collection and a gift from the French government provided funds for the establishment of the Pasteur Institute in Paris. Three years later, a government-funded Institute for Infectious Diseases was opened for Koch in Berlin. There followed the Lister Institute in London (1893), the Institute for Experimental Medicine in Saint Petersburg (1892), the Serotherapeutic Institute in Vienna, the Institute for Experimental Therapy in Frankfurt am Main (1899), and the Rockefeller Institute for Medical Research in New York (1902). All of these institutes shared a commitment to laboratory studies of disease, although they differed

[31] Paul Weindling, "Scientific Elites and Laboratory Organisation in *fin de siècle* Paris and Berlin: The Pasteur Institute and Robert Koch's Institute for Infectious Diseases Compared," in *The Laboratory Revolution in Medicine*, ed. Andrew Cunningham and Perry Williams (Cambridge: Cambridge University Press, 1992), pp. 170–88.

[32] Keith Vernon, "Pus, Sewage, Beer, and Milk: Microbiology in Britain, 1870–1940," *History of Science*, 28 (1990), 289–323; Patricia Gossel, "The Emergence of American Bacteriology, 1875–1900" (PhD diss., Johns Hopkins University, 1989). See also Paul F. Clark, *Pioneer Microbiologists of America* (Madison: University of Wisconsin Press, 1961); Russell Maulitz, "Pathologists, Clinicians, and the Role of Pathophysiology," in *Physiology in the American Context, 1850–1940*, ed. Gerald Geison (Bethesda, Md.: American Physiological Society, 1987), pp. 209–35.

[33] Ilana Löwy, "On Hybridizations, Networks, and New Disciplines: The Pasteur Institute and the Development of Microbiology in France," *Studies in the History and Philosophy of Science*, 25 (1994), 655–88, at p. 670.

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in the scope of the research that was envisaged and performed (for example, the Rockefeller Institute concentrated on experimental medicine broadly understood, while the Pasteur Institute focused on the study of microorganisms, including their nonmedical facets, and Koch's Institute for Infectious Diseases was focused on medical bacteriology). They were also funded differently: by government support, public collections and donations, industrial or philanthropic funding, or by generating additional income from the production of biological materials such as the antidiphtheria serum. Thus they differed also in the degree of their financial and institutional independence, in the nature of their relations to the research of a senior "founding" figure, and in their internal organizational structures. They provided a setting for advanced, often collaborative, research and training for future researchers, public health workers, laboratory workers, and clinicians. Of special importance for the dissemination of medical bacteriology were the month-long courses offered by Koch and his collaborators, initially at the Hygienic Institute at the Berlin University and later at the Institute for Infectious Diseases, which were attended not only by hundreds of German physicians but also by large numbers of foreign visitors. The courses emphasized practical experience and the learning of laboratory skills and methods, and they provided students with an opportunity to acquaint themselves with the organization of bacteriological research and teaching. When the Pasteur Institute opened its doors in 1888, Koch's course served as a model for the course offered there by Emile Roux (1853–1933), which soon attracted a similarly international audience.[34]

Perhaps the most obvious, though still not fully explored, aspect of the bacteriological revolution was that it moved the study of microorganisms from a marginal subject of research for a small group of botanists and zoologists to the center of attention among a wide group of researchers working in a variety of research fields. Although the medical and public health settings were the most dominant and produced the most celebrated achievements, around the turn of the century microorganisms continued to be studied by some academic biologists and were becoming important in agricultural research settings – in veterinary medicine, plant pathology, and soil studies – and in a few laboratories exploring the uses of these organisms in the fermentation and processing industries. Although in the first half of the twentieth century microbiology continued to be both organizationally and intellectually

[34] Weindling, "Scientific Elites and Laboratory Organisation in *fin de siècle* Paris and Berlin"; Mendelsohn, "Cultures of Bacteriology"; Gossell, "Emergence of American Bacteriology"; Löwy, "Hybridizations, Networks, and New Disciplines." See also Henriette Chick, Margaret Hume, and Marjorie Macfarlane, *War on Disease: A History of the Lister Institute* (London: Deutsch, 1972); George W. Corner, *The History of the Rockefeller Institute for Medical Research* (New York: Rockefeller University Press, 1964); Michel Morange, ed., *L'Institut Pasteur*: *Contributions à son histoire* (Paris: La Découverte, 1991).

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scattered, substantive methodological and institutional links existed between researchers working in a variety of settings.

The dispersed state of the field was often criticized, and some bacteriologists argued for the unity of the field as a means of establishing a degree of professional and disciplinary autonomy. Such unification was, for example, an explicit goal of the Society of American Bacteriologists, founded in 1899. Its founders wanted to "emphasize the position of bacteriology as one of the biological sciences" and to "bring together workers interested in the various branches into which bacteriology . . . [was] . . . ramifying."[35] In addition to publishing the generalist *American Journal of Bacteriology* (founded in 1916), the society also sponsored work aiming at the standardization of methods of studying bacteria and the establishment of a uniform system of bacterial classification. Such standardization was not only of practical importance for the development of efficient communication in the field, it was also meant to enhance the disciplinary status of bacteriology as a biological field and not just a "handmaiden" of medicine, pathology, or agricultural research.[36]

## BETWEEN PROTOZOOLOGY AND TROPICAL DISEASES

It is extremely difficult to locate the position of studies of microorganisms in the context of early twentieth-century academic biology. Studies of protozoa in Germany around the turn of the century illustrate this complexity. Protozoa came to constitute the research object of the specialized discipline of protozoology, with its own research institutes, a journal (*Archiv für Protistenkunde*, founded by Richard Hertwig in 1902), and textbooks. Protozoa and other unicellular organisms also served as models and research tools in cytological research aiming to unravel the physiology and morphology of the living cell at a time when experimental cell research was regarded in Germany as the unifying ground for all biology.[37] At the same time, protozoa were studied as agents of disease, especially in colonial or tropical medicine, and much of the institutional support for their study was clearly linked to this last interest (as witnessed by the establishment of a protozoological division in Koch's Institute for Infectious Diseases, and of research institutes such as the Hamburg Institute for Naval and Tropical Diseases in 1901). The

[35] H. Conn, "Professor Herbert William Conn and the Founding of the Society," *Bacteriological Reviews*, 12 (1948), 275–96, at p. 287.

[36] Gossel, "Emergence of American Bacteriology"; Patricia Gossel, "The Need for Standard Methods: The Case of American Bacteriology," in *The Right Tools for the Job: At Work in Twentieth Century Life Sciences*, ed. Adele H. Clarke and Joan H. Fujimura (Princeton, N.J.: Princeton University Press), pp. 287–311.

[37] Jacobs, "From Unit to Unity"; Marsha Richmond, "Protozoa as Precursors of Metazoa: German Cell Theory and Its Critics at the Turn of the Century," *Journal of the History of Biology*, 22 (1989), 243–76.

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research conducted by protozoologists often fell into more than one of these categories. Thus, a protozoologist such as Fritz Schaudinn, remembered in medicine for his identification of *Treponema pallidum* (1905) – which was not a protozoan but a spirochete – as the etiological agent of syphilis and for his work on blood parasites (malarial plasmodia and trypanosomes), was also deeply involved in debates about theories of protozoan reproduction and life cycles. A similar combination of physiological and cytological studies of protozoa with investigations of other pathological microorganisms – bacteria, rickettsia, and viruses – characterized the work of Schaudinn's coworker and successor as the director of the Hamburg Institute of Tropical Diseases, Stanislaus von Prowazek.

But it was not in the academic biological settings but in the context of colonial and military medicine that the most important protozoan parasites were initially identified and their life cycles and modes of spreading through animal vectors established. Thus, in 1880 the French Army doctor Alphonse Laveran identified merezoites in the blood of malaria victims; in 1898, Ronald Ross of the Indian Medical Service described the role of the mosquito in the transmission of malaria and traced the life cycle of the parasite in the mosquito; in 1895, David Bruce of the Royal Army Medical Corps studied trypanosomes and the role of the tsetse fly in the transmission of the cattle disease nagana; and in 1903 W. B. Leischman of the Royal Army Medical Corps elucidated the etiology of kala-azar. Patrick Manson's definition of tropical diseases as those caused by protozoa and transmitted through a vector meant that although many diseases affecting the populations of Asia and Africa were not protozoan (nor necessarily spread by an animal vector), the close connection between tropical (colonial) medicine and protozoology continued in the first half of the twentieth century in the research performed in settings such as the Liverpool School of Tropical Medicine (established in 1899) and the London School of Tropical Medicine (established in 1899; after 1927, thanks to a grant from the Rockefeller Foundation, it became the London School of Hygiene and Tropical Medicine).[38] Still, by the 1920s and 1930s, some of the research conducted in these locales in Britain or in analogous laboratories and institutions in France or Germany was also only distantly related to specific medical problems and addressed issues such as protozoan morphology, life cycles, nutrition and biochemistry, or genetics, as for example in the work on nutrition of protozoa and growth factors conducted by André Lwoff (1902–1994) at Felix Mesnil's laboratory at the Pasteur Institute.

[38] Michael Worboys, "Tropical Diseases," in Bynum and Porter, *Companion Encyclopedia to the History of Medicine*, vol. 1, pp. 512–35; Michael Worboys, "The Emergence of Tropical Medicine," in *Perspectives on the Emergence of Scientific Disciplines*, ed. Gerald Lemaine et al. (The Hague: Mouton, 1976), pp. 76–98.

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## BACTERIOLOGY BETWEEN BOTANY, CHEMISTRY, AND AGRICULTURE

A similar intersection of various institutional and intellectual contexts is apparent in the development of the tradition of microbial studies initiated by Sergei Winogradsky (1856–1953) and, independently, by Martinus Willem Beijerinck (1851–1931). Both Beijerinck and Winogradsky were trained as botanists and learned microbiological techniques in the 1880s at the botanical laboratory of Anton de Bary at the University of Strasbourg. De Bary, a rather typical representative of the new botany, investigated morphological structures, physiological processes, and the development of cryptogams, especially fungi. He was also interested in antagonistic and symbiotic relations among organisms. Beijerinck's and Winogradsky's more ecological approaches to the study of microorganisms were rooted in this background in plant morphology and physiology, reflected an interest in more fundamental biological questions concerning growth, heredity, and physiology, and emphasized interactions between an organism and its environment and among different microorganisms living in the same environment. This ecological perspective is evident not only in their critiques of standard (medical) bacteriological techniques but also in their methodologies: the use of elective, enrichment, or accumulation culture methods in which the isolation of bacteria was made possible by adjusting the chemical composition of the culture medium so as to favor the growth of a particular physiological type of microorganism. Using these methods, Winogradsky, at that time working in Zurich, identified sulfur and iron bacteria (1889) and in the 1890s studied groups of soil bacteria responsible for nitrification – the fixing of atmospheric nitrogen and the transformation of nitrite into nitrate. Beijerinck emphasized the fact that accumulation cultures provide an opportunity to study bacterial variation and, by simulating environmental conditions occurring in nature (rather than using the "artificial" media of medical bacteriology), they made it possible to study microbial ecology.

Winogradsky's studies of the physiology and biochemistry of autotrophic bacteria clearly had general biological and biochemical implications, but the issues he raised were later taken up predominantly not in academic biology, but in agricultural research settings where funding was more abundant and where research in soil microbiology was legitimized by its relevance to agriculture.[39]

Beijerinck's research program was even more obviously and explicitly linked to the current problematics in botany and biology. He studied an enormous variety of microorganisms (including yeast, algae, lichens, and

[39] S. A. Waksman, *Sergei Winogradsky, His Life and Work* (New Brunswick, N.J.: Rutgers University Press, 1953).

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viruses) and was interested in systematics as well as microbial physiology and variability. Questions of growth, heredity, and variation gave unity to all of Beijerinck's work. There can be no doubt that Beijerinck – who is remembered for his identification of nitrogen-fixing bacteria in the root nodules of leguminous plants, his demonstration that the tobacco mosaic disease is caused by a *contagium vivum fluidum* (a virus), and his work on microbial variation – pursued research questions that were more relevant to academic biologists than to agricultural practice or the industrial uses of microorganisms. And yet Beijerinck taught at an agricultural school and worked in an industrial laboratory before returning to the "more academic" though still practice-oriented setting of the polytechnical university at Delft.[40]

In the following decades, agricultural research stations and agricultural colleges in the United States and the United Kingdom became important settings for the study of soil microorganisms, and at least some of this research – the history of which still remains to be written – adopted the ecological and physiological perspectives of Beijerinck and Winogradsky. These perspectives are clearly apparent in the research on soil microbiology conducted at the New Jersey Agricultural Experiment Station at Rutgers University by Jacob G. Lipman and then by Selman Waksman and their students.[41] By the 1940s, this research was redirected toward more medical (and industrial) goals as Waksman's laboratory turned to work on antibiotics.

Most of the microbiological research in agricultural research settings – such as experimental stations, agricultural colleges, and the Federal Bureaus of Plant Research and Animal Industry of the U. S. Department of Agriculture – was focused not on soil microbiology but on plant and animal diseases. In the United States, such studies were particularly well funded, though many of the scientists engaged in studies of plant and animal diseases had to battle with the competing pressures of providing direct service to farmers and of conducting more fundamental research.[42] Simplifying grossly, one could say that studies of animal diseases were closely related to other aspects of medical bacteriology and parasitology (as in the work of Theobald Smith), dairy bacteriology was closely linked to sanitary or public health bacteriology

[40] Bert Theunissen, "The Beginnings of the 'Delft Tradition' Revisited: Martinus W. Beijerinck and the Genetics of Microorganisms," *Journal of the History of Biology*, 29 (1996), 197–228. See also G. van Iterson, L. E. den Dooren de Jong, and A. J. Kluyver, *Martinus Willem Beijerinck: His Life and Work* (orig., 1940; repr. Ann Arbor, Mich.: Science Tech, 1983).

[41] Jill E. Cooper, "From the Soil to Scientific Discovery: René Dubos and the Ecological Model for Microbial Investigation, 1924–1939," paper presented at the meeting of the History of Science Society, Atlanta, Georgia, November 1996.

[42] Charles E. Rosenberg, "Rationalization and Reality in Shaping American Agricultural Research, 1875–1914," in *The Sciences in the American Context: New Perspectives*, ed. Nathan Reingold (Washington, D.C.: Smithsonian Institution Press, 1979), pp. 143–63; Charles E. Rosenberg, "The Adams Act: Politics and the Cause of Scientific Research," in Charles E. Rosenberg, *No Other Gods: On Science and American Social Thought* (Baltimore: Johns Hopkins University Press, 1961); Margaret Rossiter, "The Organization of the Agricultural Sciences," in *The Organization of Knowledge in Modern America, 1860–1920*, ed. Alexandra Oleson and John Voss (Baltimore: Johns Hopkins University Press, 1979).

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(as in the work of Herbert William Conn), and the links with botany were more likely to be maintained by those who studied plant diseases (such as Thomas J. Burill and Erwin F. Smith).

## MICROBIOLOGY BETWEEN THE BREWING INDUSTRY AND (BIO)CHEMISTRY

Following the work of Pasteur, it might appear that the study of the chemical activities of microorganisms should have developed in connection with the various fermentation industries, yet only a few such enterprises employed microbiologists and relied on their expertise. Insofar as microorganisms were studied in these settings, the research was often conducted by chemists. The most prominent exception was the institute at the Carlsberg Brewery in Copenhagen, founded in 1876, where Emil Christian Hansen (1842–1909) studied pure yeast cultures and the problems that arose when wild yeasts contaminated the brews. In the first decades of the twentieth century, researchers at the Carlsberg Institute studied a variety of enzymatic processes in microorganisms.[43] Prior to the First World War, studies of fermentation were also conducted by Max Delbrück (1850–1919) at the Berlin Institut für Gärungsgewerbe[44] and by August Fernbach at the Pasteur Institute.

Awareness of the economic possibilities of fermentation research received a considerable boost in the years just prior to and during World War I, when another organic chemist (and future first president of the state of Israel), Chaim Weizmann (in collaboration with Fernbach), isolated a bacterium able to convert starch to acetone and butanol and on this basis developed the process for the production of these two compounds. Although the initial hope was that the butanol-acetone process would be important in the production of synthetic rubber, it achieved real economic success during the war, when it was employed for the production of explosives.

Robert Bud has linked the origins of biotechnology with the establishment of institutes for the study of fermentation technologies in agricultural and industrial schools and research institutions, and with the broadening of the potential scope of fermentation industries to processes other than brewing.[45] In the immediate post–World War I period, visions of how microorganisms could be used as small but efficient chemical factories were repeatedly articulated, and the term biotechnology was coined. But, quite apart from the importance of these visions for the actual development of biotechnology, the various research institutes serving the interests of the processing industries

[43] H. Holter and K. Max Møller, *The Carlsberg Laboratory 1876/1976* (Copenhagen: Rhodos, 1976).

[44] F. Hayduck, "Max Delbrück," *Berichte der Deutschen Chemischen Gesellschaft*, 53 (1920), 48–62.

[45] Robert Bud, "The Zymotechnic Roots of Biotechnology," *British Journal of the History of Science*, 25 (1992), 127–44; Robert Bud, *The Uses of Life: A History of Biotechnology* (Cambridge: Cambridge University Press, 1993).

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provided opportunities for more chemically oriented studies of microbial physiology.

Such research was, for example, performed by Sigurd Orla-Jensen, professor of fermentation physiology and agricultural chemistry (later renamed biotechnical chemistry) at the Copenhagen Polytechnic. Reflecting the chemical perspectives on microorganisms that dominated the research on fermentation practices, Orla-Jensen attempted to classify bacteria not in terms of their morphology or pathogenicity but in terms of their metabolism.

Another setting for research on the physiology of microorganisms where industrial orientation was combined with biochemical interests was the Delft Polytechnic, where Albert J. Kluyver (1888–1956), the successor to Beijerinck, managed to combine the training of chemical engineers in microbiology, and lively contacts with Dutch enterprises using microorganisms in industrial production, with an extensive research program on bacterial metabolism based on the idea of the unity of biochemistry. While grooming his students for positions in industry and allowing them to extend their chemical background in their work on microorganisms, Kluyver attempted to develop a general biochemical model of both fermentative and oxidative metabolism while remaining committed to the study of microbial physiology.[46]

In the 1920s and 1930s, studies of microbial physiology were thus linked not only to industrial concerns or biotechnological visions but also to the developments in academic biochemistry. In addition to working on chemical problems involving the use of microorganisms in the fermentation industries or in medicine, some biochemists used bacteria or yeast as convenient tools in their attempts to isolate and purify enzymes or to analyze the chemical steps involved in metabolic processes, while others became interested in microorganisms as biological systems with their own distinctive physiology.[47]

At the Dunn Institute of Biochemistry at Cambridge, for example, bacterial biochemistry was first studied by Harold Raistrick (1890–1971) and then by Judah Quastel (1899–1987) and Marjorie Stephenson (1885–1948). They studied bacterial enzymes, especially dehydrogenases, and developed the so-called resting cell method to study metabolic reactions in viable but not reproducing cells. Whereas Quastel's interests eventually reverted to those of mainstream biochemistry, where bacteria served primarily as convenient tools, Stephenson (and her students) continued to combine general biochemistry with microbial physiology by studying bacterial adaptation, nutritional requirements of different bacteria, and the physiological regulation of metabolic processes in a living cell. Stephenson objected to biochemists'

[46] Olga Amsterdamska, "Beneficent Microbes: The Delft School of Microbiology and Its Industrial Connections," in *Beijerinck and the Delft School of Microbiology*, ed. P. Bos and B. Theunissen (Delft: Delft University Press, 1995); A. F. Kamp, J. W. M. la Rivière, and W. Verhoeven, eds., *Albert Jan Kluyver: His Life and Work* (Amsterdam: North-Holland, 1959).

[47] Neil Morgan, "Pure Science and Applied Medicine: The Relationship between Bacteriology and Biochemistry in England after 1880," *Society for Social History of Medicine Bulletin*, 37 (1985), 46–9.

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treatment of microorganisms as "bags of enzymes."[48] Her work and that of her collaborators (especially John Yudkin) on the regulation of bacterial metabolism and the phenomenon of adaptation served as one of the sources for the later work of Jacques Monod (1910–1976) on diauxie, adaptive enzymes, and their genetic inhibition.

In the 1920s and 1930s, studies of bacterial physiology were also pursued by some medical bacteriologists. The nutritional requirements of various species of bacteria were often examined when bacteriologists tried to differentiate and classify bacterial strains or to develop diagnostic methods. A systematic program on bacterial nutrition was developed, for example, at the London Middlesex Hospital by Paul Fildes (1882–1971), working together with a number of biochemists.[49]

Studies of bacterial physiology in the 1930s and 1940s served as one of the impulses behind new attempts to counter the institutional and intellectual dispersal of the studies of microorganisms by promoting general microbiology. In 1930, a number of scientists working in a variety of settings and busy with practical as well as purely scientific problems founded a new journal, *Archiv für Mikrobiologie*, which was supposed to bring together research on microorganisms then appearing in botanical, biochemical, and morphological journals. Calls for general microbiology could also be heard in the United States and the United Kingdom, and they came in part from the same constituency. It was Cornelis Van Niel (1897–1985), Kluyver's most successful disciple and an expert on photosynthetic bacteria, who initiated courses in general microbiology at the Hopkins Marine Station of Stanford University. Van Niel promoted a broad concept of microbiology that would bring together biochemical and genetic, as well as environmental and evolutionary, investigations of microorganisms.[50] His students included such leaders of postwar general microbiology as Roger Stanier (1916–1982) and Michael Doudoroff (1911–1975). A similar aim to bring together microbiologists from differing backgrounds and establish a common ground between all forms of microbiology united the founders of the British Society for General Microbiology, which was formally inaugurated in 1945.

## GENETICS OF MICROORGANISMS AND MOLECULAR BIOLOGY

Just as research on bacterial physiology was initially an offshoot of a variety of studies in which industrial, medical, agricultural, and biochemical interests

48 Robert E. Kohler, "Innovation in Normal Science: Bacterial Physiology," *Isis*, 76 (1985), 162–81.

49 Robert E. Kohler, "Bacterial Physiology: The Medical Context," *Bulletin of the History of Medicine*, 59 (1985), 54–74.

50 Susan Spath, "C. B. van Niel and the Culture of Microbiology, 1920–1965" (PhD diss., University of California at Berkeley, 1999).

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crisscrossed and overlapped, so also were studies of microbial genetics initially a by-product of other investigations. Until the 1940s, genetic questions were not of primary concern in any of the contexts in which microorganisms were studied. Beijerinck's attempts to place the study of bacterial variation and growth in the context of De Vries's theory of mutation and to relate them to the enzymatic theory of heredity had not been followed up, even if researchers sometimes raised questions about the nature of bacterial inheritance. This situation changed profoundly in the 1940s when microorganisms became the preferred tools of biologists promoting and developing a new "molecular vision of life," a vision initially supported by philanthropic organizations such as the Rockefeller Foundation and one that received a tremendous boost in the postwar period when large-scale government funds began to flow into fundamental biological research.[51] A large number of microbiologists, as well as geneticists and biochemists who chose microorganisms as tools for their studies of the physicochemical basis of vital processes, participated in this interdisciplinary and international endeavor, which we now refer to as molecular biology.[52] Support from foundations, and then government, encouraged the development and deployment of new scientific instruments and techniques – ultracentrifuges, electrophoresis apparatus, electron microscopes, and radioactive isotope tracers – the use of which transformed all areas of microbiological research.[53]

Until the early 1900s, Koch's belief in the morphological and physiological stability of bacterial species was the dominant – but never a universally accepted – article of faith among medical bacteriologists. Only when the interests of medical bacteriologists turned toward immunological and chemical diagnostic methods did bacterial variation – changes in the antigenic and fermentative properties of bacteria, or in cell and colony morphology (especially the so-called S/R dissociation) — again become a subject of concern. During the interwar period, medical bacteriologists studied dissociation

[51] On the role of the Rockefeller Foundation, see R. E. Kohler, "The Management of Science: The Experience of Warren Weaver and the Rockefeller Foundation Program in Molecular Biology," *Minerva*, 14 (1976), 249–93; Pnina Abir-Am, "The Discourse of Physical Power and Biological Knowledge in the 1930s: A Reappraisal of the Rockefeller Foundation's Policy in Molecular Biology," *Social Studies of Science*, 12 (1982), 341–82; Lily E. Kay, *The Molecular Vision of Life: Caltech, The Rockefeller Foundation, and the Rise of the New Biology* (New York: Oxford University Press, 1993).

[52] Pnina Abir-Am, "From Multi-disciplinary Collaboration to Transnational Objectivity: International Space as Constitutive of Molecular Biology, 1930–1970," in *Denationalizing Science: The International Context of Scientific Practice*, ed. E. Crawford, T. Shinn, and S. Sorlin (Dordrecht: Kluwer, 1993), pp. 153–86.

[53] On the use of the electron microscope in morphological studies of bacteria and in research on bacteriophages, see Nicolas Rassmussen, *Picture Control: The Electron Microscope and the Transformation of Biology in America, 1940–1960* (Palo Alto, Calif.: Stanford University Press, 1997), especially chaps. 2 and 5. On the debates that the use of new methods sometimes generated among microbiologists, see James Strick, "Swimming against the Tide: Adrianus Pijper and the Debate over Bacterial Flagella, 1946–1956," *Isis*, 87 (1996), 274–305. See also Abir-Am, "The Discourse of Physical Power and Biological Knowledge in the 1930s"; Lily Kay, "Laboratory Technology and Biological Knowledge: The Tiselius Electrophoresis Apparatus, 1930–1945," *History and Philosophy of the Life Sciences*, 10 (1988), 51–72.

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primarily because it was linked to changes in pathogenicity and in immunological properties of bacteria, though they also speculated about the mechanisms of dissociation, appealed to various theories of genetics to explain their findings, and tried to test whether the changes were heritable and permanent or dependent on the environment in which bacteria were cultivated and readily reversible. Their research was largely structured by medical rather than genetic questions.[54]

The medically relevant implications of dissociation were, for example, central to the research on the transformation of pneumococci developed by Oswald Avery (1877–1955) and his group at the Rockefeller Institute Hospital. Avery and his coworkers were primarily interested in treatments for pneumococcal pneumonia, and their work on transformation fitted smoothly into their immunochemical research program. By 1943, however, when the in vitro system was fully perfected and Avery had managed to identify the responsible substance as DNA, the implications of his finding went far beyond medical bacteriology.[55]

In contrast with Avery's medical motivation, most new research on microbial genetics in the 1940s and 1950s resulted from biologists' (and some physicists') belief that microorganisms can serve as convenient experimental tools to pursue fundamental questions about the mechanisms of heredity and the genetic control of biochemical processes. Such, for example, was the motivation of the physicist-turned-geneticist Max Delbrück (1906–1981), who in the late 1930s chose the bacteriophage (a bacterial virus) as the experimental object with which he could attempt to solve "the riddle of life": Phages (bacterial viruses) appeared to Delbrück as "atoms in biology," elementary biological units able to self-replicate and thus the preferred experimental models and conceptual tools to understand gene action.[56]

General biological questions, rather than an interest in microorganisms per se, also led the geneticist George Beadle (1903–1989) and the bacterial biochemist Edward Tatum (1909–1975) to choose the bread mold neurospora as an organism particularly well suited for investigating the genetic control

[54] Olga Amsterdamska, "Medical and Biological Constraints"; Olga Amsterdamska, "Stabilizing Instability: The Controversies over Cyclogenic Theories of Bacterial Variation during the Interwar Period," *Journal of the History of Biology*, 24 (1991), 191–222; William Summers, "From Culture as Organism to Organism as Cell: Historical Origins of Bacterial Genetics," *Journal of the History of Biology*, 24 (1991), 171–90.

[55] Olga Amsterdamska, "From Pneumonia to DNA: The Research Career of Oswald T. Avery," *Historical Studies in Physical and Biological Sciences*, 24 (1993), 1–39; René Dubos, *The Professor, the Institute and DNA* (New York: Rockefeller University Press, 1976); Maclyn McCarty, *The Transforming Principle: Discovering that Genes Are Made of DNA* (New York: Norton, 1985); Ilana Löwy, "Variances of Meaning in Discovery Accounts: The Case of Contemporary Biology," *Historical Studies in the Physical Sciences*, 21 (1990), 87–121.

[56] Lily E. Kay, "Conceptual Models and Analytical Tools: The Biology of Physicist Max Delbrück," *Journal of the History of Biology*, 18 (1985), 207–46; Thomas D. Brock, *The Emergence of Bacterial Genetics* (Cold Spring Harbor, N.Y.: Cold Spring Harbor Laboratory Press, 1990); John Cairns, Gunther Stent, and James Watson, eds., *Phage and the Origins of Molecular Biology* (Cold Spring Harber, N.Y.: Cold Spring Harbor Laboratory Press, 1966).

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of biochemical mechanisms. Following Beadle's (and Boris Ephrussi's) investigations of the genetic control of eye color in *Drosophila*, Beadle, working together with Tatum, shifted his attention to neurospora because he hoped it would provide a simpler experimental model for which the biochemistry was better known and easier to control than that of *Drosophila.* Neurospora proved to be a convenient and productive experimental tool, and the one gene, one enzyme concept that emerged from this work served as a theoretical basis for the investigation of metabolic pathways using genetic mutants.[57]

A desire to make bacteria into organisms suitable for genetic research also motivated Joshua Lederberg to undertake his studies of mating in *Escherichia coli* in experiments using double nutritional mutants and phage resistance as markers.[58] The nature of the mating process and its physiology, genetic significance, and the organization of genetic material in a bacterial cell were studied in the 1950s and 1960s by a number of researchers, with particularly important work being conducted by William Hayes in Britain, and by François Jacob and Elie Wollman at the Pasteur Institute.

The fact that microorganisms, after the Second World War, became the preferred tools for the study of the physical and chemical basis of vital phenomena common to all living organisms signaled a triumph of the unitarian and reductionist biology, famously captured in Jacques Monod's dictum that what is true of *E. coli* is also true of an elephant. Because *E. coli* is simpler and easier to manipulate in a laboratory than an elephant, advances in molecular biology have resulted in an unprecedented development of knowledge about the molecular processes taking place in microorganisms and in the development of new arenas of collaboration and practice for microbiologists.

## CONCLUSIONS

The molecularization of microbiology and the use of microorganisms in molecular biological research and biotechnology have resulted in the unprecedented growth and diversification of microbiological research. Today, the American Society for Microbiology, with its 40,000 members and some twenty-four specialized subsections, boasts of being the world's largest organization for professionals in a single life science, and microbiologists work in a variety of institutional locations and settings.

But parallel with the laboratory and industrial domestication of a great number of different types of microorganisms, and our increased ability to

[57] Robert E. Kohler, "Systems of Production: *Drosophila*, Neurospora, and Biochemical Genetics," *Historical Studies in the Physical and Biological Sciences*, 22 (1991), 87–130; Lily E. Kay, "Selling Pure Science in Wartime: The Biochemical Genetics of G. W. Beadle," *Journal of the History of Biology*, 2 (1989), 73–101.

[58] Joshua Lederberg, "Forty Years of Genetic Recombination in Bacteria," *Nature*, 324 (1986), 627–9; Joshua Lederberg, "Genetic Recombination in Bacteria: A Discovery Account," *Annual Review of Genetics*, 21 (1987), 23–46.

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manipulate and engineer microbial life in the test tube and the fermentation reactor, recent decades have also underscored the continuing difficulties of controlling microorganisms beyond the laboratory walls. The resurgence of infectious diseases, such as tuberculosis, whose control had previously appeared to be imminent, and the emergence of new epidemics, such as AIDS, emphatically testify to this disparity between the laboratory and the outside world.

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# 18

# PHYSIOLOGY

*Richard L. Kremer*

Among the modern life sciences, physiology trails only the evolutionary sciences in the attention it has received from historians. Lamarck, Darwin, and Mendel may be better known than the heroes of modern physiology, but names such as François Magendie (1783–1855), Johannes Müller (1801–1858), Claude Bernard (1813–1878), Hermann von Helmholtz (1821–1894), Ivan Pavlov (1849–1936), and Charles Sherrington (1857–1952) require little introduction for those who read more than occasionally in the history of science. Physiology may have attracted such attention because it has been widely viewed as the first of the modern biological disciplines to emerge from traditional approaches to the phenomena of life embodied in medicine and natural history. Furthermore, physiology allowed historians of science of the first generation after World War II to develop a series of narratives that reflected their broader concerns about the nature and significance of modern science and about how to write its history. If the historiography of the physical sciences in the 1950s and 1960s found its normative models in the "Scientific Revolution" of the sixteenth and seventeenth centuries, so, too, in those decades did historians of the life sciences locate their normative models in nineteenth-century physiology.[1]

## FOUNDATIONAL NARRATIVES

Apart from a few heroic biographies of men such as Helmholtz or Bernard, the first attempts to write synthetic histories of modern physiology as more than a segment of the history of medicine appeared in the 1950s and 1960s. These authors included active physiologists and also leading representatives of the first generation of professional historians and sociologists of science in Europe and North America. Physiology served as the anvil on which these

[1] See Mario Biagioli, "The Scientific Revolution Is Undead," *Configurations*, 6 (1998), 141–7.



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writers hammered out some methods and concepts for the then professionalizing disciplines of the history and sociology of science.[2] These early histories of physiology present four intertwined themes: physiology's "struggle for independence," the experimentalization of the discipline, the growth of physiological "concepts," and the formation of research schools or genealogies of physiologists.

The origin of post-Aristotelian physiology (the Greek term originally meant the study of nature in general) is usually dated to the well-known French physician Jean Fernel, whose *De naturali parte medicinae* (*On the Natural Parts of Medicine,* 1542) outlines a university medical curriculum in five divisions, including physiology as the "total nature of healthy man." Yet, for the early historians, physiology would retain a subordinate status in the seventeenth- and eighteenth-century universities as part of "theoretical medicine" or the "institutes of medicine." Not until early in the nineteenth century would physiology achieve its "independence," especially from medical anatomy; only then would the history of physiology become a history of physiologists. As such, physiology provided the history of science with one of its first stories of successful nineteenth-century disciplinary specialization, a central feature of what came to be called the "Second Scientific Revolution."

Using historian Karl Rothschuh's list of significant physiological discoveries, sociologist Joseph Ben-David's studies of German university structure described disciplinary formation as a "natural" result of the drive for independence. Conceptually, this process required the articulation of new methods for practicing research and new concepts for organizing theoretical

[2] For some of the most influential of these early histories, see Owsei Temkin, "Materialism in French and German Physiology of the Early Nineteenth Century," *Bulletin of the History of Medicine*, 20 (1946), 322–7; Karl E. Rothschuh, *Entwicklungsgeschichte physiologischer Probleme in Tabellenform* (Munich: Urban and Schwarzenberg, 1952); Karl E. Rothschuh *Geschichte der Physiologie* (Berlin: Springer, 1953), English trans. Guenter Risse (Huntington, N.Y.: Krieger, 1973); Karl E. Rothschuh, "Ursprünge und Wandlungen der physiologischen Denkweise im 19. Jahrhundert [1966]," in Karl E. Rothschuh, *Physiologie im Werden* (Stuttgart: Gustav Fischer, 1969), pp. 115–81; Paul F. Cranefield, "The Organic Physics of 1847 and the Biophysics of Today," *Journal of the History of Medicine*, 12 (1957), 407–23; Chandler McC. Brooks and Paul F. Cranefield, eds., *The Historical Development of Physiological Thought* (New York: Hafner, 1959); Joseph Ben-David, "Scientific Productivity and Academic Organization in Nineteenth-Century Medicine," *American Sociological Review*, 25 (1960), 828–43; Avraham Zloczower, *Career Opportunities and the Growth of Scientific Discovery in 19th-Century Germany* (MA thesis, Hebrew University, 1960) (New York: Arno, 1981); Joseph Ben-David and Avraham Zloczower, "Universities and Academic Systems in Modern Societies," *European Journal of Sociology*, 3 (1972), 45–84; Everett Mendelsohn, "Physical Models and Physiological Concepts: Explanation in Nineteenth-Century Biology," *British Journal for the History of Science*, 2 (1965), 201–19; Georges Canguilhem, *La formation du concept du réflexe aux XVIIe et XVIIIe siècles* (Paris: J. Vrin, 1955); Georges Canguilhem, "La constitution de la physiologie comme science" [1963], in Georges Canguilhem, *Etudes d'histoire et de philosophie des sciences* (Paris: J. Vrin, 1968), pp. 226–73; Joseph Schiller, *Claude Bernard et les problèmes scientifiques de son temps* (Paris: Éditions du Cèdre, 1967); Joseph Schiller, "Physiology's Struggle for Independence in the First Half of the Nineteenth Century," *History of Science*, 7 (1968), 64–89. For an earlier history that greatly influenced authors of the foundational narratives, see Heinrich Boruttau, "Geschichte der Physiologie in ihrer Anwendung auf die Medizin bis zum Ende des neunzehnten Jahrhunderts," in *Handbuch der Geschichte der Medizin*, vol. 2, ed. Theodor Puschmann, Max Neuburger, and Julius Pagel (Jena: Gustav Fischer, 1903), pp. 347–456.

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explanations. Institutionally, the process created the structures that by 1900 would mark most scientific disciplines: specialized journals, textbooks, and handbooks edited by the leading discipline-builders; separate chairs within the German universities followed by separate institutes (dedicated buildings with classroom and laboratory spaces, staff, budgets, apparatus, and research materials) that provided permanent employment and disciplinary identity; recognition as a prescribed subject in university curricula and professional licensing examinations; and finally the establishment of specialized professional societies, both national and international. In this independence movement, French and Germans supplied the conceptual innovations and Germans the institutional ones.[3]

The site of employment provided the critical marker for disciplinary identity. Albrecht Haller (1708–1777), who wrote the leading physiological textbook of the eighteenth century, had been a professor of anatomy, surgery, and medicine in Göttingen. Lazzaro Spallanzoni (1729–1799), a pioneer in animal experimentation, was a priest and a professor of natural history in Padua. William Beaumont (1785–1853), the American author of an important study of digestion, was a military surgeon. Magendie, the leading French advocate of vivisection and founder of a new journal for physiology and pathology, had been a physician and private lecturer before becoming a professor of medicine at the Sorbonne. Johannes Müller, undoubtedly the leading German practitioner in the first half of the nineteenth century, held the chair for anatomy and physiology in Berlin University's Faculty of Medicine. The shift toward independence as measured by employment began in 1811, when a chair for physiology (not combined with anatomy) was created at the new university in Breslau. The medical schools in Paris and Montpellier established separate chairs for physiology in 1823 and 1824, respectively, but for the next half century, neither faculty appointed self-conscious discipline-builders to the positions. An independent physiology would first emerge not in France but in the German states. By 1860, nearly every German university had created a separate chair for physiology in its medical faculty. By century's end, university medical faculties in Britain, the United States, and France were following the German model. In 1871, Henry Bowditch (1840–1911) became the first professor of physiology at Harvard's Medical School; in 1874, John Burdon-Sanderson (1828–1905) inaugurated a similar chair at University College London.

With chairs came specialized physiological laboratories, again first in Germany, and elsewhere not until 1900. Already in the 1820s the first such labs appeared in Freiburg and Breslau; by the 1870s, most German universities had one. At first, these laboratories supported demonstrations that the professor performed during his lectures and perhaps some private research

[3] For diagrammatic representations of the growth of physiological discoveries, chairs, and specialized periodicals in the nineteenth century, see Rothschuh, *Physiologie im Werden*, pp. 172–6.

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by the professor or a few advanced students. Not until the 1880s, when mass-produced laboratory apparatus became cheaply available and state medical licensing began to require some experimental work, did the institutes begin to offer hands-on experimental exercises for all students in physiology classes. Staffing these institutes provided employment for dozens of newly trained "physiologists" at universities across Europe.

These new career opportunities prompted the rise of professional organizations and societies. Physiology had not fared well in the broad national associations such as the Versammlung Deutscher Naturforscher und Ärzte (established 1828), the British Association for the Advancement of Science (1831), and the American Association for the Advancement of Science (1848). In the disciplinary sections of these societies, physiology usually remained combined with anatomy and/or zoology. In the AAAS, it so appeared only from 1851 to 1860 and then disappeared completely from the sections. Only in the Versammlungen did physiology attain its own independent section, albeit not before 1889. The discipline-builders preferred rather to establish their own specialized societies. In 1875, the Berlin Physiologische Gesellschaft began meeting. In 1876, the British Physiological Society appeared. In 1887, an American Physiological Society was formed. Two years later, the first International Congress of Physiology met in Basel, with 124 participants. In 1895, as plans for the Nobel Prizes were being drafted, one category included "physiology or medicine."[4] For the early histories of physiology, these institutional innovations marked the successful emergence of an independent discipline much less tied to the needs of human medicine than had been the case in 1800.

Although the historians generally presented this move toward independence as a natural result of the "growth of knowledge," in the 1960s Ben-David and his student Avraham Zloczower sought to explain the burgeoning of new disciplines as a result of market forces within the German university system. Using physiology as their case study, they argued that young German scholars, seeking professorial positions in a nonexpanding set of universities in which hierarchical faculty organization prevented the "doubling" of chairs for the same subject, were forced to specialize. If a single university could be persuaded to create a chair for a new discipline, competition among Germany's decentralized universities would quickly drive the innovation throughout the system, and some twenty new chairs would become available. An earlier generation of German medical professors may have sought to teach both anatomy and physiology to enhance their incomes from student fees, but the generation habilitating in the 1840s sought to separate these disciplines as the only means to gain access to a university career. For early sociologists of science such as Ben-David and Zloczower,

[4] By 1902, this category had become "physiology and medicine." See Claire Salomon-Bayet, "Bacteriology and Nobel Prize Selections, 1901–20," in *Science, Technology, and Society in the Time of Alfred Nobel*, ed. Carl Gustav Bernhard, Elisabeth Crawford, and Per Sörbom (Oxford: Pergamon Press, 1982), pp. 377–400.

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physiology epitomized the waves of disciplinary specialization that swept the natural sciences during the second half of the nineteenth century.

A second theme emphasized in the early histories is the emergence of physiology as the first experimental science of life, a process again situated within the nineteenth century. Although these historians recognized the experimental efforts of eighteenth-century investigators such as Haller or Spallanzoni, they nonetheless asserted that "active propagandists"[5] for experimentation appeared only after 1800, with Magendie usually cited as the instigator of "experimental physiology." The early historians also reiterated the claims of their early nineteenth-century subjects that physiology's experimental turn represented a rejection of more philosophical, speculative, or otherwise antiempirical views of life that had been promulgated around 1800 by French *idéologues* and German Romantic *Naturphilosophen*.

By 1850, three distinct approaches to experimental physiology had emerged, employing different research materials, apparatus, and theoretical assumptions. An empirical or vivisectional approach used live animals to determine causal conditions of, and often anatomical locations for, various physiological functions.[6] Magendie's studies of the toxic actions of botanical drugs, his vivisection of the cranial nerves, or his discovery of the role of the cerebellum in maintaining animal equilibrium provided exemplars for this approach. Bernard's discoveries of the glycogenic function of the liver, the active vasodilator reflex, and the temperature topography of the vascular system continued this tradition. His influential *Introduction à l'etude de la medécine expérimentale* (Introduction to the Study of Experimental Medicine, 1865) provided a classic articulation of its methods and rationale. In Germany, Jan Purkyně opened a new chapter in sensory physiology by exploring phenomena such as pressure phosphenes or changes in the apparent relative luminosity of colors as a function of the intensity of light (the so-called Purkyně shift) by means of intricate experiments on his own body. Surgical interventions and naked-eye observations of animal behavior marked this vivisectional approach.

A second type of experimentation, deploying the apparatus of physics, quantitative measurement, graphical representation of data, and preparations of isolated tissues or organs, gained legitimacy through the efforts of Carl Ludwig (1816–1895) and a coterie of Johannes Müller's leading students, including Emil Du Bois-Reymond (1818–1896), Ernst Brücke (1819–1892), and Helmholtz. Cultivating relationships with instrument makers, military engineers, physicists, and mathematicians, these "organic physicists" in the second half of the nineteenth century made the kymograph, galvanometer, nonpolarizing electrodes, thermocouple, mercury manometer, and the frog's isolated gastrocnemius muscle into veritable icons of experimental

[5] Canguilhem, *Etudes d'histoire et de philosophie des sciences*, p. 231.
[6] Rothschuh, *Geschichte der Physiologie*, p. 93.

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physiology.[7] Ludwig's studies of mechanical aspects of circulation, Helmholtz's measurement of the velocity of propagation of nerve impulses, Brücke's analysis of how a chameleon changes colors, and Du Bois-Reymond's exploration of electrical currents in contracting muscle quickly became textbook examples of successful experimentation in the physicalist tradition.

The third experimental approach used techniques of elementary analysis to explore chemical changes accompanying physiological functions.[8] Systemic phenomena such as respiration and digestion were the first to be investigated chemically. Works on "animal chemistry" by leading chemists such as Jöns Jacob Berzelius (1779–1848), Friedrich Wöhler (1800–82), and Justus Liebig (1803–1873) showed the promise of this approach. Leopold Gmelin and Friedrich Tiedemann's *Verdauung nach Versuchen* (Experiments on Digestion, 1827) and Hermann Nasse's *Das Blut in mehrfacher Hinsicht* (Blood in Diverse Aspects, 1836) provided more detailed studies, and by 1842 Karl Lehmann could begin to organize the field in his *Lehrbuch der physiologischen Chemie* (Textbook of Physiological Chemistry). Subsequent widely emulated research by Felix Hoppe-Seyler (1825–1895) on hemoglobin or Wilhelm Kühne (1837–1900) on chemical processes in the retina by the 1870s led physiological chemistry to become one of the first subdisciplines to break away from the newly independent discipline of physiology and establish its own institutional existence.

According to the early histories, the successes of these three experimental approaches prompted considerable discussion, at least through the 1870s, of the efficacy of various explanatory models for physiology. Such debates, often described as pitting "vitalists" against "reductionists," can be found in introductions to textbooks, public lectures, or polemical, semipopular works such as Magendie's "Quelques idées générales sur les phénomènes particuliers aux corps vivants" ("Some general ideas on the phenomena peculiar to living bodies," 1809), Henri Dutrochet's *L'agent immédiat du mouvement vital* (The Immediate Agent of Vital Movement, 1826), Liebig's *Chemische Briefe* (1844), Jacob Moleschott's *Kreislauf des Lebens* (Circulation of Life, 1852), or Bernard's *Introduction*. Many French experimentalists, especially Bernard, tend to be labeled vitalists or at least antireductionists for their appeals to special "biological" laws in physiological explanation. Germans, on the other hand, are thought to have preferred reductionist or "physicalist" explanations that rely solely on the language and laws of physics and chemistry.[9] Given the institutional dominance of German physiology by 1900, such national polarization in explanation yields a progressivist narrative of nineteenth-century physiology as the triumph of reductionism over vitalism. "Every single

7 Cranefield, "Organic Physics of 1957 and the Biophysics of Today."

8 Frederic L. Holmes, "Elementary Analysis and the Origins of Physiological Chemistry," *Isis*, 54 (1963), 50–81.

9 See Temkin, "Materialism in French and German Physiology of the Early Nineteenth Century"; Mendelsohn, "Physical Models and Physiological Concepts."

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successful experiment," concluded a leading early historian, helped refute vitalism.[10]

A third theme in the early histories is their characterization of the growth of knowledge as the articulation of physiological "concepts" within long-standing traditions of research on given physiological "problems." Although a host of such research traditions have been examined (e.g., sensory physiology, muscle physiology, metabolism, cellular physiology, neurophysiology), Georges Canguilhem best exemplifies this historiography in his analysis of bioenergetics and endocrinology. In Canguilhem's account, each research tradition began with a well-defined problem, and each problem would slowly be solved not only by innovative experiments but also by the "formation, deformation and correction of scientific concepts" that in turn led to new questions and directed further research.[11]

Bioenergetics emerged as a problem coincidentally with ideas of energy conservation. By 1800, researchers had recognized two forms of energy – mechanical work and heat – but had not elaborated any quantitative relation between them. In their classic experiments demonstrating that the production of animal heat could be accounted for by the combustion of nutrients as measured by respiratory gas exchange, Antoine Lavoisier (1743–1794) and Pierre Laplace (1749–1827) had ignored work. Subsequent experiments in the 1820s to reproduce these results seemed to indicate that respiration alone could not account for all the heat produced in an animal. Thus arose the general problem of investigating whether all the energy produced in vital phenomena is derived from the caloric content of ingested nutrients. Chemists Henri Victor Regnault (1810–1878) and Jules Reiset (1818–1896) set about determining the energy content of various nutrients. Eduard Pflüger (1829–1910) showed how the respiratory quotient could be used to determine which nutrients are combusted in the animal's body. And by 1900 Americans Max Rubner and W. O. Atwater would build large calorimeters and gasometers and demonstrate on resting or working whole organisms (dogs and humans) that energy is strictly conserved over lengthy periods of time in complex physiological processes. Energy balance was the critical concept in this research.

The research problem of glandular function crystallized long before the term endocrinology was coined in 1909. Through most of the nineteenth century, the functions of the ductless glands remained completely unknown. As emphasized by Canguilhem, Bernard's concept of internal secretion did not play a "heuristic role" in the discovery of glandular function because for

[10] Schiller, "Physiology's Struggle for Independence in the First Half of the Nineteenth Century," p. 84.

[11] Canguilhem, *Etudes d'histoire et de philosophie des sciences*, p. 235. For Canguilhem's epistemology, see Caspar Grond-Ginsbach, "Georges Canguilhem als Medizinhistoriker," *Berichte zur Wissenschaftsgeschichte*, 19 (1996), 235–44; Marjorie Grene, "The Philosophy of Science of Georges Canguilhem," *Revue d'histoire des sciences*, 53 (2000), 47–63; Jonathan Hodge, "Canguilhem and the History of Biology," *Revue l'histoire des sciences*, 53 (2000), 65–8.

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Bernard that concept served merely to differentiate glands from excretory organs such as the liver. Rather, the problem of glandular function was initiated as experimenters and clinicians noticed the lethal effect of destroying the thyroid or adrenals. In the 1890s, researchers discovered the therapeutic effects of transplanting the thyroid or injecting aqueous adrenal extract, and by about 1900 John Jacob Abel (1857–1938) and Jokichi Takamine (1854–1922) isolated the active principle from the adrenal medulla, which they named adrenaline, the first hormone to be discovered. After Walter B. Cannon (1871–1945) elaborated Bernard's concept of "internal environment" into the notion of "homeostasis," researchers realized that hormones provide chemical regulation of physiological processes. With this latter fundamental concept, Canguilhem concluded, endocrinology was born.

A final theme in the early histories is the organization of the newly independent discipline by national traditions and genealogical lineages. Rothschuh and Canguilhem especially employed family trees of teachers and students to describe not only the expansion of physiology through time and space but also continuities in the selection of apparatus and research problems (see Figure 18.1). As noted, the early histories usually locate the origin of modern physiology first in France and then in Germany. However, nineteenth-century French teaching laboratories could never rival the new German physiological institutes, either in size or available resources. From the 1870s through at least 1914, the German institutes attracted students from all European nations, Russia, and the Americas. Lineages of students, taught by German physiologists such as Ludwig, Du Bois-Reymond, Brücke, Carl Voit (1831–1908), and Ewald Hering (1834–1918), transplanted the ideals and practices of German physiology to universities and medical faculties across the globe, hence ensuring the successful copying of a model of physiology as an independent discipline distinct from medicine and anatomy.

Other national traditions receive significantly less attention in the early histories. Quoting from Edward Sharpey-Schafer's 1927 history of the British Physiological Society that "in the middle part of the nineteenth century Great Britain was far behind France and Germany. . . . We had no pure physiologists and it was considered that any surgeon or physician was competent to teach the science," Rothschuh devoted only a few pages to Britain (and the United States) in his account.[12] Scandinavia, Holland, and Belgium receive even less attention. Canguilhem's description of national traditions parallels Rothschuh's but does add a short section on Russia. In the classical accounts, an independent physiology, created in France and Germany by about 1860, spread outward to Britain, the United States, and Russia over the next generation.

Encompassed in these four themes, the early histories offer a history of modern physiology limited almost exclusively to the nineteenth century. In

[12] Rothschuh, *Geschichte der Physiologie*, p. 193.

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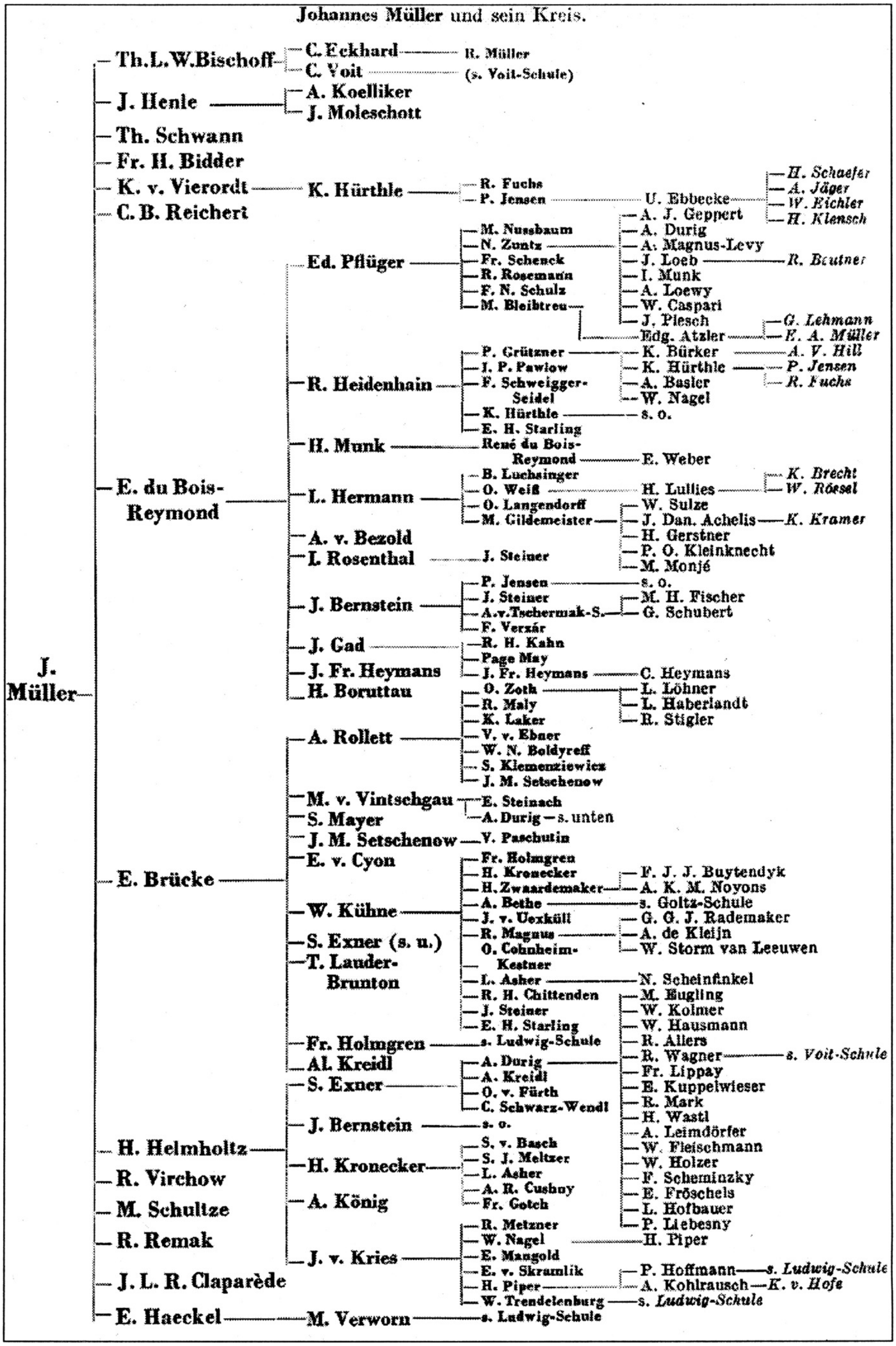


Figure 18.1. Rothschuh's family tree of modern physiologists. From Karl E. Rothschuh, *Geschichte der Physiologie*, p. 124.

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the story of the origin of physiology as an independent discipline and the establishment of its experimental practices, research problems, and conceptual frameworks, the early historians found exemplars both for a successful life science and for a coherent history and sociology of science.

## NEWER NARRATIVES

Over the past two decades, increasingly professionalized historians of science have turned from writing broad disciplinary histories to a myriad of new questions, emphasizing the heterogeneity of scientific practices, local contingency, and the embeddedness of scientific knowledge production within broader economic, political, cultural, and gendered networks. For the history of physiology, this turn to heterogeneity has meant that few attempts have been made to extend the early narratives, as comprehensive surveys of the discipline, into the twentieth century.[13] A synthetic history of physiology, covering both the nineteenth and twentieth centuries, has not been written. Rather, a host of recent studies, motivated by the new approaches to the history of science, has reworked some of the foundational accounts of the history of physiology.[14] Gone is the image of a unified, independent discipline, similarly instantiated in universities from Berlin and Paris to Moscow, London, Chicago, and Buenos Aires.

Although the story of the emergence of physiology as an independent discipline during the course of the nineteenth century has retained its force, newer scholarship has diversified the plot. Several historians, for example, have illustrated the variety of nineteenth-century physiological discourses that represented alternatives to the experimentalisms of Magendie or the German physicalists. Stressing the influence of Kant, historian Timothy Lenoir

[13] But see Chandler McC. Brooks, "The Development of Physiology in the Last Fifty Years," *Bulletin of the History of Medicine*, 33 (1959), 249–62; Gerald L. Geison, ed., *Physiology in the American Context, 1850–1940* (Bethesda, Md.: American Physiological Society, 1987), a collection of narrowly focused essays, albeit originally intended as a comprehensive "history of American physiology to 1940," according to J. R. Brobeck, O. E. Reynolds, and T. A. Appel, eds., *History of the American Physiological Society* (Bethesda, Md.: American Physiological Society, 1987), p. 491; and most recently Ilse Jahn, ed., *Geschichte der Biologie*, 3rd rev. ed. (Jena: Gustav Fischer, 1998), with lengthy chapters summarizing mostly German conceptual developments in "developmental physiology," "comparative animal physiology," and the "physiology and biochemistry of plants" from about 1850 to the present.

[14] For a useful orientation, see John V. Pickstone, "Physiology and Experimental Medicine," in *Companion to the History of Modern Science*, ed. R. C. Olby et al. (London: Routledge, 1990), pp. 728–42. Biography has remained an important genre. A highly selective list might include John C. Eccles, *Sherrington* (Berlin: Springer, 1979); Frederic L. Holmes, *Claude Bernard and Animal Chemistry* (Cambridge, Mass.: Harvard University Press, 1974); Frederic L. Holmes, *Hans Krebs*, 2 vols. (New York: Oxford University Press, 1991–3); Pinero Lopez and José Maria, *Cajal* (Madrid: Debate, 2000); Philip J. Pauly, *Controlling Life: Jacques Loeb and the Engineering Ideal in Biology* (New York: Oxford University Press, 1987); Daniel Todes, *Pavlov's Physiology Factory* (Baltimore: Johns Hopkins University Press, 2001); Elin L. Wolfe, A. Clifford Barger, and Saul Bennison, *Walter B. Cannon* (Cambridge, Mass.: Harvard University Press, 2000).

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identified a "teleomechanical" tradition, strong in German-speaking areas before 1850, that included purpose and form in explanations of living processes and provided the whipping boy against which the slightly younger physicalists sought to differentiate themselves. John Pickstone attributed the antireductionist "organic physics" of Henri Dutrochet to a strong botanical context in some post-Revolutionary French physiology. Gerald Geison explained the particularly anatomical discourse of British physiology before 1870 as the result of a peculiar mix of natural theology, antivivisectionism, and utilitarian extra-university medical education. In North America, "physiology" through much of the nineteenth century meant personal hygiene and health reform. Itinerant lecturers on physiology, often women, preached the virtues of cleanliness, exercise, diet, and temperance and created not only a short-lived American Physiological Society in 1837 (to promote vegetarianism) but also what Toby Appel has called a "women's subculture" of physiology that would be taught at private women's colleges in the United States until well after 1900.[15] Other studies have emphasized a host of local and biographical, as well as larger economic and political, issues that shaped the efforts of the well-known French and German discipline-builders, thereby offering highly differentiated stories of contingency in contrast to the teleological "naturalness" of disciplinary independence emphasized by the early histories.[16]

Likewise, in the newer histories, the spread of an independent experimental physiology has become much more complex than a simple transplantation of French or German models to universities in Britain, the United States, or elsewhere.[17] As told by Geison, a new, experimental physiology was very

[15] Timothy Lenoir, *The Strategy of Life: Teleology and Mechanics in Nineteenth-Century German Biology* (Dordrecht: Reidel, 1982); Kenneth Caneva, "Teleology with Regrets," *Annals of Science*, 47 (1990), 291–300; J. V. Pickstone, "Vital Actions and Organic Physics: Henri Dutrochet and French Physiology during the 1820s," *Bulletin of the History of Medicine*, 50 (1976), 191–212; Gerald L. Geison, *Michael Foster and the Cambridge School of Physiology: The Scientific Enterprise in Late Victorian Society* (Princeton, N.J.: Princeton University Press, 1978); Sally Gregory Kohlstedt, "Physiological Lectures for Women: Sarah Coates in Ohio, 1850," *Journal of the History of Medicine and Allied Sciences*, 33 (1978), 75–81; Toby A. Appel, "Physiology in American Women's Colleges: The Rise and Decline of a Female Subculture," *Isis*, 85 (1994), 26–56; Hebel E. Hoff and John F. Fulton, "The Centenary of the First American Physiological Society," *Bulletin of the History of Medicine*, 5 (1937), 687–734; Edward C. Atwater, "'Squeezing Mother Nature': Experimental Physiology in the United States before 1870," *Bulletin of the History of Medicine*, 52 (1978), 313–35.

[16] William Randall Albury, "Experiment and Explanation in the Physiology of Bichat and Magendie," *Studies in History of Biology*, 1 (1977), 47–131; John V. Pickstone, "Bureaucracy, Liberalism and the Body in Post-Revolutionary France: Bichat's Physiology and the Paris School of Medicine," *History of Science*, 19 (1981), 115–42; William Coleman, "The Cognitive Basis of the Discipline: Claude Bernard on Physiology," *Isis*, 76 (1985), 49–70; Timothy Lenoir, "Laboratories, Medicine and Public Life in Germany, 1830–1849," in *The Laboratory Revolution in Medicine*, ed. Andrew Cunningham and Perry Williams (Cambridge: Cambridge University Press, 1992), pp. 14–71; Richard L. Kremer, "Building Institutes for Physiology in Prussia, 1836–1846: Contexts, Interests and Rhetoric," in Cunningham and Williams, *Laboratory Revolution in Medicine*, pp. 72–109; Arleen Tuchman, *Science, Medicine, and the State in Germany: The Case of Baden, 1815–1871* (New York: Oxford University Press, 1993).

[17] Although most newer narratives remain preoccupied with Northern Europe and North America, see Josep Lluís Barona, *La doctrina y el laboratorio: Fisiología y experimentación en la sociedad española*

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quickly established in Britain after the Royal College of Surgeons in 1870 began requiring all candidates for membership to have attended a laboratory course in that subject. Although most medical schools at universities and hospitals soon began to offer courses in experimentation, it was at University College London, Cambridge, and Oxford that new laboratories under John Burdon Sanderson, Michael Foster (1836–1907), and Edward Sharpey-Schäfer (1850–1935), respectively, would become internationally recognized centers of physiological research by 1900.[18] Despite the fact that Foster had visited Germany and had modeled the organization of his laboratory on Ludwig's institute in Leipzig, British physiology at these leading centers became much more "biological" in orientation than in Germany, where the physiologists remained within medical faculties. At Cambridge, Foster, under the influence of Thomas Henry Huxley (1825–1895), developed a very popular laboratory course in elementary biology and incorporated evolutionary explanations into his physiology. Geison has argued that a distinctly nonmedical, Darwinian concern for evolutionary relationships between structure and function is what especially distinguished late Victorian physiology from the German model. Likewise, Paul Elliot has noted that experimental physiology in France "had to grow up" not in the human medical schools but rather in the Parisian Académie des Sciences and the Alfort veterinary school for most of the nineteenth century.[19]

Recent work on the history of experimental physiology in North America has also emphasized distinctiveness.[20] Although a few Americans after the Civil War had journeyed to Paris to work with Bernard, it was Ludwig's Leipzig Institute that in the 1870s to 1890s attracted what Robert Frank has described as the "German generation" of American travelers. Returning with a new appreciation for the value of laboratory research and an autonomous physiology and buoyed by the emerging reform movement in American medical education, these travelers took the lead in establishing physiology as a

*del siglo XIX* (Madrid: Consejo Superior de Investigaciones Científicas, 1992); Claudio Pogliano, "La fisiologia in Italia fra ottocento e novecento," *Nuncius*, 4, no. 1 (1991), 97–121; Kh. S. Koshtoyants, *Essays on the History of Physiology in Russia* [1946], trans. Donald B. Lindsley (Washington, D.C.: American Institute of Biological Sciences, 1964); M. Lindemann, S. A. Cesnokova, and V. A. Makarov, "I. M. Secenov und die Entwicklung der Elektrophysiologie in Rußland," *Zeitschriftenreihe für Geschichte der Naturwissenschaften, Technik und Medizin*, 16 (1979), 1–11; Daniel P. Todes, "Pavlov and the Bolsheviks," *History and Philosophy of the Life Sciences*, 17 (1995), 379–418.

[18] See Stella V. F. Butler, "Centers and Peripheries: The Development of British Physiology, 1870–1914," *Journal of the History of Biology*, 21 (1988), 473–500, who explains the less successful research programs at British provincial universities as a result of the dominance of clinical interests.

[19] Geison, *Michael Foster and the Cambridge School of Physiology*; Paul Elliott, "Vivisection and the Emergence of Experimental Physiology in Nineteenth-Century France," in *Vivisection in Historical Perspective*, ed. Nicolaas A. Rupke (London: Croom Helm, 1987), pp. 48–77.

[20] See Robert E. Kohler, *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline* (Cambridge: Cambridge University Press, 1982); Geison, *Physiology in the American Context*; W. Bruce Fye, *The Development of American Physiology: Scientific Medicine in the Nineteenth Century* (Baltimore: Johns Hopkins University Press, 1987); John Harley Warner, *Against the Spirit of System: The French Impulse in Nineteenth-Century American Medicine* (Princeton, N.J.: Princeton University Press, 1998), chaps. 8 and 9.

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required laboratory science at American medical schools. A British model of locating experimental physiologists in university biology departments, initialized by Foster's student Henry Newell Martin (1848–1896) at the new Johns Hopkins University and tried in varying configurations at Columbia, Toronto, and Chicago, failed to take hold.[21] It was in the university-related medical schools with their new four-year curricula that American physiology would develop. Yet as Frank has noted, the travelers to Germany brought back "everything but physiology."[22] Unlike the experimental physiologists at Cambridge, Oxford, and University College, London, American physiologists, perhaps burdened by their "service role" for medical students, were slow to establish internationally credible programs of research, as indeed was also the case in the British provinces and most of London's hospital-based medical schools. Although by 1910 American authors increasingly had begun to publish original research, not until 1929 would the United States host the International Physiological Congress. Not until the late 1930s would citations of American work in international physiological journals begin to burgeon. And not until 1944 would the first U.S.-trained physiologists win a Nobel Prize.[23]

Recent scholarship also has multiplied the "products" created by the newly independent discipline of physiology. That is, nineteenth-century physiological laboratories produced more than physiological knowledge and physiologists. At the German universities, physiological laboratories (like the early chemical laboratories at these universities) provided a setting where students could conduct original inquiries and thus realize the neo-humanist ideals of anti-utilitarian *Bildung* or individual cultural formation being heralded by German idealist reformers such as Georg Hegel or Wilhelm von Humboldt. Historian William Coleman has argued that concern for educational reform prompted Purkyně in the 1820s to establish an early physiological institute at the Prussian university in Breslau. Deeply influenced by pedagogical innovators such as Jean Jacques Rousseau and Johann Heinrich Pestalozzi, Purkyně sought to bring to the university a style of learning based on practice (i.e., on having students interact directly with physical objects rather than simply with texts). He also outlined a comprehensive program to implement Pestalozzian reforms in elementary and secondary schools. For Purkyně, hands-on laboratory physiology thus became a vehicle for the individual self-development of university medical students.[24] Other studies of early physiological

[21] For the largely unsuccessful attempt to establish a separate discipline of general physiology in North America, see Philip J. Pauly, "General Physiology and the Discipline of Physiology, 1890–1935," in Geison, *Physiology in the American Context*, pp. 195–207.

[22] Robert G. Frank, "American Physiologists in German Laboratories, 1865–1914," in Geison, *Physiology in the American Context*, p. 40.

[23] Gerald L. Geison, "Toward a History of American Physiology," in Geison, *Physiology in the American Context*, pp. 1–9; John Harley Warner, "Physiology," in *The Education of American Physicians*, ed. Ronald L. Numbers (Berkeley: University of California Press, 1980), pp. 48–71.

[24] William Coleman, "Prussian Pedagogy: Purkyně at Breslau, 1823–1839," in *The Investigative Enterprise: Experimental Physiology in 19th-Century Medicine*, ed. William Coleman and Frederic L. Holmes (Berkeley: University of California Press, 1988), pp. 15–64.

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laboratories in Heidelberg and Leipzig have emphasized the utilitarian interests of modernizing German states in supporting laboratory science, including physiological institutes. By encouraging university students to conduct standardized experiments, to make measurements, and to reason in a disciplined fashion about chains of causally linked phenomena that they had observed themselves, state educational authorities thought they could train a citizenry better able to meet the needs of industrializing economies. As physiological laboratories over the course of the nineteenth century became larger, routinized, and filled with mass-produced, standardized apparatus, they provided cultural indoctrination (at least in the eyes of state educational officials) for the thousands of students who passed through their doors.[25] By early in the next century, specialized laboratories, such as the Kaiser Wilhelm Institut für Arbeitsphysiologie or Harvard's Fatigue Laboratory, would begin to produce knowledge aimed specifically at problems of "industrial relations" in modern states.[26]

The newer studies also have emphasized the utility of experimental physiology for a medical profession eager to make itself more "scientific." Historians are not of one mind, however, on whether this utility was more relevant ideologically than therapeutically, or on when the balance tipped toward the latter. For example, historian John E. Resch has argued that already in the 1830s and 1840s, the Parisian Académie de Médecine, with many veterinarians among its members, actively supported experimentation (mostly vivisection on animals) as a means to improve surgical techniques. Such a surgical physiology, more empirical than speculative, resonated well within the French tradition extending from Xavier Bichat (1771–1802) and Magendie to Bernard and created an image of experimental physiology as, to use Resch's term, "incorporated" into medicine.[27] On the other hand, Geison, John Warner, and others have argued that for most of the nineteenth century physiology offered few resources for the improvement of human medical diagnosis or therapy. Even after experimentalists such as Robert Koch (1843–1910) and Louis Pasteur (1822–1895) launched their crusades for bacteriology and immunology, many practicing clinicians remained skeptical of the

[25] Timothy Lenoir, "Science for the Clinic: Science Policy and the Formation of Carl Ludwig's Institute in Leipzig," in Coleman and Holmes, *Investigative Enterprise*, pp. 139–78; Tuchman, *Science, Medicine, and the State in Germany*; Todes, *Pavlov's Physiology Factory*.

[26] Anson Rabinbach, *The Human Motor: Energy, Fatigue and the Origins of Modernity* (New York: Basic Books, 1990); Steven M. Horvath and Elizabeth C. Horvath, *The Harvard Fatigue Laboratory: Its History and Contributions* (Englewood Cliffs, N.J.: Prentice-Hall, 1973); Carleton B. Chapman, "The Long Reach of Harvard's Fatigue Laboratory, 1926–1947," *Perspectives in Biology and Medicine*, 34 (1990), 17–33; John Parascandola, "L. J. Henderson and the Mutual Dependence of Variables: From Physical Chemistry to Pareto," in *Science at Harvard University: Historical Perspectives*, ed. Clark A. Elliott and Margaret W. Rossiter (Bethlehem, Pa.: Lehigh University Press, 1992), pp. 167–90; Richard Gillespie, "Industrial Fatigue and the Discipline of Physiology," in Geison, *Physiology in the American Context*, pp. 237–62; Philipp Sarasin and Jakob Tanner, eds., *Physiologie und industrielle Gesellschaft: Studien zur Verwissenschaftlichung des Körpers im 19. und 20. Jahrhundert* (Frankfurt: Suhrkamp, 1998).

[27] John E. Resch, "The Paris Academy of Medicine and Experimental Science, 1820–1848," in Coleman and Holmes, *Investigative Enterprise*, pp. 100–38.

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laboratory. In the United States, medical schools rarely required a laboratory course in physiology until after Abraham Flexner published his hard-hitting criticism of medical education in 1910; indeed, even into the 1930s, many influential American physicians complained about the nonutility of physiology for medical practice. Yet in this same period, as medicine in both Europe and North America sought to elevate its professional status, the ideology of the laboratory and "scientific" experimental physiology could be deployed as rhetorical resources to enhance the public status and authority of medicine. Leading physiologists such as Bernard, Du Bois-Reymond, and Foster pounded this theme in popular lectures and essays.[28] By the 1920s, some of this rhetoric became reality, as what Pickstone has called the "clinical physiologists" isolated insulin and found cures for anemia. Although the early histories had largely taken for granted the medical utility of experimental physiology, the newer historiography has sought to differentiate the ideological and medical contributions produced by physiological research, especially after 1900.

An enhanced public profile, however, also brought with it waves of public criticism of what Bernard in his *Introduction* had called the "ghastly kitchen" of the vivisector's laboratory. Recent studies have presented antivivisectional resistance to experimental physiology and to the attempts of physiologists to promulgate "scientific medicine" as important chapters in the late nineteenth-century social history of European and North American class and gender relations.[29] Although discussions of the morality and utility of experiments on living animals extend back to the very beginnings of Western medicine, organized antivivisectionist social movements emerged first in nineteenth-century Britain, where ironically, given the "stagnancy" of British physiology, very little vivisection had been practiced before 1870. Yet British popular discussions of Magendie's lecture demonstrations in the 1820s and reports of animal experiments at the veterinary school at Alfort or in Bernard's laboratory prompted the Royal Society for the Prevention of Cruelty to Animals to open an anti-French, antivivisection campaign. By the 1870s, dozens of antivivisection societies had been organized, hundreds

[28] Gerald L. Geison, "Divided We Stand: Physiologists and Clinicians in the American Context," in *The Therapeutic Revolution: Essays in the Social History of American Medicine*, ed. Morris J. Vogel and Charles E. Rosenberg (Philadelphia: University of Pennsylvania Press, 1979), pp. 67–90; John Harley Warner, "Ideals of Science and Their Discontents in Late 19th-Century American Medicine," *Isis*, 82 (1991), 454–78; John Harley Warner, "The Fall and Rise of Professional Mystery: Epistemology, Authority and the Emergence of Laboratory Medicine in Nineteenth-Century America," in Cunningham and Williams, *Laboratory Revolution in Medicine*, pp. 110–41; Merriley Borell, "Training the Senses, Training the Mind," in *Medicine and the Five Senses*, ed. W. F. Bynum and Roy Porter (Cambridge: Cambridge University Press, 1993), pp. 244–61.

[29] H. Bretschneider, *Der Streit um die Vivisektion im 19. Jahrhundert* (Stuttgart: Gustav Fischer, 1962); Richard D. French, *Antivivisection and Medical Science in Victorian Society* (Princeton, N.J.: Princeton University Press, 1975); Coral Lansbury, *The Old Brown Dog: Women, Workers and Vivisection in Edwardian England* (Madison: University of Wisconsin Press, 1985); Rupke, *Vivisection in Historical Perspective*; Craig Buettinger, "Women and Antivivisection in Late 19th-Century America," *Journal of Social History*, 30 (1997), 857–72.

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of pamphlets and books had rolled off the presses, and a royal commission had called for limited government regulation of animal experimentation. In 1876, Parliament approved the Cruelty to Animals Act, empowering the Home Office to license all experiments on live animals and mandating the use of anesthesia whenever possible. This legislative regulation sparked similar antivivisection campaigns over the next twenty years in Germany and the United States, albeit without much legislative success. The antivivisection efforts also prompted physiologists to organize their own political lobbying groups, such as the Association for Advancement of Medicine by Research in Britain (1882) or the Council for the Defense of Medical Research in the United States (1907).

Despite some national differences in tone – British antivivisectionists emphasized animal rights, Americans the theme of Christian moral reform, Germans a view of medicine as a noninterventionist reliance on the healing forces of nature – the arguments and contestants in these debates remained quite uniform from the 1870s through the eclipse of the first wave of antivivisectionism by the time of the Great War. Most of the rhetoric swirled around four issues: the medical utility of animal experiments, the moral status of animals, the moral effects of vivisection on the vivisectors and their audiences, and what historians Andreas-Holger Maehle and Ulrich Tröhler have called *tu-quoque* arguments (i.e., whether the slaughtering or abuse of animals for other human needs does or does not justify vivisection). Many of the leading antivivisectionists were urban, upper middle-class or aristocratic women who also opposed evils such as slavery, compulsory vaccination, strong drink, dishonoring the Sabbath, child labor, and prostitution. The leading defenders of vivisection tended to be well-known medical scientists, such as Rudolph Virchow (1821–1902), T. H. Huxley, or Cannon. Most accounts agree on the outcome of the struggle. Despite some restrictive legislation in Britain and a more disciplined use of anesthesia in animal experiments, the forces of experimental physiology won the day. After 1900, animal experimentation flourished not only in physiology but also in bacteriology, pharmacology, and immunology, and the antivivisectionist organizations faded into partisan bickering and crankdom.

Yet several historians have detected deeper issues in this struggle. Echoing themes of Fritz Ringer's *The Decline of the German Mandarins* or Frank Turner's *Between Science and Religion*, Richard D. French saw in antivivisectionism an articulation of hostilities shared by leading members of the middle and upper classes, especially religious thinkers and literary intellectuals, to science as the leading institution in late Victorian society. Coral Lansbury has detected, at a level "beyond conscious awareness," a fusion of themes of women's rights and antivivisectionism. From her analysis of antivivisectionist novels, such as Wilkie Collins's *Heart and Science* (1883), Sarah Grand's *The Beth Book* (1897), or H. G. Wells's *The Island of Dr. Moreau* (1896), of pornographic novels, and of women's widespread distaste for the

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gynecological procedures and ovariotomies practiced by late Victorian male physicians, Lansbury concluded that "the vivisected animal stood for vivisected woman: the woman strapped to the gynecological table, the woman strapped and bound in the pornographic fiction of the period." In the late Victorian emotional landscape, she argued, images of victim-animal-woman as objects resonated with images of pornographer-gynecologist-vivisector as subjects. Similarly, Stewart Richards suggested that experimental practices, such as those described in John Scott Burdon Sanderson's widely used *Handbook for the Physiological Laboratory* (1873), must have forced physiology to "demand of its practitioners a special kind of psychological commitment sufficiently powerful to bracket off not merely aesthetic, but in some circumstances, ethical misgivings also." Unlike most other sciences, experimental physiology necessarily "prescribed the infliction of pain," argued Richards. As such, physiology became not simply a "special case of physics or chemistry" but rather a "science whose instrumental norms are inseparable both from its public and its private ethics."[30]

## THE DISAPPEARANCE OF PHYSIOLOGY?

As noted, the early historians of physiology's "independence" usually concluded the story around 1900 for reasons not difficult to surmise. Geison has perceptively argued that "Just as physiology had once declared its independence from medicine and medical anatomy, so new fields and specialties now [after 1900] seemed to declare their own independence from physiology."[31] Indeed, to a host of observers, physiology in the twentieth century, especially after 1945, has seemed like a discipline on the verge of "being pulled apart"[32] by new clinical specialties such as endocrinology or immunology and by new biological disciplines such as biochemistry or neurology. Such centrifugal

30 French, *Antivivisection and Medical Science in Victorian Society*, p. 371; Lansbury, *Old Brown Dog*, p. x; Stewart Richards, "Anaesthetics, Ethics and Aesthetics: Vivisection in the Late Nineteenth-Century British Laboratory," in Cunningham and Williams, *Laboratory Revolution in Medicine*, pp. 142–69, at p. 168; Stewart Richards, "Vicarious Suffering, Necessary Pain: Physiological Method in Late Nineteenth-Century Britain," in Rupke, *Vivisection in Historical Perspective*, pp. 125–48, at p. 144. See also Stewart Richards, "Drawing the Life-blood of Physiology: Vivisection and the Physiologists' Dilemma, 1870–1900," *Annals of Science*, 43 (1986), 27–56.

31 Geison, "Divided We Stand," pp. 78–9. Robert E. Kohler, "Medical Reform and Biomedical Science: Biochemistry – a Case Study," in Vogel and Rosenberg, *Therapeutic Revelution*, pp. 27–66, at p. 60, employed a geographical metaphor, describing physiology "losing provinces as separate disciplines or research specialties."

32 Toby A. Appel, "Biological and Medical Societies and the Founding of the American Physiological Society," in Geison, *Physiology in the American Context*, p. 155. For similar sentiments, see Rothschuh, *Geschichte der Physiologie*, p. 222; Brooks, "Development of Physiology in the Last Fifty Years," p. 250; Peter Hall, "Fragmentation of Physiology: Possible Academic Consequences," *The Physiologist*, 19 (1976), 35–9; Alan C. Burton, "Variety – the Spice of Science as Well as Life: The Disadvantages of Specialization," *Annual Review of Physiology*, 37 (1975), 1–12.

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tendencies have been most apparent in the United States, with its rapidly expanding scientific, medical, and philanthropic infrastructure. The American Physiological Society (APS), founded in 1887, has served as a lightning rod both for expressions of anxiety about the disappearance of the discipline and for efforts to counteract that disappearance.[33]

By the mid-1940s, several trends began to trouble leaders of the APS. The number of new doctoral degrees awarded in physiology had begun to decline, both absolutely and relative to the number of degrees awarded in other life sciences. A host of new professional societies had begun to compete with the APS for members and professional identity.[34] The number of physiology departments, especially at U.S. medical schools, had been declining; remaining departments were being merged, reconfigured, or renamed to mark new alliances with other biomedical specialties.[35] In response, the APS commissioned several extraordinary self-studies in the 1940s and 1950s. The Adolf Study of 1945–6 surveyed the activities of 750 researchers (only 52 percent of whom labeled themselves "physiologists"), a heterogeneous group of persons either formally trained in the field, currently working in the field, or currently working in other fields but researching topics considered "physiological" by the study committee. Explicitly refusing to define physiology any more narrowly than "the study of processes in living units," the Adolf Study decried the "inevitable but bewildering and doubtless deleterious fragmentation of physiology into subdivisions" and the lack of required courses in "general physiology" at North American medical schools.[36]

The Adolf Study immediately came under fire as an inadequate response to the challenges facing the APS. At a symposium on physiological education at the society's 1947 annual meeting, the Chicago developmental biologist Paul A. Weiss (1898–1989) proposed to define physiology not as a discipline or a subject matter but rather as an "attitude" toward the study of "functions" (not merely "mechanisms" or "processes") that give meaning to biological systems. Such an attitude would cover a "much wider field than do most of

[33] For a perceptive analysis of the APS during the mid-twentieth century, see George Joseph, "Physiologists Face 'Going Molecular': Professional Identity and Professional Anxiety in Mid-twentieth-Century American Physiology," unpublished manuscript, 1999. I thank Mr. Joseph for sending me a copy of this essay.

[34] Such competitors included the Society for Experimental Biology and Medicine, founded in 1903, American Society for Biological Chemists (1906), American Society for Pharmacology and Experimental Therapeutics (1908), American Society of Experimental Pathology (1913), Society of General Physiologists (1946), Biophysical Society (1957), and Society for Neuroscience (1969).

[35] A review of nearly one hundred North American medical schools indicates that fewer than half currently have departments of "Physiology." Nearly a quarter have departments of "Physiology and Biophysics." Departments of "Anatomy and Physiology," "Neuroscience and Physiology," "Physiology and Pharmacology," "Molecular and Cellular Physiology," and "Cell Biology and Physiology" also appear with some frequency. See *Peterson's Graduate Programs in the Biological Sciences*, 35th ed. (Peterson's: Princeton, N.J., 2001).

[36] E. F. Adolph, et al., "Physiology in North America, 1945: Survey by a Committee of the American Physiological Society," *Federation Proceedings*, 5 (1946), 407–36.

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the traditional physiological agencies – departments, societies, and journals," and could help to unify biology, "our mother science."[37] A second self-study, conducted from 1952 to 1954 by Ralph Gerard (1900–1974), a professor of neurophysiology at the University of Michigan, with massive funding from the newly established National Science Foundation, reiterated Weiss's definition: "In spirit, physiology is not a science or a profession but a point of view . . . [that] pervades the life sciences; it is a way of looking at life processes and understanding them."[38] Distinguishing "central" (those self-identifying "physiology" as first in rank order of the "fields of biology" in which they worked) from "peripheral" (self-identifying "physiology" as second to fourth in rank order) physiologists, Gerard's survey found fewer than one-third of its 4,500 respondents had earned a doctorate in physiology, fewer than one-fifth were employed in departments of physiology, and fewer than one-quarter of all authors of articles in APS journals were central physiologists. Yet by considering physiology as a "point of view," Gerard was not completely pessimistic about such data:

> In sum, physiology, though growing, is lagging. Whether the relative shrinkage of physiology and its most traditional sub-areas is to be viewed with alarm, in terms of the encroachment of other disciplines (especially the chemical ones) upon it; or with pride, in terms of the infiltration of physiological attitudes and methods into other disciplines (including microbiology and psychology), is perhaps a matter of taste.[39]

To save physiology from fragmenting completely into other biological specialties, Gerard recommended a complex strategy of public relations and curricular reform, ranging from making and promoting movies about career opportunities to more teaching of "integrative biology" in U.S. high schools and colleges.

Despite Gerard's recommendations, problems of fragmentation only increased for the Society. By 1976, the APS had begun to divide into specialty "sections" with their own membership lists, meetings, and journals. Within a decade, over fifteen sections had emerged.[40] A "White Paper on the Future of Physiology," prepared in 1990 by a Long Range Planning Committee of the APS, noted that "a deep malaise permeates the physiological community regarding the future of the science and of the institutions that represent it. . . . This malaise has been extant since the founding of the APS more than a century ago." Over the twentieth century, physiology has faced a "continual propensity to fractionation and to found new divisions that

[37] Paul Weiss, "The Place of Physiology in the Biological Sciences," *Federation Proceedings*, 6 (1947), 523–5.

[38] R. W. Gerard, *Mirror to Physiology: A Self-Survey of Physiological Science* (Washington, D.C.: American Physiological Society, 1958), p. 1.

[39] Ibid., p. 48.

[40] John S. Cook, "Sectionalization," in Brobeck, Reynolds, and Appel, *History of the American Physiological Society*, pp. 427–61.

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became scientific disciplines in their own right." Hence, physiology is "not a unitary science, its heterogeneity may be regarded as an intrinsic property of the subject.... Physiology as a science and a profession does not in fact exist."[41] Gerard's irony of the 1950s had become nihilism by the 1990s.

A comprehensive study of the fate of twentieth-century physiology as a discipline, source of professional identity, or collection of research programs has yet to be written.[42] A preliminary reconnaissance of several quantitative indicators presents a mixed picture of physiology's continuing viability amid the explosion of increasingly specialized biological sciences. On the one hand, since 1906, an increasing percentage of all biological scientists (excluding the health, agricultural, forestry, and food sciences) featured in successive editions of the *American Men (and Women) of Science* have identified themselves as "physiologists." In an ordinal ranking, biochemistry and physiology by mid-century replaced botany and zoology as the most populated fields of the biosciences, ranks they retained through the 1980s (see Table 18.1).[43] This indicator would suggest that significant numbers of bioscientists are continuing to identify themselves as physiologists, even as the life sciences have become more specialized. However, my longitudinal study of the fields of U.S. doctoral recipients in the biosciences indicates a decline since World War II in the percentage of newly minted PhDs who identify themselves as physiologists, as well as a drop in the ordinal rank of that field (see Table 18.2 and Figure 18.2). Until about 1970 (except for a dip during World War II), the numbers of all doctorates obtained at U.S. universities, as well as the numbers of doctorates in the biosciences and in physiology and the numbers of self-identified bioscientists and physiologists in the successive editions of the *American Men (and Women) of Science* all experienced roughly similar exponential growth. If anything, the annual growth of physiological doctorates was slightly higher than the growth of all doctorates from 1920 to 1940

[41] Long Range Planning Committee [of the APS], "What's Past Is Prologue: A 'White Paper' on the Future of Physiology and the Role of the American Physiological Society in It," *The Physiologist*, 33 (1990), 161–80, at 176–7.

[42] A rich historiography has emerged for many of the new twentieth-century biomedical disciplines such as immunology, ecology, biochemistry, biophysics, genetics, or molecular biology, yet a sociology of relationships between disciplines and "scientific specialities" remains underdeveloped. See Harriet Zuckerman, "The Sociology of Science," in *Handbook of Sociology*, ed. Neil Smelser (Newbury Park, Calif.: Sage, 1988), pp. 511–74, at p. 541.

[43] The disciplinary categories of Table 18.1 were defined largely by James McKeen Cattell, editor of the first seven editions of *American Men of Science* (1906–44). Folding newer disciplines marked by the emergence of increasingly specialized professional societies onto a classical Comtean hierarchy of the sciences, Cattell constrained his biographees' self-designations of discipline to a limited number of categories. The basic structure of these categories was retained in later editions of the *AMWS*. See Michael M. Sokal, "Stargazing: James McKeen Cattell, *American Men of Science*, and the Reward Structure of the American Scientific Community, 1906–1944," in *Psychology, Science, and Human Affairs: Essays in Honor of William Bevan*, ed. Frank Kessel (Boulder, Colo.: Westview Press, 1995), pp. 64–86. The slight increase in the percentage of life scientists identifying themselves as physiologists in the 1989 edition might reflect the increasing frequency with which life scientists in that edition placed themselves in more than one field. In that edition, life scientists on average listed themselves under 1.2 different fields.

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Table 18.1. *North American AMWS Bioscientists. Ordinal Rank by Self-Identified Field (Physiology in bold), Percentage of Total Below*

| 1906 | 1921 | 1933 | 1949 | 1960 | 1976 | 1989 |
|---|---|---|---|---|---|---|
| Zoology | Botany | Zoology | Zoology | Biochemistry | Biochemistry | Biochemistry |
| 24 | 24 | 19 | 16 | 22 | 20 | 20 |
| Botany | Zoology | Botany | Biochemistry | **Physiology** | **Physiology** | **Physiology** |
| 23 | 22 | 15 | 15 | **12** | **11** | **15** |
| Biology | Entomology | Entomology | Bacteriology | Zoology | Zoology | Microbiology |
| 13 | 11 | 12 | 13 | 10 | 11 | 10 |
| Pathology | Biology | **Physiology** | **Physiology** | Bacteriology | Botany | Ecology |
| 10 | 9 | **11** | **12** | 9 | 9 | 7 |
| Bacteriology | Bacteriology | Bacteriology | Botany | Biology | Microbiology | Genetics |
| 9 | 9 | 11 | 11 | 8 | 8 | 7 |
| **Physiology** | **Physiology** | Biochemistry | Entomology | Entomology | Ecology | Zoology |
| **8** | **7** | 10 | 10 | 7 | 8 | 6 |
| Entomology | Anatomy | Biology | Biology | Microbiology | Biology | Botany |
| 8 | 7 | 8 | 7 | 6 | 8 | 6 |
| Anatomy | Biochemistry | Anatomy | Plant Physiology | Botany | Genetics | Immunology |
| 8 | 7 | 5 | 4 | 6 | 6 | 6 |
| Paleontology | Neuroscience | Plant Physiology | Anatomy | Anatomy | Neurosciense | Molecular Biology |
| 4 | 2 | 3 | 4 | 4 | 4 | 6 |
| Physiology Chemistry | Plant Physiology | Genetics | Genetics | Genetics | Entomology | Biology |
| 3 | 2 | 2 | 3 | 4 | 4 | 6 |
| Neurology | Embryology | Neuroscience | Ecology | Plant Physiology | Cytology | Biophysics |
| 2 | 1 | 2 | 1 | 4 | 4 | 5 |
| Embryology | Microbiology | Microbiology | Embryology | Neuroscience | Anatomy | Entomology |
| 2 | 1 | 1 | 1 | 3 | 4 | 4 |
| 1,230 | 2,800 | 5,600 | 10,500 | 21,300 | 40,000 | 46,100 |

*Note:* Last row is estimated total number of bioscientists in given *AMWS* edition.

*Sources: American Men and Women of Science*, 1st, 3rd, 5th, 8th, 10th, 13th, and 17th editions. Fields self-identified by entrants into categories established by James McKeen Cattell and successive editors. Proportion of entrants self-identifying themselves into two or more bioscience fields gradually increases, rising to 22% by the seventeenth edition. Estimates based on sample sizes of 4,000 (100% of all entries) in the first edition; 3,800 (40%) in the third; 3,140 (14%) in the fifth; 3,330 (7%) in the eighth; and 6,000 (7%) in the tenth. Subject indices in the thirteenth and seventeenth editions enabled counts of 100% of 110,000 and 128,500 entries, respectively. Clinical sciences such as medicine, ophthalmology, pediatrics, surgery, and others, as well as agriculture, forestry, and food technologies, are not included in these biosciences. See Sokal, "Stargazing," and Kessel, *Psychology, Science, and Human Affairs*, pp. 64–86, for procedures used by Cattell in classifying entrants.

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Table 18.2. *Bioscience Doctorates from U.S. Universities. Ordinal Rank by Field (Physiology in bold), Percentage of Total Below*

| 1920–4 | 1932–6 | 1945–9 | 1958–62 | 1970–4 | 1981–5 | 1993–7 |
|---|---|---|---|---|---|---|
| Botany | Zoology | Zoology | Biochemistry | Biochemistry | Biochemistry | Biochemistry |
| 26 | 29 | 29 | 22 | 18 | 17 | 16 |
| Zoology | Botany | Botany | Microbiology | Microbiology | Microbiology | Molecular Biology |
| 22 | 20 | 21 | 16 | 12 | 9 | 12 |
| Misc. Biology | Misc. Biology | Biochemistry | Zoology | Biology | Biology | Biology |
| 16 | 15 | 16 | 14 | 10 | 8 | 9 |
| **Physiology** | **Physiology** | **Physiology** | Botany | **Physiology** | **Physiology** | Microbiology |
| **15** | **15** | **14** | 9 | **10** | **7** | 8 |
| Biochemistry | Biochemistry | Microbiology | **Physiology** | Zoology | Pharmacology | **Physiology** |
| 9 | 10 | 14 | **9** | 10 | 7 | **5** |
| Microbiology | Microbiology | Misc. Biology | Entomology | Entomology | Molecular Biology | Cellular Biology |
| 9 | 9 | 5 | 8 | 6 | 6 | 5 |
| Anatomy | Anatomy | Anatomy | Genetics | Botany | Ecology | Ecology |
| 4 | 5 | 2 | 7 | 6 | 5 | 4 |
| | | | Plant Physiology | Genetics | Zoology | Genetics |
| | | | 4 | 4 | 5 | 4 |
| 699 | 2,077 | 2,197 | 5,670 | 16,356 | 18,429 | 25,904 |

*Note:* Last row is total number of bioscience dissertations per half-decade.

*Sources:* 1920–49 from Lindsey R. Harmon and Herbert Soldz, *Doctorate Production in United States Universities, 1920–1962* (Washington, D.C.: National Academy of Sciences–National Research Council, 1963), who divide the biosciences into seven fields and themselves assign dissertations to given fields; 1958–97 from Fred D. Boercker, *Doctorate Recipients from United States Universities, 1958–1966* (Washington, D.C.: National Academy of Sciences, 1967), and Fred D. Boercker, *Summary Report 1970–: Doctorate Recipients from United States Universities* (Washington, D.C.: National Research Council, 1970–1997), both of which are based on the annual "Survey of Earned Doctorates," in which authors of bioscience dissertations self-identified from a selection of 17 fields in 1958, which was expanded to 26 by 1997.

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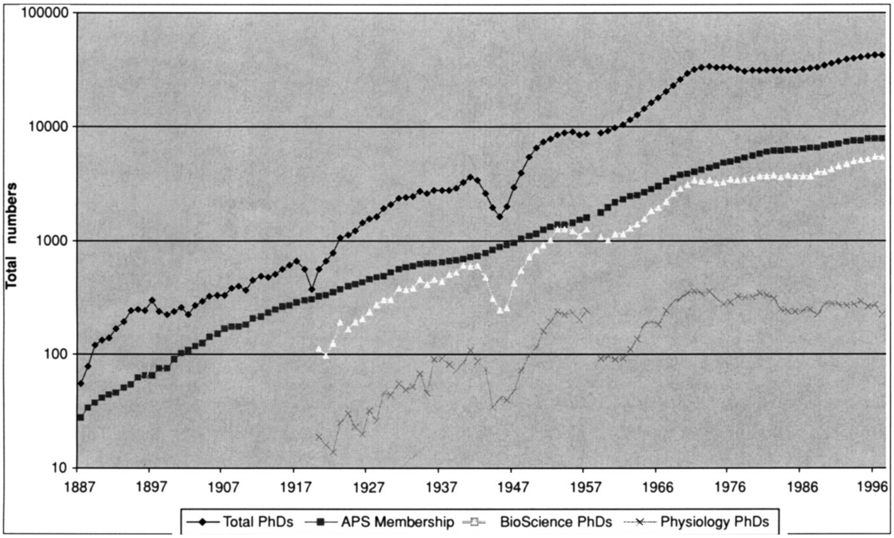


Figure 18.2. Physiology in the United States, 1887–1997, Annual Indicators. *Sources:* Brobeck, Reynolds, and Appel, *History of the American Physiological Society*, p. 302; *The Physiologist*, 1987–1997; Lindsey R. Harmon, *A Century of Doctorates: Data-Analysis of Growth and Change* (Washington, D.C.: National Academy of Sciences, 1978); and also sources for Table 18.2.

(9 percent versus 7 percent) and from 1961 to 1971 (13 percent versus 12 percent). Yet, since 1971, growth of new physiological doctorates has stagnated and even declined, whereas growth for all PhDs (1 percent) and all bioscience PhDs (2 percent) has continued even though it has slowed considerably from previous rates. Another measure of the identity of physiology as a discipline – membership in the American Physiological Society – has continued to grow exponentially over the past century. Yet the annual rate of increase has declined from about 6 percent annual growth prior to 1971 to only 2 percent after that date. As a professional identity in North America, physiology has become less robust over the course of the twentieth century.

Likewise, since 1900, physiology departments at medical schools have never served as the primary site of employment for North American physiologists. A recent APS study found that only about one-third of U.S. medical school faculty with doctorates in physiology hold appointments in departments of physiology. Among medical school faculty with appointments in departments of physiology, only about half self-identify themselves as "physiologists" (12 percent labeled themselves biochemists, 6 percent pharmacologists, 5 percent biologists, and 4 percent biophysicists).[44] My longitudinal study of employment of *AMWS* physiologists reveals a similar pattern of widely distributed employment (see Table 18.3). With remarkable consistency

[44] Marsha Lakes Matyas and Martin Frank, "Physiologists at US Medical Schools: Education, Current Status, and Trends in Diversity," *The Physiologist*, 38 (1995), 1–12.

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Table 18.3. *Employment of North American AMWS Physiologists. Ordinal Rank by Site, Percentage of Total Below*

| 1906 | 1921 | 1933 | 1949 | 1960 | 1976 | 1989 |
|---|---|---|---|---|---|---|
| Med. School[a] | Med. School | Med. School | Med. School | Med. School | Med. School | Med. School |
| 43 | 53 | 57 | 60 | 44 | 54 | 50 |
| Arts & Sci. | Arts & Sci. | Arts & Sci. | Arts & Sci. | Arts & Sci. | Arts & Sci. | Arts & Sci. |
| 33 | 28 | 25 | 23 | 25 | 35 | 30 |
| Government | Corp./Found. | Hospital | Government | Government | Government | Corp./Found. |
| 4 | 5 | 7 | 8 | 16 | 8 | 9 |
| Hospital | Hospital | Corp./Found. | Hospital | Corp./Found. | Corp./Found. | Government |
| 4 | 4 | 3 | 5 | 8 | 6 | 6 |
| Corp./Found.[b] | Government | Government | Corp./Found. | Hospital | Hospital | Hospital |
| 3 | 3 | 1 | 1 | 6 | 1 | 2 |

[a] Includes medical and veterinary schools.
[b] Includes corporations and philanthropic foundations.
*Sources:* Same as for Table 18.1 using entrants self-identified as "Physiologists."

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over the twentieth century, only about half of American self-designated physiologists have worked in medical or veterinary schools; about one-quarter have been located in departments of biology, zoology, or biochemistry in university or college faculties of Arts and Sciences; and the remainder have been employed by governmental agencies or research establishments, private foundations, corporate research facilities, or hospitals.[45]

Such data might support Weiss's claim that physiology in the twentieth century has become more of an attitude than a discipline with traditional nineteenth-century disciplinary forms. Or they might suggest that physiology, if it still exists in the twentieth century, has become what may be called a supradiscipline. The 1958 APS self-study concluded that "physiologists are characteristically dispersed through many branches of biological science."[46] In this dispersal, many have adopted the vocational identity and institutional structures of narrower, more specialized disciplines while still retaining a commitment to physiology as a "point of view" or a source for what Susan Leigh Star and James R. Griesemer have called "boundary objects."[47] Sectionalization of the APS reflects such double identities among American physiologists.[48] Perhaps the nineteenth-century Comtean divisions of the natural sciences have been preserved by twentieth-century scientists as supradisciplines, even as specialization and reconfiguration have radically differentiated the disciplinary landscape they inherited from the nineteenth century.[49]

[45] Employment surveys restricted to APS members taken since 1980 indicate that about 65% work in medical schools, with 10%–20% in Arts and Sciences faculties. See *The Physiologist*, 23 (1980), 18; 34 (1991), 79; 42 (1999), 402.

[46] Gerard, *Mirror to Physiology*, p. 2.

[47] Susan Leigh Star and James R. Griesemer, "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907–39," *Social Studies of Science*, 19 (1989), 387–420.

[48] Larger American disciplinary societies, such as the American Chemical Society and the American Physical Society, also became "sectionalized" over the course of the twentieth century. Although at times leaders of these two societies viewed specialization as a threat, levels of anxiety in these large organizations (currently over 160,000 and 40,000 members, respectively) never approached those experienced by the American Physiological Society. See Karl H. Reese, ed., *A Century of Chemistry: The Role of Chemists and the American Chemical Society* (Washington, D.C.: American Chemical Society, 1976); Harry Lustig, "To Advance and Diffuse the Knowledge of Physics: An Account of the One-Hundred-Year History of the American Physical Society," *American Journal of Physics*, 68 (2000), 595–636; David Kaiser, "Making Theory: Producing Physics and Physicists in Postwar America" (PhD diss., Harvard University, 2000), chap. 3.

[49] For suggestive, albeit diverse, reflections on the place of disciplines in twentieth-century science, see Lindley Darden and Nancy Maull, "Interfield Theories," *Philosophy of Science*, 44 (1977), 43–64; Pnina Abir-Am, "Themes, Genres and Orders of Legitimation in the Consolidation of New Scientific Disciplines: Deconstructing the Historiography of Molecular Biology," *History of Science*, 23 (1985), 73–117; Timothy Lenoir, "The Discipline of Nature and the Nature of Disciplines," in *Knowledges: Historical and Critical Studies in Disciplinarity*, ed. Ellen Messer-Davidow, David R. Shumway, and David J. Sylvan (Charlottesville: University Press of Virginia, 1993), pp. 70–102.

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# 19

# PATHOLOGY

*Russell C. Maulitz*

The development of pathology, with all its complex roots and branches, seems harder to account for than for most of the disciplines we take to underlie medical education and "scientific medicine." The effort requires an array of cross-cutting bits of medical history, all working at different levels. A full account needs to include pathological museums and the collecting impulse, and the development of disease concepts as far as those concepts came to be localized in bodies (or, for that matter, generalized in bodies). It also needs to include the developing patterns of "disease itself," however we mean that,[1] for "the Seats and Causes of disease" that Giovanni Battista Morgagni and later doctors described in bodies depended as much on changing epidemiological patterns as on the tools with which the doctors variously confronted the diseased. New diseases such as severe acute respiratory syndrome (SARS) or acquired immune deficiency syndrome (AIDS), human ritual behaviors, family lives, *Homo sapiens'* relations with other animals and with natural environments, and (especially) urbanization with its attendant concentration of illnesses in hospitals – all helped condition the "pathology" that would be "seen" by medical observers.

It is continually fascinating to contemplate the paradoxes buried in these relationships between bodies, tools, and diseases – the tussle between epistemic and epidemiologic "reality." In some sense, we can see any one of these players – bodies, tools, and diseases – at any point in historical time, as mediating between the two others. Perhaps that complexity is why the history of pathology has been given a rather wide berth by most historians.[2]

[1] On the critical importance of the element of time, see Chapter 1, "Pathological Time," in Harry Marks's forthcoming monograph on the evolution of the notion of change over time in disease theory, *Time and Disease in Modern Medicine* (unpublished manuscript).

[2] Russell Maulitz, "Review of Cay-Rüdiger Prüll, *Traditions of Pathology in Western Europe: Theories, Institutions and Their Cultural Setting* (Herbolzheim: Centaurus, 2003)," *Bulletin of the History of Medicine*, *79* (2005), 604–6. See also the brief but excellent historiographic essay by Prüll in Cay-Rüdiger Prüll, ed., *Pathology in the 19th and 20th Centuries: The Relationship between Theory*



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Only two high-level facts are clear about the evolution of pathology as a discipline. The first, the principal focus of this chapter, is that an important practice and "science" of pathology grew up in Western cultures over the past two hundred years. The second, which we mention here only in passing, is that in recent decades pathology has entered an odd sort of decline. Pathology as a discipline, as a touchstone of scientific medicine, remains a mainstay of medical education in ways somewhat similar to anatomy and physiology.[3] But pathology's other traditional bases, procedurally in the autopsy and epistemologically in the role of diagnostic arbiter, have been substantially diminished.[4] Those bases were vital for hundreds of years; only recently, after the two world wars of the twentieth century, did they seem to become fossilized.[5] In many countries, fewer autopsies are now being conducted – to reduce cost, to avoid litigation risks, and to maximize returns on investments within pathologist groups.

The pathologist is simultaneously morphing into a manager, typically of chemical autoanalysis, serology, radioimmunoassay, and microbiology laboratories. But ironically, as the discipline declines, it is becoming more self-aware. In 2005, the president of the Royal College of Pathologists could define his field as "the hidden science at the heart of modern medicine, vital for the diagnosis and clinical management of disease," while listing no category of professional positions in anatomical pathology.[6] And indeed the description is, at multiple levels, quite apt. In epistemic terms, the pathologist is the person who finds the pathology – the unseen plane or level of analysis within which one notion of disease is mapped to another.[7]

We might periodize pathology in terms of a prehistory and three modern periods – perhaps then slipping into "posthistory." What I refer to as the prehistory continued until the late eighteenth and early nineteenth centuries,

*and Practice* (Sheffield: EAHMH, 1998), pp. 1–9. Significant historiographic assistance is also available online at the site maintained by Axel Bauer, International Bibliographic Guide to the History of Pathology, at http://www.uni-heidelberg.de/institute/fak5/igm/g47/bauerpat.htm (last accessed November 5, 2005).

3 See the companion article by Susan C. Lawrence, Chapter 15, this volume.

4 Jack Hasson, "Medical Fallibility and the Autopsy in the USA," *Journal of Evaluation in Clinical Practice*, 3 (1997), 229. See also Cay-Rüdiger Prüll, "German Pathology and the Defence of Autopsy since 1850," in *Traditions of Pathology in Western Europe: Theories, Institutions and Their Cultural Setting*, ed. Cay-Rüdiger Prüll (Neuere Medizin- und Wissenschaftsgeschichte, Quellen und Studien, vol. 6.) (Herbolzheim: Centaurus, 2003), pp. 139–62.

5 Hasson, "Medical Fallibility and the Autopsy since 1850."

6 Sir James Underwood, http://www.rcpath.org.uk/index.asp (accessed June 11, 2005). In this presentation, a long list of professional niches includes none for anatomical pathology. Underwood does, however, provide a section for forensic pathology – traditional final domicile of the autopsy – noting that it is perhaps the most "high profile" but has become "relatively small" in terms of positions. This also implies that sitting in courtrooms is, for denizens of this last bastion, as important as cutting up and understanding what lies inside dead bodies.

7 Russell Maulitz, "Review of Cay-Rudiger Prüll, *Traditions of Pathology in Western Europe*"; Russell Maulitz, "The Pathological Tradition," in *Companion Encyclopaedia of the History of Medicine*, ed. W. F. Bynum and Roy Porter (London: Routledge, 1993), pp. 169–91.

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when the first "realistic mystification" of disease theory shifted the focus of scrutiny from the observable "normal" body to its tissue planes.

A further shift occurred sometime in the latter half of the nineteenth century, when improved microscopes allowed the focus to shift again from tissues to individual cells. A third shift occurred when pathologists in the twentieth century captured the business of analyzing both blood and subcellular components in the body. This is sometimes called "clinical pathology," and dominates today. Finally, just possibly, is a still more recent shift, begun perhaps as recently as the 1990s – a posthistory of pathology as genomic medicine seeks a place in the sun. In a postlude, this chapter provides some of the evidence, currently sketchy, for the possibility of this final shift.

In what follows, I suggest a way of thinking about the origins and subsequent directions taken by the stages of (a) prehistory, (b) tissue pathology, (c) cellular pathology, (d) clinical pathology, and (e) present-day paradigms. Each of these models of pathology overwrote its older predecessor without obliterating it. Because each model added a new layer of "seeing" diseased bodies, practitioners in the present day can (and do, and must) still speak, for example, of the gross appearance of diseased organs or disturbed tissue planes. The history of medical thought is deeply conservative, at least that emerging from the pathology community. Hence the history of pathology is not a series of destructive revolutions but a palimpsest, a larding of newer ways of seeing normal and abnormal anatomy on top of older ways.[8]

We can then add another framing device, for to understand how pathology has progressed, both as disease theory and discipline, we can conceptualize each period of its development in terms of a paired step sequence. In each case, some notion of body structure and its morbid derangements, a "theoretical phase" based on anatomical observation but not yet disease observation, has preceded a more performative, clinical phase during which changes in social and institutional settings permitted the entrenchment and elaboration of the initial disease theory, projecting it into a particular clinical context. Again and again, this biphasic development of pathology led to new subdisciplines of pathology, as well as new roles for pathology and its practitioners.[9]

## PATHOLOGY'S PREHISTORY

The prehistory of Western pathology – the practice of pathology before there were "pathologists" – began in the early modern period. Anatomical

[8] For more on this kind of cumulation, see John V. Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine* (Chicago: University of Chicago Press, 2001).

[9] Of course, as we sometimes forget, some theoretical notions never panned out. On indeterminism and the role of luck in the various sciences, see Nassim Taleb, *Fooled by Randomness: The Hidden Role of Chance in Life and in the Markets*, 2nd ed. (New York: Random House, 2005). Of course, it is fair to say that, in medical science to a greater extent than in some of the human sciences, the "success" of a theory would likely be enhanced by its correspondence to patterns of illness seen in nature at a given time; that is, "reality-testing" would perhaps be a bit more immediate (though not always).

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dissection for medical instruction in major Southern European centers began to combine the interest in storing knowledge, harbored by academically trained physicians, with the interest in the minute structures of bodies, harbored particularly by apprentice-trained surgeons. Soon there was added an interest in disease changes, so one might speak of three axes of discourse: an *x* axis, that of preserving information about the body in order to further the creation of a corpus of medical theory; a *y* axis, that of observing the constituent parts of dead bodies in order to pin down certain planes or structures in the human frame and hence to constrain theory; and a *z* axis, to further constrain theory and practice by tethering certain types of diseases to certain parts of the body as observed in multiple cases over time.[10]

Foremost among the practitioners of this new "xyz" of medicine was Giovanni Battista Morgagni, a notable "pan-European" who followed in the footsteps of William Harvey at the University of Padua and corresponded with scientific societies from Italy to Britain.[11] Morgagni set out to codify organ pathology, as the organs of the human body might provide a way to obtain a better understanding of disease processes. Anchoring disease in specific organs of the body provided for anatomically localized approaches to diseases such as cancer, as opposed to the global, humoralistic approaches to diagnosis and treatment still dominating the thinking of most physicians.

Late in his career, and only a generation away from the French anatomical pathologists of the early nineteenth century, Morgagni was ready to launch his magnum opus, summarizing his life of dissection in *De sedibus et causis morborum per anatomen indagatis* (*On the Seats and Causes of Diseases, Investigated by Anatomy*). In this 1761 work, quickly translated into English and other major languages, Morgagni published seventy letters describing about seven hundred cases. Each case depicted in some way the manner in which disease might best be understood in terms of its anatomical substrate.

A "theory of organs" of course could be traced backward to the great tradition of anatomical dissection that accompanied the Southern and Northern Renaissances of Europe. What Morgagni added was the critical *z* axis, the systematic demonstration of disease over time having "this effect" in "this patient" and "that effect" in "that patient," reifying organ pathology by means of repetitive demonstration, a "performance-in-the-world" of its importance as a way of understanding illness and disease. The accretion of case after case, description upon description, formed the bedrock of a disease theory based on anatomical localization.

Thus Morgagni, who was born just four years after William Harvey's death and lived until the year of Xavier Bichat's birth, became the key transition

[10] Russell Maulitz, "Anatomie et anatomo-clinique," in *Dictionnaire de la pensée médicale*, ed. D. Lecourt (Paris: Presses Universitaires de France, 2004), pp. 47–51. In *Time and Disease in Modern Medicine*, Harry Marks demonstrates how the *z* axis of which we speak here, that of disease, is actually that of "disease-over-time"; that is, how serial representations of disease, and the time changes documented therein, informed physicians' thought more than mere static snapshots.

[11] Maulitz, "Anatomie et anatomo-clinique." This brief section on Morgagni is based largely on that account.

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figure between the organ-based anatomy of the seventeenth and eighteenth centuries and the tissue-based anatomy of the nineteenth. In this way, Morgagni as an individual demonstrated the performativity that would collectively characterize the anatomo-clinique of the ensuing century. The next layer, the next conceptual link to fall into place, was the tissue-based anatomy of the next generation.

## FIRST TRANSITION: TISSUE PATHOLOGY

In a sense, of course, it is something of a conceit to label as "prehistory" everything that occurred before the British, French, and other early heralds of tissue pathology. But it is a conceit based on an idea about professional roles and the perquisites of teaching: *This* person, trained *this* way, can now teach *this* discipline. Not until the naked-eye appearance of organs, and their attendant morbid appearances, came to read through an esoteric wisdom of the body, could a person honestly claim to be a "pathologist." And indeed, it was not accidental that in early nineteenth-century Europe and perhaps a generation later in the United States, separate academic courses of "pathological anatomy," "surgical pathology," or "medical pathology" (sometimes two or more of them in the same institution) would begin to flourish.[12]

The notion of a pathology of tissues was, in essence, the privileging of a taxonomy of layers within the body as the primary seats of the processes of this or that disease. As a number of authors have suggested, tissue pathology was initially an eighteenth-century product and a multicentric one.[13] In the English context, John Hunter, Matthew Baillie, and a number of their students, aided by an early form of hospital-based clinical observation, began to speak of the individual tissues of the body in ontological, or essentialist, terms – as the basic building blocks of anatomy in its normal and deranged states. As Georges Canguilhem, Christiane Sinding, and others have pointed out, however, this early terminology of tissues awaited its ultimate institutional and cultural performative setting, and that setting was the Paris medicine accurately limned in the mid-twentieth century by Michel Foucault, Erwin Ackerknecht, and others.[14]

[12] We recall here that physicians and surgeons were in some respects (and more in Europe than in the United States) still officially lords and masters of separate domains. Yet pathological anatomy was surely one bridge between the two. See Russell Maulitz, *Morbid Appearances: The Anatomy of Pathology in the Early Nineteenth Century* (Cambridge: Cambridge University Press, 1987). On the American picture, see Russell Maulitz, "Pathology," in *The Education of the American Physician*, ed. Ronald Numbers (Berkeley: University of California Press, 1979), pp. 122–42.

[13] Othmar Keel, *L'avènement de la médecine clinique moderne en Europe, 1750–1815: Politiques, institutions et savoirs* (Montréal: Presses Universitaires de Montréal, 2001).

[14] Michel Foucault, *Naissance de la Clinique* (Paris: Presses Universitaires de France, 2003); Erwin Ackerknecht, *Medicine at the Paris Hospital* (Baltimore: Johns Hopkins University Press, 1967); Georges Canguilhem, *Le normal et le pathologique* (Paris: Presses Universitaires de France, 2005).

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Beginning at the turn of the century, in the crowded hospitals of post-Revolutionary Paris, a new tradition of "pathological anatomy," based on tissues and bridging the "external pathology" of the surgeons and the "internal pathology" of the physicians, was adumbrated by Xavier Bichat (1771–1802), then further developed by Gaspard Bayle (1774–1816), Théophile Laennec (1781–1826), and others. This tradition did not represent the old theoretical and natural historical "internal" pathology still taught in somewhat creaky fashion by various lights (including Philippe Pinel) of the Paris faculty. Rather, it grew up in the interstices of the system as internal medicine and surgery, the practices of two separate practitioner communities, were slowly melded: in the several private courses in pathological anatomy taught by Laennec and others, in the memoirs presented to the equally diverse newly formed medical societies, and in the contributions found in the pages of journals such as Jean Corvisart's (1755–1821) and Alexis Boyer's (1757–1831) authoritative *Journal de médecine, chirurgie, pharmacie.*

No doubt one reason for the lugubrious pace at which Bichatian tissue pathology was incorporated was the fact that, although it partook of some of the localism of official, surgically oriented "external" pathology, it was almost as nonvisual as the old general "internal" pathology of the physicians. Given the visual stress in gross anatomy and the visual emphasis in later pathology, it is ironic that general anatomy in the Paris clinic was largely verbal. By locating disease in tissues rather than organs, the new pathology project tended to substitute words for pictures.[15]

A variety of day-to-day clinical activities propelled the performative phase of the new pathological anatomy. In the case of Laennec, it was the elaborate ritual of clinico-pathological correlation, comparing in great detail the antemortem findings afforded by physical diagnostic tools such as percussion and auscultation with the postmortem findings afforded by the ever more elaborate dissection series permitted in Napoleonic and Empire Paris.[16] In the case of men such as Auguste Chomel (1788–1858) or his younger colleague (and erstwhile pupil) Pierre Louis, it was the almost manic attention to a strobe-like sequence of minute autopsy observations allowing, finally, for a sort of summative explanation of how disease "fitted" with the pathology of tissues as evidenced by repetitive variations on the theme of "get this disease, die in this way, the morbid appearances of tissues thus displayed."[17] A classic example of this performativity, for a particular disease that was increasingly prevalent, increasingly recognized, or both, was the affection of the pleural surfaces of the lungs, or pleuritis. Adrian Wilson has demonstrated how this disease exemplified the new tissue pathology as a lens through which to view the tuberculous and other serositides of early nineteenth-century Paris, even

[15] The preceding section is adapted from Maulitz, *Morbid Appearances*, with permission.
[16] Maulitz, *Morbid Appearances*; Jacalyn Duffin, *To See with a Better Eye: A Life of R. T. H. Laennec* (Princeton, N.J.: Princeton University Press, 1998).
[17] Russell Maulitz, "In the Clinic: Framing Disease at the Paris Hospital," *Annals of Science*, 47 (1990), 127–37; Marks, *Time and Disease in Modern Medicine.*

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though "pleurisy" itself, like the basic *concept* of "tissues," was ancient.[18] In a newer essay, Adrian Wilson addresses the apparent paradox between continuity and change in pathology. His important article in a volume dedicated to Roy Porter compares that scholar's views of pathology with those of Michel Foucault and reaches much the same conclusions as the present chapter. Wilson describes the array of conditions needed to satisfy the requirements for the emergence of a science of pathology and notes that with "the Paris *Ecole de Santé*, these conditions were met – not because this had been intended, but simply as a result of the fortuitous triple combination of practices and circumstances that emerged there."[19]

From the 1830s, beginning in Paris and continuing across many national boundaries – and indeed bouncing back to England in the works of clinician-investigators such as Robert Carswell, Thomas Hodgkin, and Richard Bright – the anatomical tradition soon came to be marked by attempts to localize disease in both organs and tissues, together with methodical observation of large numbers of patients in systematic series, often using statistical methods. Using student *rédacteurs*, Chomel probably took this notion further than any of his French counterparts. But in the hands of Chomel himself, of Pierre Louis, or of their English-speaking disciples, the project always combined several notions: of localizing disease in highly specific bodily locations; of tissues throughout the body occupying a sort of middle-ground position between isolated solid organs such as the liver and the "humors" represented by the bloodstream; of needing to study large numbers of cases of patients suffering from affections of these tissues; and (usually) of requiring a system of population-specific wards for the elaboration and dissemination of anatomical knowledge in the clinical context. The result was not just a new system of pedagogy but a new way of thinking about the relationships between living substructures in disease states. Thus, to take the most striking and pervasive example, within a given moribund patient, the presence of grossly observable tubercles – the large, granulomatous morbid appearances of phthisis or tuberculosis – in a variety of serosal membranes was a way of understanding how someone with tuberculous pericarditis might also be predicted to have tuberculous peritonitis, or vice versa. This was a powerful, esoteric, yet intensely realistic way of understanding sympathies and correspondences between diseased tissue planes within the body. It was a way of performing prognosis, the clinical prediction of disease course. And it was a way of forming a new field of inquiry.

Thus pathology was propelled into a form that might now be called a discipline. It was a fusion of anatomical, clinical, and educational principles, each performed on the stages of the major capitals of Europe and, after the

[18] Adrian Wilson, "On the History of Disease-Concepts: The Case of Pleurisy," *History of Science*, 38 (2000), 271–319.

[19] Adrian Wilson, "Porter versus Foucault on the 'Birth of the Clinic'," in *De Omni Scribili: Essays in Memory of Roy Porter*, ed. Roberta Bivins and John V. Pickstone (Basingstoke: Palgrave, 2007).

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mid-nineteenth century, in the United States as well. It was to Paris that American students of pathological anatomy and clinical medicine would be drawn in the second quarter of the nineteenth century. Historian John Warner has chronicled the manner in which these "memories of Paris" powerfully bonded such students into a certain worldview about the clinic.[20] For these students, pathology had become, quite literally, performative, as the practitioner followed the patient from diagnosis to deathbed to autopsy table.

## SECOND TRANSITION: CELLULAR PATHOLOGY

As students were flocking to Paris in the mid-nineteenth century, already a new conceptual stratum was being formed atop tissue pathology: the theory of the cell. As Susan Lawrence discusses (see Chapter 15, this volume), a number of observers in Britain, the German lands, and elsewhere were using improved microscopes to discern finer structures than the mere lamellar tissues that had sufficed for the first disciplinary generation of pathologists. The theoretical phase of cellular pathology, as this new form of pathology was dubbed by Rudolf Virchow in Germany, lasted for much of the middle third of the nineteenth century. During that phase, microscopists seized on earlier work from France and elsewhere suggesting that "cells," not "tissues," represented the fundamental functional and structural units of life. And if life, then disease, as Virchow was to state first in 1855.[21] In an article in the *Archiv* that he created as a platform for the new pathological science, and in his classic *Cellular Pathology* (1858), Virchow laid out an agenda for the study of disease.[22] His agenda would move the theory of cellular anatomy from the theoretical bases supplied by Matthias Schleiden, Theodor Schwann, and others into a performative phase of autopsies and laboratories.[23]

The grand, programmatic vision projected by Virchow in the 1850s, whereby tissue pathology was re-formed into cellular pathology, led to an entire industry of scientific medicine.[24] What lent performativity to this project was the specificity Virchow and his disciples imposed on the theoretical agenda. Two examples will suffice. The first was the work that Virchow

[20] This discussion of Chomel and his progeny is adapted from Maulitz, "In the Clinic." For the American students' view, see John Harley Warner's important *Against the Spirit of System: The French Impulse in Nineteenth-Century American Medicine* (Princeton, N.J.: Princeton University Press, 1997), and Matthew Ramsay's review thereof, *Times Literary Supplement*, no. 4978, August 28, 1998, p. 8.

[21] Rudolf Virchow, "Cellular-Pathologie," *Archiv für Pathologische Anatomie und Physiologie und für klinische Medicin*, 8 (1855), 3–39.

[22] Rudolf Virchow, *Die Cellularpathologie in ihrer Begründung auf physiologische und pathologische Gewebelehre* (Berlin, 1858). For its staying power, see Erwin H Ackerknecht, "Zum 100. Geburtstag von Virchows 'Cellularpathologie': ein Rückblick," *Virchows Archiv*, 332 (1959), 1–5.

[23] Lawrence, Chapter 15, this volume.

[24] On the rise of this project in Germany, see Axel Bauer, "Die Institutionalisierung der Pathologischen Anatomie im 19. Jahrhundert an den Universitäten Deutschlands, der deutschen Schweiz und Österreichs," *Gesnerus*, 47 (1990), 303–28.

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himself performed on tumors. The increasingly crowded urban social conditions of the nineteenth century, the century of infectious diseases, formed a laboratory for pathologists. It was the pathologist who claimed to be able to distinguish infections, notably chronic ones such as tuberculosis, from other conditions such as indolent cancers; in *Die Krankhafte Geschwültse* (Disease Related Tumors), published in 1863, Virchow discussed just this matter of the differentiation of cancer and other disorders. When a cancer could be seen under the microscope, from the late nineteenth century, the clinical impulse was to surgically extirpate the diseased tissue structures and confirm the presence of histologically (that is, cell-based) "clean margins."[25] There was and is controversy about whether Virchow correctly diagnosed, for example, the cancer of Frederick II, emperor of Germany and king of Prussia, but that is beside the present point. Microscopic diagnostic techniques were coming to be accepted as routine in medical settings: Cellular pathology had morphed into a performative discipline.[26]

Another telling example was Julius Cohnheim's work from the 1860s on the subject of inflammation. Because infection, cancer, and other processes could all result in the final common pathway of inflamed tissue, some of Virchow's students, most notably Cohnheim, problematized the process whereby cells were understood to become inflamed.[27] Starting with the notion that inflammatory disease must occur in some fashion at the cellular level, Cohnheim nonetheless extended Virchow's intensely "localist" views. Using innovative vivisectionist techniques, he demonstrated that inflammatory cells may migrate from afar, summoned by some sentinel call that the local cells needed help in destroying the invader.[28]

## THIRD TRANSITION: CLINICAL PATHOLOGY

The "physiological pathology," exemplified by Cohnheim's notions of inflammation, invoking both humoral "action-at-a-distance" and local cellular explanations for disease processes, went beyond providing further performativity for the program of pathology; they made pathologists the source of disease understanding. While remaining largely rooted in the Virchovian paradigm, Cohnheim's physiological pathology provided a segue into the

[25] Rudolf Virchow, *Die Krankhafte Geschwultze* (Berlin: Hirschwald, 1863). Important historical works on this phase include W. I. B. Onuigbo, "The Paradox of Virchow's Views on Cancer Metastasis," *Bulletin of the History of Medicine*, 36 (1962), 444–9; L. J. Rather, *The Genesis of Cancer* (Baltimore: Johns Hopkins University Press, 1978).

[26] J. M. Weiner and J. I. Lin, "In Defense of Virchow: Discussion of Virchow's Pathological Reports on Frederick III's Cancer," *New England Journal of Medicine*, 312 (1985), 653.

[27] Julius Cohnheim, "Ueber Entzündung und Eiterung," *Archiv für Pathologische Anatomie und Physiologie und für klinische Medicin*, 40 (1867), 1–79.

[28] Russell Maulitz, "Rudolf Virchow, Julius Cohnheim, and the Program of Pathology," *Bulletin of the History of Medicine*, 52 (1978), 162–82.

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next era of pathology, which we shall call the neohumoralism of laboratory or clinical pathology.

In the late nineteenth century, cutting across multiple disciplines, including physiology,[29] bacteriology,[30] and pathology itself, there emerged an opposition between two tendencies in scientific medicine. One, typified by "pure" Virchovian cellular pathology and, arguably, a great deal of bacteriology, with its emphasis on culturing organisms out of abscessed nooks and crannies of the body, focused on specific locations to which disease might be traced. Thus *this* disease relates to *this* localized malignancy or *this* abscess full of bacteria. By the final quarter of the nineteenth century, however, an opposing, holistic, tendency had grown up in most of the emerging biomedical disciplines, impelled in some cases by clinicians' mistrust of the rising importance of the autopsy bench and laboratory.[31] In the case of pathology, the counterweight to Virchovian localization drew from two distinct taproots: a philosophy of clinical meaning and the empirical work of clinical chemists.

"Clinical meaning" sought to reintegrate the human body in its normal and diseased states, refusing to "see" the processes of disease in the highly atomized terms that seemed dictated by tissue pathology and more so by cellular pathology. This impulse, typified in Germany by an individual who was in many ways Virchow's direct successor, Ludwig Aschoff, sprang in some measure from the philosophical notion one observer has called the "hunger for wholeness" in medicine.[32] No doubt it sprang also from the more mundane notion, that the distance between the diagnostic vistas of clinicians at the bedside and pathologists at the bench needed to be narrowed, a notion probably made easier for those pathologists (like Aschoff) who were by now well entrenched in defined professional niches. Aschoff, importantly, was influenced by work from abroad on "wholeness," particularly that of Elie Metchnikoff in Russia, for whom phagocytic cells were the perfect nexus for relating cells to the host, whether the cells stemmed from the invader or from the lesion.[33]

As we mentioned earlier, there was another reason for the emergence of clinical pathology as a new layer atop cellular pathology: clinical chemistry. Diagnosis through the use of chemical analysis had begun in the era of Laennec and Chomel. At Guy's Hospital, London, in the 1820s and 1830s, Richard Bright showed that autopsies of patients with "dropsy," what might today be

[29] Gerald Geison, "Divided We Stand: Physiologists and Clinicians in the American Context," in *The Therapeutic Revolution: Essays in the Social History of American Medicine*, ed. Morris J. Vogel and Charles E. Rosenberg (Philadelphia: University of Pennsylvania Press, 1979), pp. 67–90.

[30] Russell Maulitz, "'Physician versus Bacteriologist': The Ideology of Science in Clinical Medicine," in Vogel and Rosenberg, *Therapeutic Revolution*, pp. 91–108.

[31] For two other examples, see Geison, "Divided We Stand"; Maulitz, "'Physician versus Bacteriologist'."

[32] Lazare Benaroyo, "Pathology and the Crisis of German Medicine (1920–1930): A Study of Ludwig Aschoff's Case," in *Pathology in the 19th and 20th Centuries*, pp. 101–13.

[33] Alfred Tauber and Leon Chernyak, *Metchnikoff and the Origins of Immunology: From Metaphor to Theory* (New York: Oxford University Press, 1991).

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termed heart failure, whose urine contained protein, often revealed abnormal kidneys. Steven Peitzman has described the association of these findings as "Bright's Disease," possibly the first disease entity for which the diagnosis depended on demonstration of a chemical abnormality. Bright's associates, John Bostock and George Owen Rees, were to measure the decreased albumin content of the blood and the urea in the serum of several of Bright's patients.

Yet, for many decades, clinical chemistry did not fuse with pathology. Manuals of diagnostic chemistry for physicians were published in the mid-nineteenth century, but until post-Virchovian pathologists assumed control of them in the following century, routine measurement of substances such as urea, uric acid, and glucose in urine and blood were more oddities than customary tasks. For chemical pathology, the "performative moment," bridging bench–bedside gap, reflected a triad of twentieth-century advances: first, the introduction of rapid chemical assays, as applied to small amounts of blood, particularly by Ivar C. Bang, Otto Folin, and Donald D. Van Slyke; second, the idea that specimens of blood could be obtained for laboratory analysis as a matter of routine practice; and third, the rise of diagnostic laboratories in hospitals, using automated machines for analysis. By now it was already the middle of the twentieth century. The ritual of submitting all types of specimens, fluids as well as solids, finally led to the vesting of clinical chemistry performance in pathology departments. This in turn ensured the continued prosperity of pathology as a discipline, long after the autopsy suffered its precipitous decline.[34]

As the importance of the autopsy waned, clinical chemistry became a centerpiece of the business of medicine and indeed the business of pathology. Such an evolutionary scenario can be discerned in the arc described by the department of pathology at the citadel of scientific medicine in the United States, Johns Hopkins University. The progenitor of pathology at the Johns Hopkins, William H. Welch, had been a key link between nineteenth-century European pathology and the newly ascendant medical education of the twentieth-century United States; he placed extraordinary value on training clinicians in laboratory medicine.[35] But Welch's successor a century later, though no doubt an accomplished scientist who could claim double-digit increases in research funding from year to year, was not primarily a physician-scientist with the MD-PhD degree combination that marked academic pathologists for much of the twentieth century. Rather, he was a

[34] Steven Peitzman and Russell Maulitz, "La fondazione della diagnosi," in *Storia del Pensiero Medico Occidentale: 3. Dall'Età Romantica alla Medicine Moderna*, ed. Mirko D. Grmek (Rome: Laterza, 1998), pp. 255–81. On the machine created for autoanalysis, see Leonard T. Skeggs, Jr., "Persistence and Prayer: From the Artificial Kidney to the Autoanalyzer," *Clinical Chemistry*, 46 (2000), 1425–36.

[35] Studies of Welch, in the wake of Abraham Flexner and his 1910 Report on medical education, are legion – as are studies of Flexner. An intriguing recent one is Angus Rae, "Osler Vindicated: The Ghost of Flexner Laid to Rest," *Canadian Medical Association Journal*, 164, no. 13, June 26, 2001. For the place of pathology in American medical education, see Maulitz, "Pathology," pp. 122–42.

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physician with a business administration degree: pathology, by century's end, had become Big Business.[36]

Another part of that business was surgical pathology. Over the twentieth century, the pathologist became the arbiter of frozen and other histological sections of tissue emanating from the surgical operating room. Negotiations between operating room and pathologist's suite, echoing earlier negotiations between bedside and bench, became particularly fraught with the adjudication of breast cancer diagnoses. As one reviewer put it, "a woman officially makes the transition to breast cancer at the moment that a surgical pathology report is finalized."[37]

## POPULAR FORENSIC PATHOLOGY

In the year 2000, a new pop-culture franchise, that of the *CSI* (Crime Scene Investigation) television empire, sprang into the consciousness of television viewers in the United States, gaining similar large audiences a year or two later in the United Kingdom, Germany, Spain, and no doubt elsewhere. The several *CSI* casts, along with a flurry of other programs such as *Crossing Jordan*, glorified the personal and professional lives of pathologists and other forensic investigators.[38] Observers would be forgiven for believing that such an efflorescence of interest somehow mirrored changes in the profession itself, but such was not the case.[39] At least in the United States, a mismatch emerged between the number of pathologists and the positions open to them, such that in January 2001, at the key Internet site for such opportunities, the total number of individuals seeking positions was 184 and the number of openings 116.[40] This "blogger" also cited a transparent public resource, the jobs board of the College of American Pathologists, where, in January 2006, the number of positions posted was 76.[41] Pathologists, if they were lucky, found managerial posts supervising a myriad of new technologists hired into

36 http://pathology.jhu.edu/department/letter.cfm, accessed January 22, 2006.

37 On surgical pathology, see C. H. Browning, "Pathology in Britain in the First Half of the Twentieth Century, with a Glance Forward," *British Medical Journal*, no. 561 (August 5, 1967), 359–62. On cancer, see Elliott Foucar, "The Breast Cancer Wars: Hope, Fear, and the Pursuit of a Cure in Twentieth-Century America," *American Journal of Surgical Pathology*, 27, no. 3 (March 2003), 417–19. Foucar's article is a pathologist's view of Barron H. Lerner's important monograph on the subject. See Barron H. Lerner, *The Breast Cancer Wars: Hope, Fear, and the Pursuit of a Cure in Twentieth-Century America* (New York: Oxford University Press, 2001).

38 Press release at http://www.allianceatlantis.com/corporate/press_media/AAC05_26.asp, accessed January 28, 2006.

39 On the "profession" of CSI, a growth industry for sure in the twenty-first century, see Hayden Baldwin, "How to Become a CSI," http://www.icsia.org/faq.html, accessed January 29, 2006.

40 "Update on the Pathology Job Market," posted at the Weblog of the Committee for the Improvement in the Pathology Job Market, http://members.tripod.com/∼philgmh/CIPJM.html and http://members.tripod.com/∼philgmh/pjmd.htm.

41 http://www.cap.org and http://www.healthecareers.com/site_templates/CAP/index.asp?aff = CAP& SPLD = CAP (requires registration), accessed January 29, 2006.

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paraprofessional roles. In 2001, forensic pathology was a small field with a dearth of new practitioners,[42] so unless the TV glamour has led to an as yet unchronicled upsurge, we must look elsewhere for pathology's future. Here we may be guided by our historical model – that at any time pathology was creatively divided between practices that had become performative and those under development – still in what I called the theoretical phase. From the 1990s, the new vision, for pathology and medicine more generally, seemed to be "translational medicine."

## RECENT TRANSLATIONAL MEDICINE

Just as tissue cellular pathology drew from biology and microscopy, and clinical pathology drew from chemistry, so translational medicine and its daughter discipline, genomics, drew on informatics, a discipline that grew out of computer science and clinical information systems, including taxonomies of diseases and procedures. At the turn of the twenty-first century, a manifesto for this idea occupied an entire issue of the *Journal of Pathology*.[43] The concerns of this laboratory-based movement, still awaiting its stage of performativity, include the creation of systems of standards for storing pathology images and terminologies, the interoperability of those systems, and – probably the most important for future performativity – the correlation of things found by pathologists with the recently decoded human genome. (Other genomes on the phylogenetic spectrum are also of interest to these pioneers.) Fundamental to this growing community within a community – a "budding-off" effect that both explains and typifies the emerging layers we have discussed here – is the notion that future understandings of disease will require an improved armamentarium for dealing with huge and complex datasets and more attention to fields previously neglected in medical education, such as informatics, genetics, and bioengineering. Partly to attain such goals, the Association of Pathology Informatics was created in the early 2000s.[44]

Should one ask whether and how this latest stratum in the palimpsest of pathology would reach its stage of performativity, clues may be seen in oncologic pathology. Laboratories involved in this new wave of pathology remain interested in the peculiarities of malignant disease, echoing Virchow and his

[42] For the scale of forensic pathology and a view of the "real" predicament of this subdiscipline, see Brad Randall, "Survey of Forensic Pathologists," *American Journal of Forensic Medicine and Pathology*, 22 (2001), 123–7.

[43] K. J. Hillan and P. Quirke, "Preface to Genomic Pathology: A New Frontier," *Journal of Pathology*, 195, no. 1 (September 2001), 1–2.

[44] This organization, which early on lined up corporate sponsors from both the medical records and the digital-imaging worlds, can be examined, and some of its early ephemeral documents reviewed, at http://www.pathologyinformatics.org (last accessed April 2, 2006).

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predecessors' work in the nineteenth century.[45] Now, however, they hope to use the large arrays of data that have been categorized and indexed, from both images and DNA sequences of cancer patients, to "tailor" the diagnosis of cancer and ultimately its therapy. In the mid-1990s, some encouragement came from changes in the way clinicians viewed the so-called mucosa-associated lymphoid tissue, or MALT, a malignant lymphoma seen in some patients with stomach cancer. For over a century, extirpation of such lesions had been the hallmark of localist tissue pathology and cellular pathology, little affected by the advent of performative twentieth-century clinical pathology. Suddenly, however, the MALT lymphoma was proven to be easily susceptible to a regimen of medication – not the classic poison of chemotherapy but combinations of acid inhibitors and antibiotics. An organism, *Helicobacter pylori*, was shown to be associated with the chronic irritation of the stomach lining that led, in most if not all cases, to this tumor. More startlingly, the use of therapeutic means other than surgery or chemotherapy was shown to reliably produce shrinkage of the lymphoma.[46] In at least a few cases, litigation ensued when patients who had lost their stomachs after a cancer diagnosis discovered they might have been spared surgery or chemotherapy. If this bit of contemporary history is an early intimation of translational medicine entering its performative phase, then the whole image of disease may begin to change anew.

## CONCLUSION

Pathology has always had two very different gazes, one diagnostic or forensic ("What is wrong with *this* person?") and the other boundary-maintaining ("What is the difference between 'diseased' and 'normal'?"). Because of this dual gaze, pathology has developed through a series of layers – as each theoretical phase was succeeded by a performative phase. The idea of tissues, a pan-European phenomenon, gave rise to early nineteenth-century performative pathology as part of the French *clinique médicale*. Later in the nineteenth century, microscopy and the new biology that it spawned gave rise to the theory of cellular pathology, which, later in an era of bacteriology and aseptic surgery, created its own performative possibilities. A similar dynamic occurred as a new layer of clinical pathology, drawn from nineteenth-century clinical chemistry, was rendered performative by the automated procedures of the twentieth century. Finally, in the late twentieth century, a new form of "patho-genomics" appears on the verge of creating the conditions for

[45] Jules Berman, "Tumor Classification: Molecular Analysis Meets Aristotle," *BMC Cancer*, 4 (2004), 10. Electronic version accessible at http://www.biomedcentral.com/1471-2407/4/10.

[46] Julie Parsonnet and Peter Isaacson, "Bacterial Infection and MALT Lymphoma," *New England Journal of Medicine*, 350 (2004), 213–15.

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the morphing of the larger parent discipline of pathology. Would this new information-driven pathology of the twenty-first century be recognizable to Morgagni, Bichat, Virchow, and Van Slyke? The palimpsest adds new layers, but underneath are those that preceded them, and still may change. And at the very bottom, in some sense ultimately unknowable, is the body itself and its ills.

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*Part III*

# NEW OBJECTS AND IDEAS

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# 20

# PLATE TECTONICS

*Henry Frankel*

The earth sciences underwent a revolution during the 1960s, ending nearly sixty years of controversy over the reality of continental drift. Before 1966, few workers accepted continental drift as a working hypothesis; most earth scientists preferred fixist theories. *Fixist* theories maintain that the continents and oceans have not appreciably changed their positions relative to each other, whereas theories within the continental drift tradition, hereafter referred to as the *mobilist* tradition, maintain that relative displacement occurs. However, most earth scientists became mobilists soon after the confirmation of seafloor spreading, and plate tectonics, the modern theory of continental drift, remains the reigning theory in the earth sciences. The aim of this chapter is to outline the major historical aspects of the plate tectonics revolution.[1]

[1] Among the best histories of the controversy are Homer E. Le Grand, *Drifting Continents and Shifting Theories* (Cambridge: Cambridge University Press, 1988), and Naomi Oreskes, *The Rejection of Continental Drift: Theory and Method in American Earth Science* (New York: Oxford University Press, 1999). Two early – and still useful – discussions are Anthony Hallam, *A Revolution in the Earth Sciences* (Oxford: Oxford University Press, 1973), and Ursula B. Marvin, *Continental Drift: The Evolution of a Concept* (Washington, D.C.: Smithsonian Institution Press, 1973). Far less reliable is Walter Sullivan, *Continents in Motion* (New York: McGraw-Hill, 1974). On the nature of the revolution, see Henry Frankel, "The Reception and Acceptance of Continental Drift Theory as a Rational Episode in the History of Science," in *The Reception of Unconventional Science*, ed. Seymour H. Mauskopf (Boulder, Colo.: Westview Press, 1979), pp. 51–90; Henry Frankel, "The Career of Continental Drift Theory: An Application of Imre Lakatos' Analysis of Scientific Growth and Change to the Rise of Drift Theory," *Studies in History and Philosophy of Science*, 10 (1979), 21–66; Henry Frankel, "The Non-Kuhnian Nature of the Recent Revolution in the Earth Sciences," in *Proceedings of the 1978 Biennial Meeting of the Philosophy of Science Association*, *PSA 1978*, ed. Peter D. Asquith and Ian Hacking, vol. 2 (East Lansing, Mich.: Philosophy of Science Association, 1981), pp. 240–73; Michael Ruse, "What Kind of Revolution Occurred in Geology," in Asquith and Hacking, *PSA 1978*, vol. 2, pp. 197–214; Rachel Laudan, "The Recent Revolution in Geology and Kuhn's Theory of Scientific Change," in *Paradigms and Revolutions: Appraisals and Applications of Thomas Kuhn's Philosophy of Science*, ed. Garry Gutting (Notre Dame, Ind.: University of Notre Dame Press, 1980), pp. 284–96; Robert Muir Wood, *The Dark Side of the Earth* (London: Allen and Unwin, 1985); John A. Stewart, *Drifting Continents and Colliding Paradigms: Perspectives on the Geoscience Revolution* (Bloomington: Indiana University Press, 1990); Anthony Hallam, "Shift in Theories," *Nature*, 345 (1990), 586; Henry Frankel, "Continental Drift and the Plate Tectonics Revolution," in *Sciences of the Earth: An Encyclopedia of Events, People, and Phenomena*, ed. Gregory A. Good (New York: Garland, 1999), pp. 118–36.



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## THE CLASSICAL STAGE OF THE MOBILIST CONTROVERSY: FROM ALFRED WEGENER TO THE END OF THE SECOND WORLD WAR

The prospect of finding an overall geological theory looked promising to many earth scientists during the 1880s, for they believed that Eduard Suess (1831–1914), the great Austrian geologist, had provided them with a basic framework. His fixist theory was secular contractionism, the reigning tradition during the latter half of the nineteenth century.[2] Suess maintained that the earth has been contracting since its initial formation as it cooled. He postulated that tensions are produced in the crustal layer because the earth's inner layers contract more rapidly than its crust. Tensions relieved by horizontal thrusting and folding form mountain systems and island arcs; tensions relieved by vertical faulting and large-scale subsidence cause the foundering of former continents into present-day oceans. Suess argued that the present arrangement of continents and oceans is not a permanent feature of the earth. Continental collapses occur with the resolution of radial tensions brought about by contraction of the earth. Suess developed an extensive paleogeographic reconstruction, supporting it with structural and paleontological arguments.

Although many European paleontologists supported Suess's theory, geophysicists raised problems. With the discovery that the major features of the earth's crust tend to remain in isostatic equilibrium with its denser fluid-like interior, they argued that Suess's paleocontinents could not have sunk into a denser seafloor. With the discovery of abundant amounts of heat-producing radioactive material within the earth's interior, they argued that the assumption of a continuously cooling earth had become dubious. Geologists raised problems with Suess's account of mountain building. Some argued that contractionism could not explain why mountains are located in concentrated groups rather than evenly distributed over the earth's surface. Others claimed that the amount of radial contraction needed to "unfold" the mountains of the Alpine system, not to mention all other mountain systems that had appeared on the face of the earth, was much greater than allowed for by Suessian contractionism.

Although this multifaceted assault on Suess's theory lessened its popularity, the majority of continental European geologists continued to support contractionism even though many rejected Suess's version, and they developed new contractionist theories that avoided some of the difficulties faced by Suessian contractionism. In fact, contractionism remained an important view until the acceptance of seafloor spreading.

[2] For an excellent account of Suessian contractionism, see Mott T. Greene, *Geology in the Nineteenth Century: Changing Views of a Changing World* (Ithaca, N.Y.: Cornell University Press, 1982).

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Several highly speculative, sketchy, and ignored versions of continental drift appeared before the twentieth century. The American geologist Frank Taylor (1860–1938) presented the first detailed version of continental drift in 1907 but attracted little attention.[3] Alfred Wegener (1880–1930), a German meteorologist and geophysicist, presented his version of continental drift in 1912. It attracted considerable attention.[4]

Wegener argued that his theory offered solutions to many problems, including the following:

1. Why do the contours of the coastlines of eastern South America and western Africa fit together so well, and why are there many similarities between the respective coastlines of North America and Europe? Wegener posited that the continents had originally been united into a single continent, named Pangea, and subsequently broke apart.
2. Why are there numerous geological similarities between Africa and South America and others between North America and Europe? Again, Wegener appealed to the breakup of the continents. The separation of the continents divided continuous geological structures into old and new world components.
3. Why are there many examples of past and present-day life forms having a geographically disjunctive distribution? Wegener argued that the distribution of the life forms had become disjunctive through the separation of the respective land areas with the breakup of Pangea.
4. Why are mountain ranges usually located along the coastlines of continents, and why are orogenic regions long and narrow in shape? Wegener hypothesized that the leading edges of drifting continents crumbled as the resisting ocean floor compressed them. The Andes served as his best example. He also claimed that the Himalayas formed when peninsular India collided with Asia.
5. Why does the earth's crust exhibit two basic elevations, one corresponding to the elevation of the continental tables and the other to the ocean floor? Wegener argued that there simply were two undisturbed primal levels that remained relatively unchanged once they reached isostatic equilibrium.
6. What is the origin of the Permo-Carboniferous ice cap, which covered parts of South Africa, Argentina, southern Brazil, India, and Australia? Wegener's solution was to suppose that the respective regions had been united during the Permo-Carboniferous, and he positioned the center of the ice cap at the South Pole.

In comparing the relative problem-solving effectiveness of his theory with contractionism, Wegener argued that only his theory offered solutions to problems (1), (5), and (6) and that the solutions his theory offered to (2), (3), and (4) were superior to the respective contractionist ones. His solutions

[3] For an account of Taylor's theory and its reception, see Rachel Laudan, "Frank Bursley Taylor's Theory of Continental Drift," *Earth Sciences History*, 1 (1985), 118–21.

[4] For two interesting discussions of Wegener's early work on continental drift, see Anthony Hallam, *Great Geological Controversies* (Oxford: Oxford University Press, 1983); Mott T. Greene, "Alfred Wegener," *Social Research*, 51 (1984), 739–61.

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to (2) and (3), unlike the contractionist solutions, were consistent with the principle of isostasy, and his solution to (4), unlike the contractionist solution, could explain the location and concentration of existing mountainous regions and did not depend on the dubious assumption of a cooling earth.

However, Wegener didn't think his theory merited immediate acceptance, in part because he recognized that one problem that he could not solve was the question of the forces responsible for the displacement of the continents. This second-order problem, the "mechanism" problem, was an obvious one for him to address. Because he had criticized the contractionist mechanisms for the collapse of paleocontinents and the formation of mountain ranges, he had to say something about the mechanism for continental drift. Wegener suggested that horizontal displacement of drifting continents is not physically impossible if there are enduring forces that propel the sialic continents through the simatic seafloor. To be sure, there were a number of possibilities, and Wegener referred to several of them: flight from the poles, tidal forces, meridional rifting, processional forces, polar wandering, or some combination of them. However, he admitted that none of these solutions were adequate, and claimed that any serious attempt to answer the question would be premature because little was known about such forces.

Wegener expanded his account in his book *The Origin of Continents and Oceans*. The first edition appeared in 1915, followed by new editions in 1920, 1922, and 1929. Wegener continued to enlarge the evidential base for his theory in ensuing editions, paid special attention to geodetic studies that appeared to indicate a westward drift of Greenland relative to Europe, and attempted to remove criticisms that opponents raised against his theory. Wegener died in 1930.[5]

Reactions to Wegener's theory were of three types. First, his theory spawned a number of subcontroversies within different fields of the earth sciences. In each case, fixists and mobilists raised problems with the competing solutions, and neither group was able to develop a recognized difficulty-free solution. Consequently, no fixist or mobilist theory gained anything approaching universal acceptance. Fixists criticized Wegener's solution to the problem about the matchup of continental margins. They argued that the fit between Europe and North America was not nearly as good as the one between Africa and South America, that Wegener had overestimated the similarity between the

[5] For discussions about the development of Wegener's ideas in various editions of *The Origin of Continents and Oceans*, see Hallam, *Great Geological Controversies*; Le Grand, *Drifting Continents and Shifting Theories*; Marvin, *Continental Drift*. To learn more about Wegener's life, see Martin Schwarzback, *Alfred Wegener, the Father of Continental Drift*, trans. Carla Love (Madison, Wis.: Science Tech, 1986); Johannes Georgi, "Memories of Alfred Wegener," in *Continental Drift*, ed. S. K. Runcorn (London: Academic Press, 1962), pp. 309–24. Also see Johannes Georgi, *Mid-Ice: The Story of the Wegener Expedition to Greenland*, trans. F. H. Lyon (New York: Dutton, 1935), for several moving essays about Wegener's ill-fated expedition to Greenland. The essays, by members of the expedition, describe their last meeting with Wegener and the hardships they faced during the winter at their mid-ice camp.

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two southern continents, and therefore that the amount of similarity was simply an accident.[6] Although many paleontologists welcomed Wegener's theory and argued in favor of its solution to the disjunctive distribution of life forms, other paleontologists, such as G. G. Simpson (1902–1984), the most prominent American vertebrate paleontologist of the period, developed a permanentist solution to the problem and raised several objections to the mobilist solution throughout the 1940s, arguing that Wegener had greatly overestimated the number of disjunctively distributed life forms because of his appeal to unreliable data.[7] The controversy in paleoclimatology over Wegener's solution to the Permo-Carboniferous ice cap underwent a similar evolution.[8] Several eminent paleoclimatologists supported continental drift. However, throughout the 1920s and 1930s, fixists raised problems with Wegener's solution. Among other things, they argued that the existence of glaciated regions during the Permo-Carboniferous in the United States was anomalous with Wegener's theory because, according to Wegener, the United States had been tropical. Although Wegener and Alex du Toit (1878–1948) altered the mobilist solution to avoid some of the problems fixists raised, they were unable to produce a recognized difficulty-free solution. In addition, Charles Schuchert (1858–1942) and Bailey Willis (1857–1949), two American fixists, developed an alternative solution to the problem in the early 1930s. A similar controversy arose in geodesy over the apparent westward drift of Greenland relative to Europe. Initial results during the 1920s made by the Danish Geodetic Survey offered support for mobilism. Wegener hailed the results as offering a potentially difficulty-free solution. However, new and more reliable results by the Danish Geodetic Survey in the 1930s failed to support Wegener's theory, as fixists predicted, and led to the conclusion that the previous measurements were unreliable.[9]

Second, the mechanism of continental drift proposed by Wegener was regarded as the weakest link in his theory. Fixists such as Harold Jeffreys (1891–1990), the most important British geophysicist during this period, and many North American geologists and geophysicists argued that the particular forces that Wegener had invoked to move continents were inadequate.

[6] See Hallam, *Revolution in the Earth Sciences*; Le Grand, *Drifting Continents and Shifting Theories*; Marvin, *Continental Drift*.

[7] For accounts of the subcontroversy in paleontology and biogeography, see Henry Frankel, "The Paleobiogeographical Debate over the Problem of Disjunctively Distributed Life Forms," *Studies in History and Philosophy of Science*, 12 (1981), 211–59; Léo F. Laporte, "Wrong for the Right Reasons: G. G. Simpson and Continental Drift," *Geological Society of America: Centennial Special*, 1 (1985), 273–85.

[8] For a discussion of the subcontroversy over the Permo-Carboniferous ice cap, see Henry Frankel, "The Permo-Carboniferous Ice Cap and Continental Drift," *Compte rendu de Neuvieme Congres International de Stratigraphe et de Geologie du Carbonifere*, 11 (1979), 113–20.

[9] The geodetic evidence for mobilism is discussed in Hallam, *Revolution in the Earth Sciences*; Le Grand, *Drifting Continents and Shifting Theories*; H. W. Menard, *The Ocean of Truth: A Personal History of Global Tectonics* (Princeton, N.J.: Princeton University Press, 1986); Frankel, "Career of Continental Drift Theory."

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Jeffreys raised these objections in various editions of his very influential work *The Earth: Its Origin, History and Physical Constitution* (first edition, 1924) and at various symposia on continental drift during the 1920s and 1930s; many North American geologists and geophysicists raised their objections at a 1926 symposium on continental drift sponsored by the American Association of Petroleum Geologists. Because these objections raised against Wegener's mechanism affected every other solution of Wegener's theory, many specialists in paleoclimatology and paleontology who favored continental drift tempered their support.[10]

Third, Wegener's theory, however, gained the support of several influential earth scientists whose interests cut across several fields within the earth sciences. These researchers presented their own theories of continental drift. Among them were Emile Argand (1879–1940), Alex du Toit, John Joly (1857–1933), Arthur Holmes (1890–1965), and Reginald Daly (1871–1957). In 1923, Argand, a leading Alpine geologist from Switzerland, greatly expanded drift's solution to the problem of the origin of mountains. John Joly, an Irish geophysicist, suggested a new solution to the mechanism question during the 1920s. Alex du Toit, a renowned field geologist and one of the few South African geologists elected to the Royal Society, began to defend continental drift in the 1920s and continued to support it until his death in 1948. Du Toit, concentrating on the geology of southern Africa and South America, garnered much additional support, and he presented his own version of continental drift in his 1937 book *Our Wandering Continents*.[11] The British geologist and geophysicist Arthur Holmes was probably the most respected earth scientist to defend continental drift.[12] During the 1920s, he supported the contractionist theory of mountain building, but by the end of the decade he had rejected contractionism and began arguing in favor of continental drift. Holmes defended continental drift's solution to a number of problems and developed a new solution to the mechanism question, in which he invoked large-scale convection and offered an improved mobilist solution to the question of the origin of mountains. Although vehement fixists

[10] Because of its importance, the mechanism objection has received considerable attention in the literature. See, for example, Frankel, "Career of Continental Drift Theory"; Hallam, *Revolution in the Earth Sciences*; Le Grand, *Drifting Continents and Shifting Theories*; Marvin, *Continental Drift*; Menard, *Ocean of Truth*; Henry Frankel, "The Development, Reception, and Acceptance of the Vine-Matthews-Morley Hypothesis," *Historical Studies in the Physical Sciences*, 13 (1982), 1–39.

[11] See Emile Argand, *Tectonics of Asia*, trans. Albert V. Carozzi (New York: Hafner Press, 1977), which provides an informative introduction to Argand's mobilist ideas. Discussions of Joly, Daly, and du Toit may be found in Frankel, "Career of Continental Drift"; Hallam, *Revolution in the Earth Sciences*; Le Grand, *Drifting Continents and Shifting Theories*; Marvin, *Continental Drift*; Menard, *Ocean of Truth*.

[12] With the exclusion of Wegener, Holmes has received more attention than any other mobilists. See Henry Frankel, "Arthur Holmes and Continental Drift," *British Journal for the History of Science*, 11 (1978), 130–50; Naomi Oreskes, "The Rejection of Continental Drift," *Historical Studies in the Physical Sciences*, 18 (1988), 311–48; Alan Allwardt, "The Roles of Arthur Holmes and Harry Hess in the Development of Modern Global Tectonics" (PhD diss., University of California, Santa Cruz, 1990).

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such as Harold Jeffreys agreed that Holmes's new mechanism made mobilism no longer impossible, Jeffreys argued that it was extremely improbable. Moreover, many questioned the plausibility of positing large-scale convection currents, and some, such as the Dutch geophysicist Vening Meinesz, were willing to suppose that there was large-scale convection but did not endorse mobilism. In addition, Holmes's hypothesis could not be tested because it depended on data about the seafloor that were not collected until the 1960s. Consequently, Holmes's mobilism faced both theoretical and empirical difficulties. Continental drift enjoyed almost no support among earth scientists in North America, a situation that Naomi Oreskes has attributed to the fact that the theory violated their established methodology and norms of scientific practice. Reginald Daly and Beno Gutenberg, the only two prominent American mobilists, were, at best, excused for their mobilist tendencies.[13]

## THE MODERN CONTROVERSY OVER CONTINENTAL DRIFT

During the early 1950s, when the controversy over continental drift had come to a standstill, workers in paleomagnetism began to develop a new case for mobilism. The two major groups of paleomagnetists who supported mobilism were from the United Kingdom and were initially housed at the universities of Manchester and Cambridge. The Manchester group, headed by P. M. S. Blackett (1897–1974) and John Clegg (1922–1995), began working at the end of 1952. It moved to Imperial College in 1953. The other and eventually more influential group began to form at Cambridge during the summer of 1950 when Jan Hospers, a new PhD student from the Netherlands, went to Iceland to collect lava samples for paleomagnetic investigation. This group coalesced under the general leadership of S. K. Runcorn in 1951. Various members of the group continued to work at Cambridge until the end of 1955. Runcorn went to what was then the University College of the University of Durham (later the University of Newcastle), taking several paleomagnetists with him, including Kenneth Creer and Neil Opdyke. The major members of the Cambridge/Newcastle group included Creer, R. A. Fisher, Hospers, Edward Irving, Opdyke, and Runcorn. Irving left Cambridge in 1954 for the Australian National University, where he started his own group. The London, Newcastle, and Australian groups argued in favor of continental drift throughout the late 1950s and early 1960s. Their work reactivated the stagnating controversy of mobilism versus fixism. Those who were already in favor of continental drift welcomed their results. However, the land-based paleomagnetic evidence itself was insufficient to change the

[13] Oreskes, "Rejection of Continental Drift," Marvin, *Continental Drift*, and Menard, *Ocean of Truth*, discuss Gutenberg's mobilism.

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attitude of most earth scientists. Some geophysicists questioned the reliability of the paleomagnetic data supportive of mobilism, and even some paleomagnetists, such as Alan Cox (b. 1926) and Richard Doell (b. 1925), argued that polar wandering without continental drift could explain the paleomagnetic data.[14]

Oceanography expanded rapidly during the 1950s. Because knowledge of the seafloor was correctly viewed as important for purposes of defense, oceanographers were able to secure ample funds for investigating the seafloor. By 1960, combined efforts at several universities, institutes, and agencies, such as Lamont-Doherty Geological Observatory at Columbia University, Scripps Institution of Oceanography, Cambridge University, Woods Hole, the U.S. Coast and Geodetic Survey, and the U.S. Office of Naval Research, greatly increased what was known about the ocean floors. Paramount among the many discoveries was the worldwide network of oceanic ridges. Finding a solution to the origin of these ridges became a major concern of marine geologists and geophysicists. Both fixist and drift solutions were offered. For example, Maurice Ewing (1906–1974), head of Lamont-Doherty Geological Observatory, H. W. Menard (1920–1986), one of the major figures at Scripps, and Harry Hess (1906–1969), a leading geologist at Princeton University, offered differing fixist solutions. Menard and Hess also proposed mobilist solutions. Bruce Heezen (1924–1995), an oceanographer at Lamont, presented a solution that invoked an expanding earth.[15]

Nature turned out to be on the side of Hess's mobilist hypothesis, which was labeled "seafloor spreading" by Robert Dietz (1938–1995), another supporter of the hypothesis. First presented in a December 1960 preprint, it proposed that the seafloor is created along ridge axes with material forced up from the mantle by rising convection currents and that the material spreads out perpendicularly from the axes, creating new ocean basins. The next year Hess added the idea that the horizontally moving seafloor eventually sinks into the mantle, forming oceanic trenches along the periphery of the basins. Seafloor spreading offered a solution to the origin of oceanic ridges. In addition, Hess realized that if a ridge were created within a landmass, it would split apart, a new ocean basin would form between the separating landmasses, and the landmasses would continue to move away from one another, increasing the width of the newly formed ocean basin. Thus, if a new seafloor were

[14] There is no full-blown, detailed account of the rise of paleomagnetism and its use to test mobilism, but see Edward Irving, "The Paleomagnetic Confirmation of Continental Drift," *EOS: Transactions of the American Geophysical Union*, 69 (1988), 994–7. See also Hallam, *Revolution in the Earth Sciences*; Le Grand, *Drifting Continents and Shifting Theories*; Menard, *Ocean of Truth*; Henry Frankel, "Jan Hospers and the Rise of Paleomagnetism," *EOS: Transactions of the American Geophysical Union*, 68 (1987), 577–80.

[15] The most interesting account of the rise of oceanography and the competing hypotheses about the origin of oceanic ridges is found in Menard, *Ocean of Truth*. See also William Wertenbaker, *The Floor of the Sea: Maurice Ewing and the Search to Understand the Earth* (Boston: Little, Brown, 1974), by a journalist who interviewed many of the key scientists at Lamont-Doherty Geological Observatory.

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to form within a continental landmass, continental drift would occur. Hess noted that his version of mobilism offered a solution to the mechanism problem that had plagued other mobilist theories, for, in his model, continents do not plow their way through the seafloor but simply passively ride on the backs of convection currents as they move horizontally. Hess's idea was somewhat similar to Holmes's older idea of seafloor thinning.[16]

When Hess proposed his hypothesis, it was just one of several interesting solutions to the origin of mid-ocean ridges. Its importance lay in the fact that it spawned two important testable corollaries. Fred Vine (b. 1939) and Drummond Matthews (b. 1931) independently proposed the first. Vine, a Cambridge graduate student working under the supervision of Matthews, came up with the idea in 1963. Lawrence Morley, a Canadian geophysicist, developed the idea in the same year. However, Morley's account was twice rejected before he appended it onto a lengthy piece that he and a coworker published in 1964.[17] Although they differed slightly, both corollaries maintained that if seafloor spreading has occurred and the earth's magnetic field has undergone repeated reversals in its polarity, as land-based paleomagnetic studies had indicated, then the seafloor should be composed of alternating strips of normally and reversely magnetized material, the strips should run roughly parallel to the ridge axis, and the pattern of magnetic anomalies on each side of the ridge should be roughly the same.

The other hypothesis, invented by the Canadian geophysicist J. Tuzo Wilson (1908–1993), was presented in 1965. Wilson postulated a new class of faults, calling them transform faults. He argued that such faults would exist if seafloor spreading had occurred. Wilson explained how seismological data could be used to detect their existence. Both corollaries were confirmed in 1966. Vine and Wilson found confirmation of the Vine-Matthews hypothesis through examination of paleomagnetic seafloor data that had been collected by workers at Scripps Institution of Oceanography. Further confirmation of the hypothesis came from work at Lamont-Doherty Geological Observatory. Analyses of several magnetic profiles over the Pacific-Antarctic Ridge by Walter Pitman, a graduate student at Lamont-Doherty Geological Observatory working under James Heirtzler, provided strong support. The seismologist Lynn R. Sykes detected the existence of Wilson's transform faults by analyzing seismological data from the Mid-Atlantic Ridge. Neil Opdyke, a paleomagnetist who worked with Runcorn and Irving, found support for

[16] The development of Hess's ideas is discussed in Allwardt, "Roles of Arthur Holmes and Harry Hess" in the Development of Modern Global Tectonics"; Henry Frankel, "Hess's Development of his Seafloor Spreading Hypothesis," in *Scientific Discovery: Case Studies*, ed. Thomas Nickles, Boston Studies in the Philosophy of Science, vol. 60 (Dordrecht: Reidel, 1980), pp. 345–66.

[17] Detailed historical accounts of the development, reception, and acceptance of the Vine-Matthews-Morley hypothesis appear in Frankel, "Development, Reception, and Acceptance of the Vine-Matthews-Morley Hypothesis," and William Glen, *The Road to Jaramillo: Critical Years of the Revolution in Earth Science* (Stanford, Calif.: Stanford University Press, 1982). Both authors interviewed many of the key scientists.

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geopolarity reversals and the Vine-Matthews hypothesis through analysis of ocean-floor sediments. With the confirmation of both corollaries, most fixists actively engaged in oceanographic research immediately accepted mobilism because of the explanatory advantages offered by seafloor spreading when coupled with its two corollaries.

Although this ended the mobilist controversy, the revolution was not completed until the development of plate tectonics in 1967. Plate tectonics was independently conceived by Jason Morgan, a physicist turned geophysicist at Princeton University, and Dan McKenzie, a geophysicist in the Department of Geodesy and Geophysics at Cambridge University. Plate tectonics grew out of the application of seafloor spreading and its two corollaries to a spherical surface. Central to its development was the realization that Euler's theorem (which says that any movement of a point on the surface of a sphere can be described by rotation about a point, or "Euler" pole) could be used to describe the relative movements of ten or so rigid plates that comprise the earth's outer layer, the lithosphere. Scientists at Lamont-Doherty Geological Observatory immediately found support for plate tectonics. Xavier Le Pichon made extensive use of the marine paleomagnetic data at Lamont, and two other Lamont seismologists, Jack Oliver and Brian Isacks, found seismological support for a rigid outer surface extending about 100 km within the earth's interior.[18]

The advent of plate tectonics has brought about a realignment of the various subdisciplines in the earth sciences. Geology no longer reigns supreme, but geophysics does not reign over geology. Because plate tectonics has given earth scientists a successful view that conceptually unites the various subdisciplines of the earth sciences, it has forced geologists and geophysicists to work together. Witness, for example, the work on exotic terrains.

[18] Jack Oliver, *Shocks and Rocks: Seismology in the Plate Tectonics Revolution*, History of Geophysics, vol. 6 (Washington, D.C.: American Geophysical Union, 1995), is an excellent account of the seismological work of Oliver, Isacks, and Sykes. See the review by Henry Frankel, "The Tectonic Revolution as Seen by One of the Key Revolutionaries," *Physics Today*, 50 (1997), 63–4. For an account of the independent development of plate tectonics by Morgan and McKenzie, see Henry Frankel, "The Development of Plate Tectonics by J. Morgan and D. McKenzie," *Terra Nova*, 2 (1990), 202–14.

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# 21

# GEOPHYSICS AND GEOCHEMISTRY

*David Oldroyd*

Geophysics is the branch of experimental physics concerned with the earth, atmosphere, and hydrosphere. It includes such fields as meteorology and oceanography, but attention is restricted here to geodesy, gravimetry, seismology, and geomagnetism. Geochemistry is the study of the distribution and migration of the different elements in the earth, oceans, and atmosphere and therefore involves the chemical analyses of minerals, rocks, and the atmosphere, and mineral solutions. Modern geochemical research makes much use of studies of the radioisotopes of the different elements, which are also used for radiometric dating. In such work, the boundaries between geophysics, geochemistry, and geology are indistinct.

Geophysics is an important field, both practically, as in earthquake studies, and theoretically, as exemplified by geophysicists' contributions to the establishment of plate tectonics (see Frankel, Chapter 20, this volume).[1] Geochemistry is likewise important: practically, as in geochemical prospecting, and theoretically, especially regarding the earth's origin and cyclic processes, some involving living organisms. Neither field has attracted the historical attention it deserves, although there are a number of useful sources that offer information and insights of relevance.[2] Beacuse these areas are less well

[1] For a convenient summary of plate tectonics theory, with some historical information, see W. Jacquelyne Kious and Robert I. Tilling, *This Dynamic Earth: The Story of Plate Tectonics* (Washington, D.C.: U.S. Department of the Interior/U.S. Geological Survey, n.d.).

[2] For a comprehensive bibliography, see Stephen G. Brush and Helmut E. Landsberg, *The History of Geophysics and Meteorology: An Annotated Bibliography* (New York: Garland, 1985). See also Stephen G. Brush, *A History of Modern Planetary Physics*, vol. 1: *Nebulous Earth*, vol. 2: *Transmuted Past*, vol. 3: *Fruitful Encounters* (Cambridge: Cambridge University Press, 1996). D. H. Hall, *History of the Earth Sciences during the Scientific and Industrial Revolutions with Special Reference on the Physical Geosciences* (Amsterdam: Elsevier, 1976), offers a Marxist perspective on the field. For an "insider's" viewpoint, see Charles C. Bates, Thomas F. Gaskell, and Robert B. Bruce, *Geophysics in the Affairs of Man: A Personalized History of Exploration Geophysics and Its Allied Sciences of Seismology and Oceanography* (Oxford: Pergamon Press, 1982). Important studies of geochemistry include A. A. Manten, "Historical Foundations of Chemical Geology and Geochemistry," *Chemical Geology*, 1 (1966), 5–31; Claude Allègre, *From Stone to Star: A View of Modern Geology* (Cambridge, Mass.:



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known than other aspects of the earth sciences, this chapter will include outlines of the major scientific developments before indicating what is known about their history.

In what follows, the leading branches of geophysics are discussed and their contributions to the establishment of the plate tectonics synthesis outlined. The geochemistry sections indicate how another synthesis, assisting understanding of the earth's dynamics, has also been achieved. Together, these accomplishments were so profound that since the 1960s and 1970s the word "geology" has sometimes given way to "earth science" or "geoscience," indicating the establishment of a broader understanding of the earth and its history – deploying new instruments very different from the geologist's traditional hammer, hand lens, microscope, and other implements. Historians have not yet formed a synthesized view of the metamorphosis of geology into "earth science," although Robert Muir Wood provides a useful conspectus.[3]

Geophysical theory is often abstract and mathematical, and the emergence of the science is partly attributable to developments in mathematics (e.g., potential theory, deployed in geomagnetism) or physical theory (e.g., that for elasticity, required in seismology). Until the partial transformation of geology into "earth science," geophysicists were primarily trained as mathematicians, physicists, or astronomers, who took up terrestrial problems for their theoretical or mathematical interest. An example would be the mathematician/physicist Carl Friedrich Gauss (1777–1855), director of the Göttingen Observatory. There was also interest in the earth as a planetary body, exemplified by the work of Cambridge astronomy professor Harold Jeffreys (1891–1989). However, already in the nineteenth century there were specialist geodesists, seismologists, and others working as observers, calculators, or theoreticians in a variety of surveys or observatories and also in universities.

Data-gathering on a worldwide scale has been important, with both national and international efforts playing a role. At a practical level, geophysical techniques were used in mineral prospecting well before the 1960s, and much relevant data collecting was performed by surveys arising from military interests. National surveys often had military origins, including the Great

Harvard University Press, 1992). Peter Westbroek explores the role of living things in geochemical cycles in *Life as a Geological Force* (New York: Norton, 1991). On seismology, see Jack Oliver, *Shocks and Rocks: Seismology in the Plate Tectonics Revolution* (Washington, D.C.: American Geophysical Union, 1996). The contribution of geomagnetic work to the plate tectonics revolution is explored in William Glen, *The Road to Jaramillo* (Stanford, Calif.: Stanford University Press, 1982). For technical details, many of interest to the historian, see S. K. Runcorn, ed., *International Dictionary of Geophysics*, 2 vols. and maps (Oxford: Pergamon Press, 1967); Shawna Vogel, *Naked Earth: The New Geophysics* (New York: Dutton, 1995). The relationship between geodetic work and isostasy is discussed in Naomi Oreskes, *The Rejection of Continental Drift: Theory and Method in American Earth Science* (Oxford: Oxford University Press, 1998). A survey by David R. Oldroyd, *Thinking about the Earth* (London: Athlone Press; Cambridge, Mass.: Harvard University Press, 1996) gives more attention to geophysics and geochemistry than do other histories of geology. Note also the annual volume *History of Geophysics* published by the American Geophysical Union and a recent collection of papers in *Earth Sciences History*, 26 (2007), no. 2.

[3] Robert M. Wood, *The Dark Side of the Earth* (London: Allen and Unwin, 1984).

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Trigonometrical Survey of India and the U.S. Coast and Geodetic Survey. Much of the geodetic and other geophysical work done in early nineteenth-century Britain was also associated with the military.[4] The surveys of the ocean floors on which the plate tectonics revolution was based were often conducted for military reasons. But international collaboration depended more on the enthusiasm of scientists, the influence of the German polymath Alexander von Humboldt (1769–1859) being particularly important. Humboldt hoped a unified theory of the earth and cosmos would emerge from international collaborative efforts toward collection and synthesis of data.[5] Some of the international collaborative projects initiated in his time are mentioned herein, and others, such as the International Polar Year (1882–3), were established later in the nineteenth century. The twentieth century has seen grand collaborative projects such as those of the International Geophysical Year (IGY) of 1957–8. Geochemists have tended to have a somewhat closer association with traditional geology than have geophysicists. Geochemical data are also collected worldwide, but a network of observatories is not required.

## THE SIZE, SHAPE, AND WEIGHT OF THE EARTH: GRAVIMETRY AND ASSOCIATED THEORIES

Isaac Newton (1642–1727) recognized the earth as spheroidal, and in the eighteenth century much attention was given to its precise shape – through courageous explorations to determine the length of a degree at different latitudes by the French academicians Charles de la Condamine (1701–1774) in Peru and Pierre Maupertuis (1698–1759) in Lapland and heroic mathematical analyses, especially by Alexis-Claude Clairaut (1713–1765).[6] Because the earth is spheroidal, its acceleration due to gravity (*g*) varies over its surface, and determining *g* at different localities allows calculation of the planet's actual shape. The fact that earth is spheroidal also causes the moon's observable inequalities of latitude and longitude. So, by 1837, Friedrich Bessel (1784–1846), director of the Königsberg Observatory, had sufficient information to calculate the ratio of the earth's equatorial to polar axes to be 3271953.854: 3261072.900 – a "bulge" of about 11.5 miles.[7] Such work might be called

4 See David P. Miller, "The Revival of the Physical Sciences in Britain, 1815–1840," *Osiris*, 2 (1986), 107–34.

5 L. Kellner, "Alexander von Humboldt and the Organization of International Collaboration in Geophysical Research," *Contemporary Physics*, 1 (1959), 35–48.

6 Alexis-Claude Clairaut, *Théorie de la Figure de la Terre* (Paris: David Fils, 1743); Isaac Todhunter, *A History of the Mathematical Theories of Attraction and the Figure of the Earth* (London: Macmillan, 1873), vol. 1, chap. 11; John L. Greenberg, *The Problem of the Earth's Shape from Newton to Clairaut* (Cambridge: Cambridge University Press, 1995).

7 Friedrich W. Bessel, "Determination of the Axes of the Elliptical Spheroid of Revolution Which Most Nearly Corresponds with the Existing Measurements of the Arcs of the Meridian," in *Scientific Memoirs, Selected from the Transactions of Foreign Academies of Science and Learned Societies*, ed. Richard Taylor (London: Richard and John Taylor, 1841), vol. 2, pp. 387–400 (first published in *Astronomische Nachrichten* [1837]).

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Kuhnian "articulation" of Newton's paradigm in that it involved exploring the ramifications of Newton's physical theory and general worldview.

The earth's mean density (and hence mass) could be calculated by observing a pendulum's deflection from the vertical by an adjacent mountain, as attempted by the hydrographer Pierre Bouguer (1698–1758) on Condamine's Peruvian expedition and by Astronomer Royal Nevil Maskelyne (1732–1811) near Schiehallion, Perthshire, or by comparing pendulum oscillations on the earth's surface and in a mine of known depth, as was done by George Airy (1801–1892), another Astronomer Royal.[8] Using estimates of Schiehallion's mass and volume, Maskelyne calculated the earth's mean density to be four to five times that of its surface rocks, thus precluding a hollow interior.

The earth's mass could also be determined from $g$ and the gravitational constant ($G$). The earth attracts an object (mass $m$) according to the "Newtonian" formula $GMm/R^2 = mg$. Hence, knowing $g$ (measurable with a pendulum) and the earth's radius ($R$, known by geodetic survey), its mass ($M$) could be calculated. The value of $G$ was determined by English aristocrat-physicist Henry Cavendish (1731–1810). He measured the pull between two large lead spheres and two small ones suspended from a torsion rod. But the forces were minute and extreme efforts were needed to eliminate errors caused by temperature fluctuations or other effects. Cavendish also estimated the forces exerted by the instrument case, and the calculations became inordinately complex, as is usually the case in geophysics, even where the principles are straightforward. He gave the earth's relative density as 5.48 (equivalent to a value of $G$ of $6.75 \times 10^{-8}$ $cm^3/g/sec^2$).[9]

Cavendish's method was improved in the nineteenth century, for example by the London physicist and inventor Charles Boys (1855–1944). Boys used a short torsion rod on a quartz suspension fiber with gold weights suspended at different levels so that the large lead spheres were well apart from one another. Boys's value for the earth's mean density was 5.5270 g/cc. Further work was undertaken by Paul Heyl of the National Bureau of Standards in Washington. Using an evacuated apparatus, his value for $G$ was $6.670 \pm 0.005$ $cm^3/g/sec^2$. If Cavendish's correction procedures were already complex, those deployed by Heyl were horrendous, indicating the importance of accuracy in geophysics.[10]

At about the same time as Boys, the Hungarian physicist Baron Roland von Eötvös (1848–1919) developed a torsion balance instrument of great accuracy

[8] Pierre Bouguer, *La Figure de la Terre Déterminée par les Observations faites au Pérou* (Paris: C. A. Jombert, 1749); Nevil Maskelyne, "An Account of Observations Made on the Mountain of Schiehallion for Finding Its Attraction," *Philosophical Transactions*, 65 (1775), 500–42; George Airy, "On the Pendulum Experiments Lately Made in the Horton Colliery," *Philosophical Transactions*, 146 (1856), 297–356.

[9] Henry Cavendish, "Experiments to Determine the Density of the Earth," *Philosophical Transactions*, 83 (1798), 469–525.

[10] Charles V. Boys, "On the Newtonian Constant of Gravitation," *Philosophical Transactions, Series A*, 186 (1895), 1–72; Paul R. Heyl, "A Redetermination of the Constant of Gravitation," *Journal of Research of the National Bureau of Standards*, 5 (1930), 1243–90.

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that could measure gravitational gradients or minute variations in *g* over the surface of the earth.[11] Subsequently, instruments of this type were developed for use in geophysical prospecting, for minute variations in gravity could be indicative of subsurface structures and bodies.

Clairaut's analysis of the (rotating) earth assumed its surface was a spheroid of equilibrium, as would be formed by an envelope of water covering the earth entirely. Concentric solid layers of uniform density were assumed within the earth, each spheroidal layer having the same shape as the imaginary fluid surface. However, the earth has numerous inhomogeneities, as Bouguer recognized, so *g* generally differs from the "ideal" value for a given latitude. The difference between the ideal and actual value at any place, calculated after allowing for the altitude and the density of the rock between the observation site and sea level, is the "Bouguer anomaly." It is relevant to determination of the "geoid," a figure of great geological significance. In earlier work, a long, reversible bar pendulum was used to determine *g*. Later instruments used shorter pendulums.[12]

In the early nineteenth century, work by the Indian Trigonometrical Survey under George Everest (1790–1866) on determination of the Indian meridian arc revealed a difference in the latitudinal distance between Kalianpur and Kaliana depending on whether it was determined astronomically or by trigonometric survey. John Pratt (1809–1871), mathematical archdeacon of Calcutta, attributed the difference to the Himalayas attracting the instruments' plumb lines during the astronomical determinations. This led Airy to consider the forces acting on mountain ranges, and he suggested that they were like icebergs, "floating" on fluid but with solid "roots" extending into the earth. The idea was developed by the English clergyman-mathematician Osmond Fisher (1817–1914), who noted that dense basaltic rocks formed the seafloors, whereas less dense, granitic rocks tended to form mountain ranges.[13]

The American geologist Clarence Dutton (1841–1912) introduced the term "isostasy" ("equal standing") for the idea that the earth deviated from an ideal spheroid – bulging where matter is light and forming depressions where it is heavy. The whole might be in a state of balance unless disturbed by earth movements, chiefly caused by erosion and deposition. After such movements,

[11] Roland, Baron Eötvös of Vásárosnamény, "Untersuchungen auf dem Gebiete der Gravitation und des Erdmagnetismus," *Annalen der Physik und Chemie*, 59 (1896), 354–400.

[12] For example, Captain Henry Kater, "An Account of Experiments for Determining the Length of the Pendulum Vibrating Seconds in the Latitude of London," *Philosophical Transactions*, 108 (1818), 32–102; Major Robert von Sterneck, "Der neue Pendelapparat des k. k. Militär-geographischen Instituts," *Zeitschrift für Instrumentenkunde*, 8 (1888), 157–71.

[13] John Pratt, "On the Attraction of the Himalayan Mountains and of the Elevated Regions beyond Them, upon the Plumb-Line in India," *Philosophical Transactions, Series B*, 145 (1855), 53–100; George B. Airy, "On the Computation of the Effects of Mountain-Masses as Disturbing the Apparent Astronomical Latitude of Stations in Geodetic Survey," *Philosophical Transactions, Series B*, 145 (1855), 101–4; Osmond Fisher, *Physics of the Earth's Crust* (London: Macmillan, 1881).

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isostatic adjustment might be expected.[14] To test this idea required precise knowledge of the "geoid" – the form a water envelope would have (*not* a perfect spheroid) if it covered the whole, extending hypothetically "through" the continents. (The geoid's surface is everywhere normal to the direction of gravity.) Dutton's theory was at odds with the idea, prevalent in the nineteenth century, that crustal inhomogeneities were caused by cooling and contracting.

Around the turn of the century, much U.S. Coast and Geodetic Survey work was devoted to establishing the form of the geoid, principal workers being the civil engineering–trained John Hayford (1868–1925), chief of the Computing Division and geodetic inspector, and William Bowie (1872–1940), head of the Geodesy Section. The Coast and Geodetic Survey was developed into one of America's major scientific institutions by the efforts of Alexander Bache and provided an important institutional base for geophysical work and the professionalization of American geophysical sciences.[15] Testing the isostasy hypothesis was an important consideration for the survey. The method involved an accurate trigonometric survey of the United States and comparison with an astronomical survey, relating the results to the surrounding topography (as in the Indian work). Also, departures of the plumb line from the vertical were determined at different localities.[16]

The chosen "ideal" spheroidal form of the earth was that calculated by Captain Alexander Clarke (1828–1914),[17] and the geoid was represented graphically as contour lines relative to Clarke's spheroid. When the geoid (for parts of the United States at least) was published, it could be compared with the continent's topography. Discrepancies between the expected deviations from the vertical caused by topographic features and the empirically determined deviations (revealing the form of the geoid) were explained by the distribution of subsurface densities. Hayford claimed, however, that his results suggested that the earth was mostly in isostatic equilibrium. Mountains, it seemed, were not held up in a state of stress. There was sufficient plasticity to allow isostatic adjustment.

Following Pratt, Hayford envisaged a certain depth, uniform around the globe, at which isostatic compensation was complete. Hypothetically, one

[14] Clarence E. Dutton, "On Some of the Greater Problems of Physical Geology," *Bulletin of the Philosophical Society of Washington*, 11 (1892), 51–64.

[15] See Hugh R. Slotten, *Patronage, Practice, and the Culture of American Science: Alexander Dallas Bache and the U.S. Coast Survey* (Cambridge: Cambridge University Press, 1994). Slotten regards the Survey's work as essentially Humboldtian in character.

[16] U.S. Coast and Geodetic Survey, "The Form of the Geoid as Determined by Measurements in the United States," in *Report of the Eighth International Geographic Congress* (Washington, D.C.: U.S. Government Printing Office, 1905), pp. 535–40; John F. Hayford, "The Geodetic Evidence of Isostasy, with a Consideration of the Depth and Completeness of the Isostatic Compensation and of the Bearing of the Evidence upon some of the Greater Problems of Geology," *Proceedings of the Washington Academy of Science*, 8 (1906), 25–40; John F. Hayford, *The Figure of the Earth and Isostasy from Measurements in the United States* (Washington, D.C.: U.S. Government Printing Office, 1909; *Supplement*, 1910).

[17] A. R. Clarke, "Figure of the Earth," in Sir Henry James, *Comparisons of the Standards of Length of England, France, Belgium, Prussia, Russia, India, Australia* (London: Her Majesty's Stationery Office, 1866), vol. 1, pp. 281–7.

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could think "columns" of rock, of different heights and density but equal weight, at different parts of the "surface of compensation" (about 113 km below the spheroid). It was as if there were many icebergs, all with bases at the same depth but having different heights above the sea because they had different densities. For Hayford, this was a working hypothesis amenable to computation. However, he recognized the alternative, namely that the crust's undersurface might not be a simple spheroid: There could be thin areas of dense oceanic crust and thick areas of less dense continental crust. High mountains had deep roots, as in the Airy/Fisher model.[18]

Bowie developed Hayford's ideas, pointing to geological features such as rift valleys that suggested primacy of vertical movements, though there were also (as Dutton suggested) lateral movements arising from erosion and deposition, disturbing isostatic equilibrium.[19] The U.S. geodetic work and the plausible inferences therefrom partly explain why Americans were reluctant to accept continental drift: If the crust and underlying mantle were plastic, and could achieve isostatic equilibrium by vertical movements, lateral movements would be relatively insignificant.

But Hayford's understanding of isostasy was reached without knowledge of the values of $g$ for oceanic areas. In the 1920s, gravity studies were extended to oceanic areas, particularly by the Dutch civil engineer and geodesist Felix Vening Meinesz (1887–1966), who worked on American and Dutch submarines in the Gulf of Mexico, Southeast Asia, and elsewhere, cooperating with Bowie.[20] Again, a main objective was investigation of the geoid, and again most of the earth's crust was apparently in isostatic equilibrium, although anomalies were found, particularly in regions of tectonic activity. Gravity-deficit belts were discovered near island arcs (e.g., south of the Indonesian archipelago), suggesting that such regions contained an excess of light silicon- and aluminum-rich ("sialic") rock. It seemed that downward crustal buckling occurred in such compression zones in response to lateral forces, forming light "roots." Contrary to Bowie's expectations, such regions were found to be isostatically unbalanced and were apparently geologically active provinces. Subsequent upward movement of the "roots" might account for mountain formation.[21] Vening Meinesz also contemplated convection in the earth's

[18] Osmond Fisher, "On Deflections of the Plumb Line in India," in *Account of the Operations of the Great Trigonometrical Survey of India*, 22 vols. (Dehra Dun, 1870–1912), vol. 18, appendix 1; Osmond Fisher, "On the Variations of Gravity at Certain Stations of the Indian Arc of the Meridian in Relation to Their Bearing upon the Constitution of the Earth's Crust," *Philosophical Magazine*, 22 (1886), 1–29.

[19] William Bowie, *Isostasy* (New York: Dutton, 1927).

[20] The apparatus involved two pairs of pendulums perpendicular to one another, the pendulums of each pair swinging in opposite directions. The "fictitious pendulum" was that imagined to be oscillating with an amplitude equal to the difference in amplitude of the two pendulums of a pair. The mean period for the "fictitious" pendulum was independent of movements of the submarine, and changes in $g$ could be deduced from changes in its period. Movements were recorded photographically and comparisons made with a base in Holland.

[21] Felix A. Vening Meinesz, *Gravity Expeditions at Sea, 1923–1932* (Delft: N.V. Technische Boekhandlung en Drukkerij J. Waltman Jr., 1934), vol. 2, pp. 118–19.

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interior associated with tectonic processes.[22] He distanced himself from Alfred Wegener's "continental drift" hypothesis, but his gravity-deficit belts were later associated with the "subduction zones" of plate tectonics theory.

Gravimetry is also important for mineral prospecting, as small regional changes in *g* may indicate hidden ore bodies. Portable instruments have been devised to determine "gravity gradients," a type devised by American inventor Lucien LaCoste (1908–1995) dominating the field after the Second World War.[23] Plotted lines of equal gravity gradient can reveal significant subsurface structures.[24]

## SEISMOLOGY

In the early nineteenth century, the earth's interior was presumed to be liquid, in accordance with the observed temperature gradient of the upper crust and the requirement of a source for volcanic material. But the Cambridge mathematician-geologist William Hopkins (1793–1866) and others argued that such an earth – rotating and subject to tidal forces – would be unstable. Consequently, through the nineteenth and into the early twentieth century, the earth was generally assumed to have a solid interior, perhaps having occasional "lava lakes" in a thick crust, as Hopkins envisaged, or having a relatively thin (tideless) layer of fused rock containing superheated steam not far below the solid crust, as Fisher suggested.[25] Such models were eventually refuted by seismological investigations.

Numerous instruments were devised for detecting and/or recording earthquakes during the nineteenth century.[26] Essentially, they were pendulum devices, with movement of the weight relative to the support being recorded in some fashion. Initially, the leading instrument makers were in Italy, then Japan and Germany. Italy and Japan were natural places for seismological research, and Germany, under the influence of von Humboldt, became the leading country in geophysical research in the nineteenth century. Seismic observatories were established around the world – in university laboratories, astronomical observatories (often maintained by the Jesuits), meteorological stations, and other places.

A seismograph devised by Emil Wiechert (1861–1928) of Göttingen was widely adopted. It used a "reverse pendulum" (with the weight at the top

[22] Ibid., p. 136.

[23] Chris Harrison, "Lucien J. B. LaCoste: Portrait of a Scientist-Inventor," *Earth in Space*, 8 (1996), 12–13.

[24] Karl Sundberg, "The Boliden Gravimeter – A New Instrument for Ore Prospecting," *Bulletin of the Institution of Mining and Metallurgy*, no. 402 (1938), 1–25, and plate; R. D. Wyckoff, "The Gulf Gravimeter," *Geophysics*, 6 (1941), 13–33.

[25] On these developments, see Stephen G. Brush, "Nineteenth-Century Debates about the Inside of the Earth," *Annals of Science*, 36 (1979), 224–54.

[26] Graziano Ferrari, *Two Hundred Years of Seismic Instruments in Italy* (Bologna: Storia-Geofisica-Ambiente, 1992); Oldroyd, *Thinking about the Earth*, chap. 10.

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of the bar), and large motions were damped by air pistons. Vibrations were recorded continuously on moving strips of smoked paper. The markers were lifted every minute, so the time of any disturbance could be known. By the early twentieth century, many observatories were equipped with effective seismographs such as Wiechert's, so arrival times at different observatories could be compared for signals from the same earthquake.

In 1899, former Indian Geological Survey director Richard Oldham (1858–1936) – atypical in being both a geologist and a geophysicist – reported that the Assam earthquake (1897) produced two kinds of impulses, as detected in Italy. First came "condensational" (also known as "primary," "pressure," or "*P*") waves, then "distortional" ("secondary," "shear," or "*S*") waves, longitudinal and transverse, respectively. They apparently traveled through the earth – from Assam to Italy. Oldham also remarked that the wave velocities seemingly increased according to the distance traveled – comprehensible if they traveled faster at the higher temperatures and pressures in the earth's interior. Furthermore, the difference between the arrival times for *P* and *S* waves depended on the distance from the earthquake: The *S* waves were apparently delayed if the "distance" between earthquake and observatory was more than 120°. Oldham hypothesized that for some reason the *S* waves traveled abnormally slowly in the earth's central region, or perhaps they were blocked from passing through the earth's deep interior, only reaching the observatory by being (somehow) refracted around the outer part. Initially, Oldham preferred the first alternative. Even so, he envisaged some physical difference between the earth's central part (the "core") and the region above this, up to the crust. Making simplified geometrical assumptions about the waves' trajectories, he estimated the core's radius to be about 0.4 times that of the whole. But he did *not*, in 1906, propose a liquid core, blocking transmission of *S* (shear) waves. He had discovered the core, but not that it was liquid.[27]

A liquid core was proposed by Russian mathematician-geophysicist Leonid Leybenzon (1879–1951) in 1911, and by 1913 Oldham thought the core might be liquid and that what he had presumed were slowly arriving *S* waves – beyond 120° – were not *S* waves at all. But the liquid-core hypothesis did not immediately prevail. The influential German-American seismologist Beno Gutenberg (1889–1960) argued that tide studies, gravimetry, geodesy, nutation, and the argument that a rotating earth with a liquid interior would be unstable all indicated solidity.[28] Only seismology suggested a liquid core. Even so, seismologists agreed there was some kind of internal boundary about where Oldham suggested.

Movement toward accepting a liquid core followed a publication by the influential Harold Jeffreys that showed that it was compatible with the data,

[27] Richard D. Oldham, "Report on the Great Earthquake of 12 June 1897," *Memoirs of the Geological Survey of India*, 29 (1899), 1–379; Richard D. Oldham, "The Constitution of the Earth as Revealed by Earthquakes," *Quarterly Journal of the Geological Society*, 62 (1906), 456–75. See Stephen Brush, "Discovery of the Earth's Core," *American Journal of Physics*, 48 (1980), 705–24.

[28] Beno Gutenberg, *Der Aufbau der Erde* (Berlin: Gebruder Borntraeger, 1925).

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provided greater mantle rigidity was hypothesized. Jeffreys showed that, given a solid mantle, a liquid core did not preclude stability. The following decade, the Danish seismologist Inge Lehmann (1888–1993) discovered seismographic evidence for a solid inner core within the liquid core.[29]

Seismologists have made other fundamental contributions to knowledge of the earth's interior. The Croatian seismologist Andrija Mohorovicic (1857–1936), director of the Zagreb Observatory, reported a local earthquake that seemed to generate *two* sets of *P* and *S* waves, to explain which he proposed some kind of boundary within the earth at a depth of about 45 km. Today, this is believed to mark the boundary between the crust and mantle and is named the "Mohorovicic Discontinuity."[30]

In 1914, the American geologist Joseph Barrell (1869–1919) coined the term "asthenosphere" for a weak, plastic layer in the upper mantle, as isostasy theory required. Twelve years later, Gutenberg offered inconclusive evidence for a zone of decreased velocity at a depth of 70–100 km possibly related to this.[31] By the late 1920s, Jeffreys favored an asthenosphere. But the idea was not fully accepted until the 1950s, when disturbances from atomic blasts were investigated. A somewhat plastic interior was essential for "continental drift" theory or plate tectonics.

An important result was due to Kiyoo Wadati (1902–1995) of the Japanese Meteorological Agency, who discovered certain sloping "weak surfaces" around Japan where earthquakes were concentrated. Such "surfaces" were "rediscovered" by Hugo Benioff (1899–1968) of the California Institute of Technology[32] and (in plate tectonics theory) are thought to represent fault planes along which material is "subducted" into the earth's interior by mantle convection – somewhat as envisaged by Vening Meinesz.

Thus seismology has provided essential evidence concerning the earth's internal structure and is generally supportive of the plate tectonics paradigm. Recent work has explored the structure of the mantle–core boundary and movement of material in the core, which are relevant to geomagnetic theories.[33] Seismology also assists stratigraphers. To determine the thickness of the Greenland ice cap, explosives were detonated and the time for reception of

[29] See Brush, "Nineteenth-Century Debates about the Inside of the Earth." The term *Mantel* was introduced by Wiechert in 1896 to denote the large outer part of the earth's interior, beneath the crust.

[30] Andrija Mohorovicic, "Das Beben vom 8. X. 1909," *Jahrbuch des meteorologischen Observatoriums in Zagreb (Agram) für das Jahr 1909*, 9 (pt. 4, sec. 1) (1910), 1–65, and charts; James B. Macelwane and Frederick W. Sohon, *Introduction to Theoretical Seismology* (New York: Wiley; London: Chapman and Hall), vol. 1, p. 204.

[31] Joseph Barrell, "The Strength of the Earth's Crust," *Journal of Geology*, 22 (1914), 655–83; Beno Gutenberg, "Untersuchungen zur Frage, bis zu welcher Tiefe die Erde kristallin ist," *Zeitschrift für Geophysik*, 2 (1926), 24–9. See Oldroyd, *Thinking about the Earth*, pp. 238–40.

[32] Kiyoo Wadati, "On the Activity of Deep-Focus Earthquakes in the Japan Islands and Neighborhood," *Geophysical Magazine, Tokio*, 8 (1935), 305–25; Hugo Benioff, "Orogenesis and Deep Crustal Structure – Additional Evidence from Seismology," *Bulletin of the Geological Society of America*, 65 (1954), 385–400.

[33] See Vogel, *Naked Earth*.

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reflected signals determined. In the 1940s, Cambridge geophysicist Edward Bullard (1907–1980) and coworkers investigated the form of the Paleozoic floor of eastern England by this method.[34] Using more delicate apparatus, subterranean strata are now commonly "mapped" by such methods.

## GEOMAGNETISM

Geomagnetism is puzzling, with its many irregularities (secular, annual, diurnal, and geographical) in intensity, declination, and inclination, and the earth's field apparently reverses on occasion. As geomagnetism can be investigated with relatively simple apparatus, it has been closely studied for many years, the first site for regular observations having been established in Paris as early as 1667. Edmund Halley (1656–1743) published a chart showing lines of equal declination in about 1701.[35]

Some iron-free geomagnetic observatories were built in the eighteenth century. From 1828, observations in Alexander von Humboldt's Berlin laboratory were synchronized with those in Paris and in a mine at Freiberg. Humboldt hoped to find laws underlying the peculiarities of geomagnetic phenomena. He discovered, for example, that geomagnetic disturbances occurred simultaneously worldwide and could be correlated with sunspot activity. His program has been called "Humboldtian science"[36] and was without previous parallel except in astronomy. Through contacts Humboldt made during a Russian expedition in 1829, a network of observatories was established in that huge country. Prompted by Humboldt, Gauss established a "Magnetic Union" (1834), encouraging the establishment of observatories and coordination and publication of their observations. Two years later, Humboldt contacted the Royal Society, and after much debate, observatories were established throughout the British Empire.[37] Nineteenth-century explorers regularly made geomagnetic observations following the Royal Society's guidelines. The British Association likewise encouraged and coordinated geomagnetic work. Much work was done by the U.S. Coast and Geodetic Survey and later the Carnegie Institution. Geophysics has long required worldwide observations and has correspondingly been a leader in establishing

[34] E. C. Bullard, T. F. Gaskell, W. B. Harland, and C. Kerr-Grant, "Seismic Investigations on the Palaeozoic Floor of East England," *Philosophical Transactions, Series A*, 239 (1946), 29–94.

[35] Sydney Chapman and Julius Bartels, *Geomagnetism*, 2 vols. (Oxford: Clarendon Press, 1940), plate 38.

[36] A term suggested by Susan F. Cannon, *Science in Culture* (Flokestone: William Dawson; New York: Science History Publications, 1978). Von Humboldt's favored technique was to plot "isolines" for various quantities (biological or physical) and then account for the patterns. He collected observations rather than specimens. Slotten (*Patronage, Practice, and the Culture of American Science*) has seen the work of Bache and the U.S. Coast Survey as Humboldtian in character.

[37] See John Cawood, "The Magnetic Crusade: Science and Politics in Early Victorian Britain," *Isis*, 70 (1979), 493–518; Susan Zeller, *Inventing Canada: Early Victorian Science and the Idea of a Transcontinental Nation* (Toronto: University of Toronto Press, 1987), pt. II.

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international scientific associations. Gauss's Magnetic Union was a precursor of the International Association of Geomagnetism and Aeronomy.

As the nineteenth century progressed, instruments of ever-increasing sophistication were designed.[38] But geomagnetism was theoretically puzzling. Geomagnetic field strengths could be compared for two different localities by suspending a magnet and measuring its oscillation period at the two places. Declination and inclination were determinable by observing magnets suitably pivoted relative to the astronomical meridian and the horizontal, respectively. Lines of equal declination were found to be anything but smooth, and lines of equal intensity sometimes apparently formed crossing loops![39] To account for the observations, different numbers of geomagnetic poles were initially envisaged, but it proved impossible to "save the appearances" satisfactorily.

The laboratory of Gauss and Wilhelm Weber (1804–1891) was particularly important both for empirical and theoretical work. Abandoning the idea of "bar magnets" within the earth, Gauss hypothesized the existence of two magnetic fluids consisting of vast numbers of "north" and "south" particles, all attracting or repelling one another according to a "Newtonian" inverse-square law. From this assumption, and utilizing Pierre-Simon Laplace's (1749–1827) "spherical harmonics," Gauss developed equations to represent the field, into which empirical data could be inserted from fixed geomagnetic observatories. Successful predictions were made for magnetic quantities at other localities, notably the position of the south magnetic pole. Gauss emphasized that the method was compatible with different ultimate causes of geomagnetism. He thought irregular geomagnetic changes might have extraterrestrial causes, such as electric currents in the atmosphere manifested by auroras.[40] Gauss's work, like Clairaut's, was an articulation of the "paradigmatic" Newtonian force law.

Subsequently, Manchester physics professor Arthur Schuster (1851–1934) used Gauss's "harmonic analysis" and distinguished magnetism originating in the earth from that of extraterrestrial origin. Electric currents within the earth might cause geomagnetism. Diurnal variation, thought Schuster, was caused by currents in the atmosphere arising from tidal motions of solar origin. In 1918, Irish-born Cambridge mathematical physicist Joseph Larmor (1857–1942) suggested that both solar and terrestrial magnetism might be caused by some self-exciting dynamo, an idea developed by German-American

[38] Anita McConnell, *Geomagnetic Instruments before 1900* (London: Harriet Wynter, 1980); Robert P. Multhauf and Gregory Good, *A Brief History of Geomagnetism* (Washington, D.C.: Smithsonian Institution Press, 1987).

[39] Multhauf and Good, *Brief History of Geomagnetism*, p. 34 (Fig. 28).

[40] Carl Friedrich Gauss, "General Theory of Terrestrial Magnetism," in Taylor, *Scientific Memoirs, Selected from the Transactions of Foreign Academies of Science and Learned Societies*, vol. 2, pp. 184–251, 313–16, and plates (first published in German as *Resultate aus den Beobachtungen des Magnetismus Vereins*, 1839). For explication, see G. D. Garland, "The Contributions of Carl Friedrich Gauss to Geomagnetism," *Historia Mathematica*, 6 (1979), 5–24.

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physicist Walter Elsasser (1904–1991) in the 1940s. Currents are generated as the earth's fluid core moves across the earth's field, in turn producing magnetic field changes. As required, there would be secular changes in the field because, Elsasser suggested, oscillatory changes in the field amplitudes would be superimposed on the exponentially decaying currents. The westward drift of the north magnetic pole was attributed to the fluid core lagging behind the solid mantle. Bullard sought to account for local anomalies in the earth's surface field by eddies in the upper core and tried to show how a substantial field might be built up from a small initial random field. But neither Bullard nor Elsasser could account for magnetic reversals (which remain mysterious).[41]

Yet it is geomagnetic reversals that have been especially important to geologists. Early evidence for magnetization of lavas by the earth's field was found by Achille Delesse (1817–1881) and Macedonio Melloni (1798–1854). Bernard Brunhes (1867–1910) studied clays baked by lava flows and suggested that their magnetic orientations might be useful for stratigraphic correlation.[42] His work was supported by Motonori Matuyama (1884–1958) of Kyoto University, who discovered lavas with magnetic polarity opposite that of the present.[43] Skepticism remained, but by the 1960s there was evidence for paleomagnetic reversals from Iceland (Martin Rutten), Russia (Alexei Khramov), and Hawaii (Ian McDougall and Donald Tarling), and the idea became accepted.

The role of paleomagnetism in the plate tectonics revolution has been described by William Glen (see also Frankel, Chapter 20, this volume).[44] Briefly, studies of "remanent" magnetism in the 1950s led geophysicists such as Keith Runcorn (Britain) and Edward Irving (Australia) to propose that the positions of the earth's magnetic poles had apparently changed because of the slow movement of continents relative to the mantle below. In addition, the U.S. Navy and organizations such as the Scripps Institution in

[41] Arthur Schuster, "The Diurnal Variation of Terrestrial Magnetism," *Philosophical Transactions, Series A*, 180 (1889), 467–518; Joseph Larmor, "How Could a Rotating Body such as the Sun Become a Magnet?" *Report of the British Association for the Advancement of Science*, 1918 meeting, pp. 159–60; Walter M. Elsasser, "Induction Effects in Terrestrial Magnetism," *Physical Review*, 60 (1946), 876–83; Walter M. Elsasser, "The Earth's Interior and Geomagnetism," *Review of Modern Physics*, 22 (1950), 1–35; Walter M. Elsasser, "The Earth as a Dynamo," *Scientific American*, 198 (May 1958), 44–48; Edward C. Bullard, "The Secular Changes in the Earth's Magnetic Field," *Monthly Notices of the Royal Astronomical Society Geophysical Supplement*, 20 (1948), 248–57; Edward C. Bullard, "The Magnetic Field within the Earth," *Proceedings of the Royal Society, Series A*, 197 (1949), 433–53.

[42] Achille Delesse, "Sur le Magnétisme Polaire dans les Minéraux et dans les Roches," *Annales de Chimie et de Physiques*, 25 (1849), 194–209; Macedonio Melloni, "Du Magnétisme des Roches," *Comptes Rendus de l'Académie des Sciences, Paris*, 37 (1853), 966–8; Bernard Brunhes, "Recherches sur la Direction d'Aimantation des Roches Volcaniques," *Journal de Physique Théorique et Appliquée*, 5 (1906), 705–24.

[43] Motonori Mutuyama, "On the Direction of Magnetisation of Basalt in Japan, Tyôsen and Manchuria," in *Proceedings of the Fourth Pacific Science Congress Java, 1929* (Batavia–Bandoeng, 1930), vol. II B, pp. 567–9.

[44] Glen, *Road to Jaramillo*.

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San Diego did much work on magnetometry in the 1960s, with survey vessels towing sensitive instruments for measuring the geomagnetic field. The ocean floors were known to be dense basaltic rock, having become slightly magnetic when formed, as proposed by earlier workers. Unexpectedly, the magnetometers revealed that if the ocean basalts' magnetism was mapped with (say) black representing a rock's magnetism (over and above that of the general background) aligned in one direction and white for the reverse direction, then a "zebra" pattern resulted. Moreover, the pattern was symmetrical on either side of the known mid-ocean ridges. This quickly led to acceptance of the idea that magma welled up from the earth's interior along the oceanic ridges and slowly spread to either side, being moved by glacially slow convection currents in the mantle (seafloor spreading). As magma solidified on the ocean floor, its magnetic orientation coincided with that prevailing at the time. With a geomagnetic reversal (cause unknown), the basalts' magnetism would be reversed, hence the "stripes."[45]

## GEOLOGICAL SYNTHESIS FROM RESULTS OF GEOPHYSICAL INVESTIGATIONS

Geophysics thus played a major role in establishing the leading geological "paradigm" of the second half of the twentieth century. Geodesy and gravimetry supplied ideas about the earth's shape, the distribution of matter in its interior, and convection currents in the mantle. Seismology provided information about internal structure and subduction zones. Geomagnetic studies evidenced geomagnetic reversals (facilitating the development of a geomagnetic timescale), the movement of continents relative to one another, and seafloor spreading.[46]

However, one can see a more general "earth science" emerging before the plate tectonics synthesis. Sunspot activity, known to recur fairly regularly from Humboldt's time, was predicted for 1957–8, and in 1952, at Lloyd Berkner's suggestion, a major effort at data collecting was organized by the International Council of Scientific Unions and carried through under the program of the IGY. Mountains of data were collected on cosmic rays, geomagnetism, gravity, meteorology, seismology, and solar activity, for example, and provided a significant stimulus to the beginnings of space research. Much collaborative work followed, such as with the compilation of data banks in

[45] Leaders in the formulation of this idea were Harry Hess, Robert Dietz, Ronald Mason, Arthur Raff, Frederick Vine, Drummond Matthews, and Lawrence Morley. J. Tuzo Wilson added the idea of "transform faults."

[46] More recent work, combining geophysical and geochemical results, is revealing the many complexities of the earth's internal structure, composition, and behavior. A useful article, with some historical material, is: Michael Wysession, "The Inner Workings of the Earth," *American Scientist*, 83 (1995), 134–47.

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World Data Centres.[47] The politics of atomic testing also led to a massive increase in seismological research in the 1960s, with the establishment of the World-Wide Standardized Seismograph Network.[48] In 1961, the Canadian geophysicist-geologist Jock Tuzo Wilson (1908–1993), soon to be one of the main actors in the drama of the emergence of plate tectonics theory, recounted some of the events associated with the IGY that were beginning to transform earth science into planetary science.[49] Thus geology was already beginning to change from a subject concerned chiefly with the materials and history of the earth's crust into a much larger enterprise. Helpful preliminary consideration of this complex shift is given by Robert Wood.[50]

Although some geologists still stand out against plate tectonics ("expanding-earth" theory being an alternative model but of declining popularity[51]), the attraction of that theory is the way it integrates disparate areas of knowledge. Broadly speaking, the knowledge "coheres" – a good indication that it may be true. However, the theory still has problems. For example, mineralogical studies of materials under high temperature and pressure (see the section on "Geochemical Cycles") suggest that subducted material becomes "exhumed" after subduction quite rapidly, and tectonic theorists have yet to explain this satisfactorily. Thus, despite the theory's coherence with geological and geophysical information, there remain ("Kuhnian"?) anomalies, and active research continues. It is a difficult field, for it is at once specialized and broad. Whereas the history of the plate tectonics revolution has been closely studied (see Frankel, Chapter 20, this volume), historians have hardly begun to look at what has happened since, let alone use recent geology or geophysics as a field for philosophical consideration.[52]

## CHEMICAL ANALYSES OF ROCKS AND MINERALS

Techniques for the chemical analyses of rocks and minerals in the "wet" way were first devised by Swedish chemist Torbern Bergman (1735–1784).[53] He dissolved weighed samples in hot alkali, then precipitated a sequence of substances that were filtered off, ignited, and weighed. Bergman's results were grossly inaccurate, but his lead was followed by analysts such as Martin

47 Henry Rishbeth, "History and Evolution of the World Data Centre System," *Journal of Geomagnetism and Geoelectricity Supplement*, 43 (1991), 921–9.
48 Bruce A. Bolt, *Nuclear Explosions and Earthquakes: The Parted Veil* (San Francisco: W. H. Freeman, 1976).
49 J. Tuzo Wilson, *IGY: The Year of the New Moons* (London: Michael Joseph, 1961).
50 Wood, *Dark Side of the Earth*.
51 Warren S. Carey, *Theories of the Earth and Universe: A History of Dogma in the Earth Sciences* (Stanford, Calif.: Stanford University Press, 1988).
52 Brush (*History of Modern Planetary Physics*) does this to some extent, but not using recent philosophical work.
53 Torbern O. Bergman, "Disquisitio Chemica de Terra Gemmarum," *Nova Acta Regiae Societatis Scientiarum Upsaliensis*, 2 (1777), 137–70.

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Klaproth (1743–1817) of Germany, Nicholas Vauquelin (1763–1829) of France, and Jons Jacob Berzelius (1779–1848) of Sweden. By the 1830s, it was possible to ascertain chemical compositions reasonably satisfactorily in terms of the percentages of "earths" "contained" in rocks or minerals. An early research program for work of this kind has been usefully described by Jack Morrell.[54]

Knowledge of chemical compositions made chemical classifications of rocks or minerals feasible. Berzelius proposed one such system for minerals – an alternative to the prevailing systems based on external physical properties, such as Friedrich Mohs's (1773–1839). Berzelius's attempt was premature, but by 1849 August Breithaupt (1791–1873) of Freiberg, who earlier had classified by external features, was using chemical criteria. Yale geologist James Dana's (1813–1895) mineralogical text was likewise modified in later editions.[55]

## GEOCHEMISTRY

The term "geochemistry" was coined (1838) by Basel professor Christian Friedrich Schönbein (1799–1868), an electrochemist and the discoverer of ozone. He supposed that the earth's inorganic materials had been deposited according to chemical laws, so there were distinct chemical formations, analogous to different organic epochs. Indeed, the two might be interrelated. Schönbein called for "geochemic comparison" of bodies. Then the *chemist* might write the history of the globe. This monumental task, thought Schönbein, required its own Cuvier or Newton.[56]

In the early nineteenth century, the evolutionary theorist Jean-Baptiste Lamarck (1744–1829) envisaged the cycling of materials aided by living organisms. The French chemists Jean-Baptiste Dumas (1800–1884) and Jean Boussingault (1802–1887) studied the role of plants in chemical cycles. Analogous work was done by Giessen chemist Justus von Liebig (1803–1873), studying humus and fertilizers.[57]

[54] Jack B. Morrell, "The Chemist Breeders: The Research Schools of Liebig and Thomas Thomson," *Ambix*, 14 (1972), 1–46.

[55] Jons J. Berzelius, *An Attempt to Establish a Pure Scientific System of Mineralogy, by the Application of Electro-Chemical Theory and the Chemical Proportions*, trans. J. Black (London: Robert Baldwin; Edinburgh: William Blackwood, 1814); Friedrich Mohs, *The Natural History System of Minerals* (Edinburgh: W. and C. Tait, 1820); J. F. August Breithaupt, *Die Paragenesis der Mineralien: Mineralogisch, geognostich und chemisch beleuchtet, mit besonderer Rücksicht auf Bergbau* (Freiberg: J. G. Engelhardt, 1849); James D. Dana, *A System of Mineralogy* (New Haven, Conn.: Durrie & Peck & Herrick & Noyes, 1837).

[56] Christian Friedrich Schönbein, "On the Causes of the Change of Colour which Takes Place in Certain Substances under the Influence of Heat," *Annals of Electricity, Magnetism and Chemistry*, 5 (1840), 224–36 (translated from *Annalen der Physik und Chemie*, 1838). Some of the topics outlined herein are discussed at greater length in Oldroyd, *Thinking about the Earth*, chap. 9.

[57] Jean-Baptiste Lamarck, *Hydrogéologie* (Paris: the author, 1802); Jean-Baptiste Dumas and Jean-Baptiste Boussingault, *Essai de Statique Chimique des Étres Organisés* (Paris: Fortin, Masson et Ce, 1841); Justus von Liebig, *Die organische Chemie in ihrer Anwendung auf Agricultur und Physiologie* (Braunschweig: Vieweg und Sohn, 1840).

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Carl Gustav Bischof (1792–1870), chemistry professor at Bonn, produced a wealth of data on the compositions of gases, waters, minerals, and rocks. He regarded the earth as a "vast chemical laboratory," with cycles of elements, involving air and water. Low-temperature processes were important in forming rocks and ore bodies. Bischof thought that most rock-forming minerals could be derived from aqueous solutions, rock formation from melts being exceptional. Granite might be chemically altered slate; basalt was altered shale. Such "neo-Neptunist" views provided a German counterweight to the dominant "Plutonism" of nineteenth-century geology. A major argument against a "Plutonist" origin of (say) granite was that, when melted, quartz was the last component to liquefy; yet quartz crystals in granite did *not* seem to have formed first, for they apparently molded or shaped the silicate crystals (feldspars, etc.) and were presumably produced after them. Yet there were German "ultra-Plutonists," such as Justus Ludwig Roth (1819–1892), who thought that all crystalline rocks, from lavas to phyllites to slates, were of magmatic origin.[58]

Water's role in rock formation was studied by Gabriel-Auguste Daubrée (1814–1896), chief engineer of the Mines Department in France. He subjected substances to high temperatures and pressures, with or without water present, and concluded that metamorphism *could* occur without melting. Also, Daubrée could *not* produce granite in his pressure vessels. He supposed that past conditions might have been radically different from the present, with higher temperatures and pressures, and an atmosphere of different composition. Thus Daubrée's views were contrary to the "uniformitarian" principles then espoused in Britain.[59]

In Canada, the Geological Survey officer Thomas Sterry Hunt (1826–1892), working on Archaean rocks, attempted to provide a chemical "just-so" story for the earth's history, such as Schönbein had called for. Hunt imagined simple compounds interacting according to their known chemical properties: The crust would be made of silicate slags, and the dense atmosphere might have contained steam, carbon dioxide, hydrogen chloride, sulfur dioxide, oxides of nitrogen, and perhaps oxygen. On cooling, acidic oceans would form, with silica precipitated as "quartz rock." On further cooling, solid crust might be exposed, with erosion and deposition, and formation of new substances. Limestone and salt deposits formed as chemical precipitates, and eventually life forms began to play a role in the chemical interconversions. But Hunt's acceptance of evidence for Archaean life forms (*Eozoön*) was discredited when

[58] Carl Gustav Bischof, *Lehrbuch der chemischen und physikalischen Geologie*, 2 vols. in 4 (Bonn: A. Marcus, 1847–55); Justus Ludwig Adolph Roth, *Allgemeine und chemische Geologie*, 3 vols. (Berlin: W. Hertz, 1879–93).

[59] Gabriel-Auguste Daubrée, *Études et Expériences Synthétiques sur la Métamorphisme et sur la Formation des Roches Cristallines* (Paris: Imprimerie Royale, 1860). For discussion of nineteenth-century "Neptunism" and "Plutonism/vulcanism," see W. Nieuwenkamp, "Trends in Nineteenth Century Petrology," *Janus*, 62 (1975), 235–69.

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similar appearances were found in igneous rocks, and his "chemical geology" was premature in relation to available knowledge.[60]

## PHYSICO-CHEMICAL PETROLOGY

The "neo-Neptunists" and "Plutonists" disagreed as to the best explanation of the structures of crystalline igneous rocks. The problem needed to be tackled by experimental replication of cooling magmas under various pressures, with more or less water in the melts. Techniques for performing such experiments were not developed until the early twentieth century, especially after the founding of the Carnegie Institution of Washington.[61] There the physical chemistry of magma-like melts was studied, notably by Norman Bowen (1887–1956), and his school eventually dominated in a lengthy controversy concerning the origin of granite.

The Carnegie techniques were important because the earlier procedures of melting igneous rocks and letting them cool under controlled conditions gave uncertain information: The mixtures were generally so complex that the course of crystallization could not be followed. It proved more satisfactory to start with simple artificial silicate mixtures and investigate their behavior and then add components separately to ascertain the resulting changes. Various mixtures in different proportions were fused, cooled to different stages of solidification, and then rapidly quenched. By microscopic examination, the proportions of different substances in equilibrium at the elevated temperatures were thereby determined. Thus the crystallization processes of igneous rock melts could be studied.

Bowen showed that during crystallization of "mafic" rocks (those rich in ferromagnesian minerals, such as pyroxenes and amphiboles), the ferromagnesian minerals crystallized in a "discontinuous reaction series." On cooling, each material first formed reacted with the remaining melt to form the next mineral in the series and so on. By contrast, feldspars formed a "continuous reaction series" in which crystals interacted with the melt continuously until freezing was complete.[62] Put simply:

Decreasing temperature →

Olivines → Pyroxenes → Amphiboles → Biotites ↘
Potassic Feldspars → Muscovite → Quartz
Calcic Plagioclase Feldspars → Sodic Feldspars ↗

[60] Thomas Sterry Hunt, "On the Chemistry of the Primaeval Earth," *The Canadian Naturalist*, 3 (1867), 225–34; Charles F. O'Brien, "*Eozoön canadense*: The Dawn Animal of Canada," *Isis*, 61 (1970), 206–23.

[61] Hatten S. Yoder, "Development and Promotion of the Initial Scientific Program for the Geophysical Laboratory," in *The Earth, the Heavens and the Carnegie Institution of Washington*, ed. Gregory Good (Washington, D.C.: American Geophysical Union, 1994), pp. 21–8. (Note that the Carnegie Institution undertook both geophysical and geochemical work.)

[62] Norman L. Bowen, "The Reaction Principle in Petrogenesis," *Journal of Geology*, 30 (1922), 177–98.

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However, with evidence from, for example, the Baltic Shield, "magmatists" such as Jakob Sederholm (1863–1934) thought granites and gneisses might be produced by the alteration of sediments by penetration of hot, mineral-charged fluids, as Daubrée would have approved. In 1958, the "magmatists" Norman Bowen and Orville Tuttle of Pennsylvania State University showed that granite could be produced by cooling water-containing magmas. Yet, the same year, Helmut Winkler and Hilmar von Platen of Marburg University produced metamorphic gneiss and then granitic melts by the action of high temperature and pressure on clays mixed with sodium chloride.[63] Perhaps, as Harold Herbert Read (1889–1970) said, there could be "granites *and* granites."[64]

Later work, by geologists such as Oxford's Lawrence Wager, focused on the layered nature of some igneous rocks, particularly gabbros.[65] The idea was that, as melts cool, some materials may crystallize and settle under gravity, thereby altering the chemical composition of the remaining liquid, so that a succession of different minerals is formed. The idea goes back to Charles Darwin (1809–1882)[66] but provided an important research site in the second half of the twentieth century.

## GEOCHEMICAL CYCLES

After its premature realization by Hunt, Schönbein's grand program was revived by Vladimir Vernadsky (1863–1945). He studied chemistry under Dmitri Mendeleev (of periodic table fame) and soil science under Vasili Dukuchaev, also becoming interested in Henri Bergson's "evolutionary" philosophy. In early work, Vernadsky studied evidence for Precambrian life and considered that organisms might have helped form some Precambrian rocks (as today seems likely in some cases). Later, he studied the cycling of different elements through the atmosphere, oceans, living and dead organisms, and the earth's crust. He emphasized that the atmosphere's composition depended on living organisms.[67]

[63] Jakob J. Sederholm, "On Migmatites and Associated Pre-Cambrian Rocks of Southwestern Finland," *Bulletin de la Commission Géologique de Finland*, no. 58 (1923); Orville F. Tuttle and Norman L. Bowen, "Origin of Granite in the Light of Experimental Studies in the System $NaAlSi_3O_8$—$SiO_2$—$H_2O$," *Memoir of the Geological Society of America*, No. 74 (New York: Geological Society of America, 1958); Helmut G. F. Winkler and Hilmar von Platen, "Experimentelle Gesteinsmetamorphose – II: Bildung von anatektischen granitischen Schmelzen bei der Metamorphose von NaCl – führenden kalkfreien Tonen," *Geochimica et Cosmochimica Acta*, 15 (1958), 91–112.

[64] Harold Herbert Read, *The Granite Controversy* (New York: Interscience, 1957), p. 161.

[65] Lawrence R. Wager and G. Malcolm Brown, *Layered Igneous Rocks* (Edinburgh: Oliver and Boyd, 1968).

[66] See Paul N. Pearson, "Charles Darwin on the Origin and Diversity of Igneous Rocks," *Earth Sciences History*, 15 (1996), 49–67.

[67] Vladimir Ivanovich Vernadsky, *The Biosphere*, trans. David B. Langmuir (New York: Springer, 1997) (first Russian edition, 1926).

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Vernadsky's ideas were transmitted to the United States by his son and taken up at Yale by the ecologist George Hutchinson.[68] Another outstanding figure in the history of geochemistry was the Norwegian Victor Goldschmidt (1888–1947), whose monumental *Geochemistry*[69] treated the distribution of the elements in different parts of the earth, atmosphere, and other media over time, the evolution of magmatic rocks (using Bowen's work), and the principles of crystal chemistry. The basic questions were: Where have the different elements been distributed over time? And in what proportions? To answer, one requires extensive knowledge of the chemical composition of rocks, minerals, waters, and gases and their changes over time. This could only be acquired slowly, by compiling databases, as was done in geophysics.

General ideas on geochemical cycling were developed relatively recently, especially by the American geochemist Robert M. Garrels (1916–1988). He thought of movements in the lithosphere-hydrosphere-atmosphere as resembling those in a factory, with "pipes" carrying different elements from one repository to another, the whole being driven by the earth's internal radiogenic heat and by solar energy. Living organisms played an essential role in the movement of elements from one reservoir to another (such as when corals fix calcium carbonate). Moreover, elements may have been concentrated in different substances in different geological eras. For example, carbon has sometimes been concentrated in coal deposits but at other times in limestones. Sulfur may be concentrated in iron pyrites or in gypsum. Garrels (with Abraham Lerman) showed how the carbon and sulfur cycles were "coupled."[70] Thus geochemistry has begun to reveal "meaning" in the stratigraphic column.[71]

From geochemical evidence, it appears that life is of paramount importance in geohistory, maintaining the planet in a state conducive to the persistence of life by its "buffering action." Thus geochemistry offers insights of great interest in understanding the way the earth functions as an "equilibrated system," as well, of course, as providing techniques for the search for ore bodies, for example.

Geophysics and geochemistry have made fundamental contributions to geology, to our thinking about the earth, and how it functions as a planetary body and as a quasi-organism, not to mention "practical" matters such as earthquakes. The earth has been an object of great interest to professional physicists, astronomers, chemists, and would-be holists, not to mention

[68] George E. Hutchinson, *The Ecological Theater and the Evolutionary Play* (New Haven, Conn.: Yale University Press, 1965).
[69] Victor M. Goldschmidt, *Geochemistry* (Oxford: Clarendon Press, 1954).
[70] Robert M. Garrels and Frederick T. MacKenzie, *Evolution of Sedimentary Rocks* (New York: Norton, 1971); Robert M. Garrels and Abraham Lerman, "Coupling of Sedimentary Sulfur and Carbon Cycles – An Improved Model," *American Journal of Science*, 284 (1984), 989–1007.
[71] See Peter Westbroek, *Life as a Geological Force: Dynamics of the Earth* (New York: Norton, 1994).

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industrialists. Given the interest and importance of the sciences, they have attracted surprisingly little attention from historians. Geophysics and geochemistry offer rich fields for future historical research, especially of a synthetic character. But given that geology and geophysics/geochemistry did not become "married" until well into the second half of the twentieth century, it is perhaps understandable that historians of geology, relatively small in number and hitherto focusing chiefly on pre-twentieth-century topics, have not yet given its sister sciences the attention they deserve.

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# 22

# MATHEMATICAL MODELS

*Jeffrey C. Schank and Charles Twardy*

Early natural philosophers seeking to mathematicize nature almost certainly thought of themselves as seeing into the real foundations of the world, not as setting up models that might correspond to the observed phenomena. The language of "models" or "analogies" emerged first among late nineteenth-century physicists, and it is an interesting question (beyond the topic of this chapter) whether the explicit recognition of the modeling function marked a significant step toward the modern view of how science operates. In biology, where many at first believed the phenomena to be outside the scope of mathematical representation, the approach via models seemed to offer a way forward to those who felt that a bridge had to be built to the world of law and causality.

Mathematical modeling did not emerge as an important research strategy in the life sciences until the second decade of the twentieth century, but its origins properly lie in mid-nineteenth-century efforts to make the life sciences more like physics and in the growth of probability theory and mathematical statistics. At that time, European biologists were beginning to reject the idealist, vitalist biology of the German *Naturphilosophie* tradition, and several were turning toward the other physical sciences for inspiration. In particular, several young German physiologists and microbiologists advocated a reductionist biology that invoked only physico-chemical explanations, sometimes expressed as Newtonian force laws.[1] Reductionism did not flourish everywhere immediately, but even investigators who thought that some aspects of biology were not reducible to physics or chemistry agreed that one should start by trying to make such a reduction.[2]

Unlike in physics and economics, however, mathematical models had little impact on the development of biological thought until the first few

[1] David J. Depew and Bruce H. Weber, *Darwinism Evolving: Systems Dynamics and the Genealogy of Natural Selection* (Cambridge, Mass.: MIT Press, 1995).

[2] Everett Mendelsohn, "Physical Models and Physiological Concepts: Explanation in Nineteenth-Century Biology," *British Journal for the History of Science*, 2 (1965), 201–17.



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decades of the twentieth century, and even then there were biologists who objected to mathematization on the grounds that it oversimplified complex issues. Toward the end of the nineteenth century, biologists who supported Darwinian evolution theory began to marshal (and construct) *statistical* analyses to support their claims.[3] Statistical thinking eventually helped to facilitate the synthesis of Darwinism and genetics in the 1920s and set the stage for the mathematization of other parts of the life sciences. In later decades, we see (i) mathematical models as a critical part of the Darwinian synthesis, (ii) the use of population models to guide the development of theoretical and applied ecology, (iii) the spread of mathematical statistics throughout the life sciences, and (iv) the migration of mathematical models and modelers from the physical sciences to the life sciences. Today, with the introduction of computer technologies, we see more extensive uses of modeling in research (i.e., bioinformatics) and teaching, and even new experimental styles of modeling.

Because models always involve idealizations and modeling is a constructive process, we find that modeling in any area of the life sciences tends to begin with a model of a simple case – a highly idealized model, the frictionless pulley of the biological world. Subsequent models often add new elements or recombine elements from previous models. This often piecemeal evolution of models is an inevitable consequence of the constructive process of modeling influenced by a thicket of external and internal factors directly and indirectly affecting the evolution of models.

A modeler's perspective is shaped by a wide variety of influences. At the technical level, these include ontological and epistemological assumptions, modeling goals, and constraints on the analysis and testing of models. According to biologist Richard Levins, there are at least three mutually incompatible characteristics of models: *realism*, *precision*, and *generality*.[4] In Levins's view, a modeler sacrifices one (or more) of these characteristics depending on his perspective. Levins, for example, views Lotka-Volterra models as sacrificing realism for precision and generality; he considers this typical of the models inspired by mathematical physics. For such deep choices, however, it may come down to hunches about what kinds of choices will likely be most "fruitful," and these will be influenced by a host of practical and sometimes ideological factors.

Physical and formal analogies have played crucial roles in the history of mathematical modeling and are typically associated with problem-posing activities. When a modeler notices formal or physical analogies between two

[3] See Robert Olby, "The Dimensions of Scientific Debate: The Biometric–Mendelian Debate," *British Journal for the History of Science*, 22 (1988), 299–320; J. S. Wilkie, "Galton's Contribution to the Theory of Evolution, with Special Reference to His Use of Models and Metaphors," *Annals of Science*, 11 (1955), 194–205.

[4] R. Levins, "The Strategy of Model Building in Population Biology," *American Scientist*, 54 (1966), 421–31. For a recent survey of the general history of modeling, see Paola Cerrai, Paolo Feruglia, and Claudio Pellegrini, eds., *The Application of Mathematical Models to Nature: Critical Moments and Aspects* (New York: Kluwer/Plenum, 2002).

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things, one of which has a successful mathematical model, then the formal or physical analogy may suggest a similar model for the second system. Alfred J. Lotka, one of the founders of mathematical ecology, started with basic mathematical forms derived from physics and chemistry and then moved to problem-posing activities in ecology. The transfer of mathematical techniques from one area of science to another has created problems of particular interest to historians concerned with the professional identity of newly emerging disciplines.

Idealization occurs at every stage and activity in the process of modeling. This may amount to finding the simplest or easiest biological system or mechanism to work with, or the simplest and easiest conceptual idealization of a biological system. In model construction and analysis, it may amount to choosing a mathematical framework that is analytically or computationally tractable, which almost always involves approximations. In the course of debate with rivals, a model may be simplified even further, a tactic that may purchase comprehensibility even at the price of misrepresenting contemporary biological research and teaching.[5]

The form that a model ultimately takes may thus be influenced by factors unrelated to the technicalities involved. Traditionally, historians distinguished between internal factors deriving from the methodological, epistemological, and explanatory aspects of science and external factors, including psychological, social, political, and economic influences. More recently, however, the distinction between internal and external has become so blurred as to be virtually useless. For example, mathematical ecology was originally developed by "applied-side" biologists faced with the agricultural and economic need for pest control. This need guided the early researchers to problems of population stability, a research program that has shaped the subsequent development of the field. Although this external consideration was crucial, part of the staying power of this approach lies in internal factors: Population stability is a widespread concern of theoretical and practical importance in many areas of biology.

Social relationships and individual personality characteristics have figured prominently in the timing, development, and acceptance of mathematical models. As early as 1902, the mathematician George Udny Yule recognized that Mendelism was not incompatible with "continuous" evolutionary change, but social conflicts between the two primary biological camps prevented them from seeing how the two ideas could be welded together mathematically until the synthetic work of R. A. Fisher, J. B. S. Haldane, and Sewall Wright in the 1920s. Much has been written on these divisions, which have been interpreted in terms of philosophical and ideological disputes as well as professional rivalries. Similarly, the emergence of mathematical modeling

[5] J. R. Jungck, "Ten Equations That Changed Biology: Mathematics in Problem-Solving Biology Curricula," *Bioscene*, 23 (1997), 11–36.

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in ecology was accompanied by debates arising from differing perpectives on the nature of the problems to be addressed and the professional insecurity of some of the early modelers.[6]

It is beyond the scope of this chapter to provide an exhaustive survey of mathematical models and modeling in the life sciences. Instead we will survey *some* of the important historical threads that are representative of modeling in the life sciences, providing pointers to selected primary and secondary sources. We begin with three areas in which mathematical modeling has entered the life sciences: physiology and psychology, evolution and ecology, and development and form. The later parts of this survey focus on three more recent historical perspectives that we believe provide bridges from the origins of modeling to its current state. These are mathematical statistics, integrative modeling, and computers and mathematical modeling.

## PHYSIOLOGY AND PSYCHOLOGY

In 1835, Theodore Schwann (1810–1882) demonstrated the possibility of applying mathematical laws to biology by measuring the force of contraction of frog muscles of different lengths. Working in Berlin under Johannes Müller (1801–1858), Schwann found that the force of contraction varied with the length of the muscle and proclaimed that it was the first time "a vital process had been mathematically treated and included in a numerical force law." Although this is not quite correct, it made a big impression on other physiologists and catalyzed what may be called the Newtonian revolution in the life sciences.[7]

Hermann von Helmholtz (1821–1894), a physicist who did a great deal of work in sense physiology, was inspired by Schwann. Helmholtz was also a student of Müller, and his famous formulation of the principle of conservation of energy in 1847 was motivated by the antivitalism cultivated among Müller's students at the time, among them Emil Du Bois-Reymond (1818–1896). Helmholtz was convinced that vitalism amounted to perpetual motion, and he sought to refute it on physical grounds.[8] Helmholtz pursued this mechanistic research program, and some of his earliest physiological work was on the speed of propagation of a nerve impulse (1850), in which he opened the doors to the study and modeling of nerve physiology. Helmholtz

[6] The standard work on the biometry–Mendelism debate is W. B. Provine, *The Origins of Theoretical Population Genetics* (Chicago: University of Chicago, 1971). On ecology, see Sharon Kingsland, *Modeling Nature: Episodes in the History of Population Ecology* (Chicago: University of Chicago Press, 1985).

[7] Mendelsohn, "Physical Models and Physiological Concepts."

[8] F. Bevilacqua, "Helmholtz's *Ueber die Erhaltung der Kraft*: The Emergence of a Theoretical Physicist," in *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*, ed. David Cahan (Berkeley: University of California Press, 1993), pp. 291–333; E. Mendelsohn, "The Biological Sciences in the Nineteenth Century: Some Problems and Sources," *History of Science*, 34 (1964), 39–59.

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showed that nerve signals did not travel infinitely fast, nor even near the speed of light, but rather at the electrically unimpressive rate of about thirty meters per second, raising crucial questions about the mechanism of nerve signal propagation and forcing a revision in epistemology, a task Helmholtz himself pursued. Another important aspect of this experiment was Helmholtz's use of statistical error analysis to interpret his data, an avenue of mathematical modeling often overlooked because it is now so commonplace.[9]

Helmholtz did not investigate the inner workings of the nerve, leaving nerve electrophysiology unexplained until the mid-twentieth century. Two key steps along the way were the understanding of semipermeable membranes and the modeling of the action potential.[10] In 1888 and 1889, Hermann Nernst (1864–1941), a physicist-turned-chemist who had attended Helmholtz's thermodynamics lectures in Berlin, formulated an equation that specified how a membrane permeable only to one ion in a solution would give rise to an electric potential when that ion diffused through it. In 1912, Julius Bernstein (1839–1917), a physiologist and former student of Du Bois-Reymond in Berlin and assistant to Helmholtz at Heidelberg, suggested that nerve cells were surrounded by a living semipermeable membrane. If so, then by analogy he could use the Nernst equation to predict the resting electric potential of the nerve. Bernstein proposed that the permeability of the membrane might increase on electrical stimulation, suggesting a mechanism for the action potential.

Developing this idea in a series of papers in 1951 and 1952, biophysicists Alan L. Hodgkin and Andrew F. Huxley measured the potential of squid giant axons and found that a modified form of the highly idealized Nernst model could be fit very accurately to their measurements. In the final paper of the series,[11] they then proceeded to construct a dynamic model of the propagation of the nerve signal – the action potential – by analogy with an electric circuit that would give similar behavior. This parallel-capacitance model was very successful, and both the dynamical equations and the electrical analogy are still taught, even though further subcellular analysis has contributed greatly to a physical understanding of the workings of the ion channels in the membrane and the processes involved in synaptic transmission.

Sensory physiology at the macroscopic scale also became more mathematical in the late nineteenth century, starting with the work of Gustav Theodore Fechner (1801–1887) in Leipzig. Fechner, a generation older than Müller's students and a student of Ernst Weber (1795–1878), was not a reductionist, but

9 For a survey of perspectives on Helmholtz, see Cahan, *Hermann von Helmholtz and the Foundations of Nineteenth-Century Science*.

10 For a more detailed retrospective by a participant, see K. S. Cole, "Theory, Experiment, and the Nerve Impulse," in *Theoretical and Mathematical Biology*, ed. Talbot H. Waterman and Harold J. Morowitz (New York: Blaisdell, 1965), pp. 136–71.

11 A. L. Hodgkin and A. F. Huxley, "A Quantitative Description of Membrane Current and Its Application to Conduction and Excitation in Nerve," *Journal of Physiology*, 117 (1952), 500–44. See also B. R. Dworkin, *Learning and Physiological Regulation* (Chicago: University of Chicago Press, 1993).

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he did have some sympathies with that program. Originally a physicist, he insisted on experiments and based his famous law in part on Helmholtz's 1847 conservation paper. Fechner's law was the center of his *Elemente der Psychophysik* (1860), which defined the field of experimental psychology. Fechner's law was a reinterpretation of Weber's law, which stated that the larger a stimulus is, the more change is required before any difference becomes noticeable. Fechner asserted that if $S$ is the magnitude of sensation, then $S$ is a constant multiple of Weber's *just noticeable difference*. If we let $r$ be the unit of the stimulus, Fechner's law can be written as $S = k \log r$, which states that our sensation increases only as the logarithm of the stimulus. The law is simple, empirical, and limited, but its very formulation helped fuel the growing Newtonianization of the life sciences. In addition, Fechner advocated the use of statistical methods to deal with the variable measurements characteristic of psychology. Still, Fechner was creating a whole new field, and psychology as a whole did not adopt the mathematical approach until the 1930s, when "mathematical psychology in America went statistical with a vengeance."[12]

## EVOLUTION AND ECOLOGY

Although Charles Darwin (1809–1882) did not rely on mathematical reasoning in his theory of evolution, it is well known that he was influenced by the economic views expressed in Rev. Thomas Malthus's *Essay on the Principle of Population* (1798). Malthus (1766–1834) reasoned that because human populations tend to increase at a much faster rate than their food supply could match, the mass of humanity is doomed to suffer famine and plagues as nature equalizes consumption with production.[13] Biologists did not initially address Malthus's easily mathematizable model, although his work was picked up by economists and is still used as the basis of mathematical models of economic–demographic interactions.[14]

In the decades following the publication of Darwin's *Origin of Species*, the debate focused more on whether selection operated on relatively small, "continuous" variations or large saltations. Francis Galton (1822–1911) began the serious use of statistics in the investigation of inheritance, measuring parents' and offsprings' deviations from the statistical means of characteristics such as height and intelligence. Galton observed that the offspring of extreme parents tended to be less extreme for any given trait, a pattern he called "regression to the mean." This pattern led him to favor saltations somewhat, but he

[12] G. A. Miller, *Mathematics and Psychology* (New York: Wiley, 1964).

[13] Although this would appear to lead directly to natural selection, it need not. See P. J. Bowler, "Malthus, Darwin, and the Concept of Struggle," *Journal of the History of Ideas*, 37 (1976), 631–50, for an analysis of this problem.

[14] One example is M. L. Lee and D. Loschky, "Malthusian Population Oscillations," *The Economic Journal*, 97 (1987), 727–39.

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remained open to more selectionist views. The more extreme saltationists wielded the argument of "regression to the mean" as a vicious attack on the Darwinians, while on the other side, Galton's associate, the mathematician Karl Pearson (1857–1936), developed other parts of Galton's statistical intuitions and used them to defend the role of continuous variations against some of the saltationist attacks. In 1893, Pearson established a biometrical laboratory at University College London, later funded by the Drapers' Company. This was followed in 1908 by the Galton Eugenics Laboratory. It has been argued that Pearson's biometry was influenced by his concern for eugenics (selective breeding of the human population), and it has been claimed that his statistical techniques were actively developed to substantiate the eugenics policy. More recent studies have challenged this view, however, noting that Pearson's evolutionary and eugenic studies were not always closely linked in methodology.[15]

Most of the early Mendelians were saltationists and interpreted Mendel's newly discovered laws in this light. Inevitably, they opposed the biometricians, who favored Darwin's continuous variations. Both groups claimed Galton as their predecessor, and neither noticed that the two traditions were not necessarily incompatible, even though British mathematician G. Udny Yule had pointed this out in 1904. The beginnings of mathematical population genetics came from the elaboration of Mendel's[16] basic empirical formula, $A + 2Aa + a$. If instead of characteristics one models genes, the expression becomes $AA + 2Aa + aa$. Introduce frequencies $p$ and $q$ for alleles $A$ and $a$, respectively, and we have the Hardy-Weinberg equilibrium: $p^2 + 2pq + q^2 = 1$. The subsequent founders of mathematical models of evolutionary genetics, Ronald Aylmer Fisher (1890–1962), J. B. S. Haldane (1892–1964), and Sewall Wright (1889–1988), elaborated Mendel's laws in the context of selection acting on populations. All had an interest in breeding programs as well as evolution: Fisher was resident statistician at the Rothamstead Agricultural Research Station, Haldane (who was successively reader in biochemistry at Cambridge and professor of genetics at University College London) had a part-time appointment at the John Innes Horticultural Institution, and Wright moved from the U.S. Department of Agriculture to the University of Chicago. Although there were sharp disagreements over the specificity appropriate to the models and the role of complicating factors such as random drift, dominance, population structure, epistasis, and linkage, their work played the central role in the reconciliation between the

[15] The classic source on the biometrician–Mendelian conflict is Provine, *Origins of Theoretical Population Genetics*. The role of eugenics in the creation of Pearsonian statistics was urged in Donald Mackenzie, *Statistics in Britain, 1865–1930: The Social Construction of Scientific Knowledge* (Edinburgh: Edinburgh University Press, 1982). Against this, see Eileen Magnello, "Karl Pearson's Mathematization of Inheritance," *Annals of Science*, 55 (1998), 35–94; Eileen Magnello, "The Non-correlation of Biometrics and Eugenics," *History of Science*, 37 (1999), 79–106, 123–150.

[16] Frederick Churchill (personal communication) informs us that Mendel himself pointed out the regression to the heterozygote over generations of random inbreeding of populations.

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biometrical and Mendelian traditions in the modern Darwinian synthesis. The different modeling techniques were driven partly by different intellectual ancestries and partly by different philosophical and ideological perspectives. Fisher, for instance, worked originally on the kinetic theory of gases and was concerned with making his evolutionary theories compatible with his support for eugenics.[17]

As mathematical models of evolution were taking form, the relatively new discipline of ecology was also beginning to explore the possibilities of mathematical representation of population growth, predation, and competition. Sharon Kingsland has provided a detailed account of this episode, outlining the tensions created by the differing backgrounds of those involved and the problems encountered by the creation of a new scientific discipline.[18] One of the earliest modelers was Raymond Pearl (1879–1940), who worked with Pearson in London and took his mathematical approach back to the United States, working first at the Maine Agricultural Experiment Station from 1907 to 1918 and then at the School of Hygiene and Public Health at Johns Hopkins, eventually directing his own institute there.[19] Pearl conducted theoretical ecology in the Malthusian tradition. He was seeking nothing less than a law of population growth, and he thought he had found such a law in the logistic model. In modern terms, Malthus's model can be formulated as a differential equation where the rate of change of a population of size $N$ is equal to a constant, $r$, times $N$; that is, $dt/dN = rN$. This equation predicts unconstrained and unrealistic exponential growth. Pearl incorporated the idea of density dependence, represented by the parameter $K$, so that his model became $dN/dt = rN(1 - N/K)$, which slows the rate of growth as the population approaches the limiting parameter $K$, producing "S"-shaped or *logistic* curves when mathematically integrated. This was the first step in the evolution of a phylogeny of logistic models, and Pearl is largely responsible (and notorious) for propagating such variations.[20] Yule gave a largely laudatory presentation of Pearl's work to the British Association for the Advancement of Science in 1924 but cautioned against extrapolating too far from the data. Pearl's laws made no attempt to describe a mechanism, and biologists were rightly skeptical of his sometimes rash extrapolations and increasingly ad hoc curve fitting.[21]

This practiced skepticism may have contributed to the strong resistance felt by demographer Alfred J. Lotka (1880–1949), an outsider with a background in chemistry, who at the time was finishing a book, *Elements of Physical Biology*, that would eventually become a cornerstone of mathematical

[17] See the papers in S. Sarkar, *The Founders of Evolutionary Genetics* (Dordrecht: Kluwer, 1992). On the differences between Wright and Fisher, see also W. B. Provine, *Sewall Wright and Evolutionary Biology* (Chicago: University of Chicago Press, 1986).
[18] Kingsland, *Modeling Nature*.
[19] Ibid., chap. 3.
[20] Ibid., pp. 61–3.
[21] Ibid., chap. 4.

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modeling in ecology. Lotka was more squarely in the economic tradition, having worked in the insurance business, which had its own tradition of modeling strategies. But, in addition, his book's organization and themes are clearly derived from physics, especially thermodynamics. A similar model was proposed independently by Vito Volterra (1860–1940), professor of mathematical physics at the University of Rome. Lotka and Volterra developed a simple mathematical model of multiple-species (predator–prey) interactions that has since spawned its own phylogeny of models.

The case of Lotka and Volterra shows how mathematical analogy can be separate from physical analogy. Lotka followed a word-equation kind of reasoning from an elaborate analogy in physical chemistry, whereas Volterra used a "method of encounters" based on analogy with statistical mechanics. Yet both arrived at the same mathematical model. In addition, both systems were highly idealized, with homogeneous populations, no time lag, and no environmental interactions,[22] assumptions in concert with their rather abstract approaches to biology. These high-level systemwide dynamical models represent a very different approach from the currently emerging individual-based modeling approach in ecology, reflecting different sets of goals, preferences for level of detail, and resources for evaluating models. Both models represent an expansion of the Malthusian and logistic approaches in that whereas the logistic equations deal only with a single species, the Lotka-Volterra equations deal with two species together and are often referred to as the "predator–prey" equations.

The Australian entomologist Alexander Nicholson (1895–1969) drew from both Lotka and Malthus in his studies of limited population growth and the source of population stability. One of his students at the University of Sydney had noted that the traditional explanation, limited food supply, was not sufficient because pests did not usually consume all of the available food. Nicholson searched for some other mechanism that could be tied to population size and reasoned that a larger population was a bigger and easier target for predation than a smaller one. This provided the needed check on growth. With the help of a physicist colleague, Nicholson constructed a mathematical model that incorporated this mechanism, but it predicted increasing oscillations in host and parasite populations, which Nicholson thought was an unrealistic consequence.[23] The search for sources of stability has been the main driving force in the subsequent development of Nicholson-Bailey models.

There was opposition to these efforts to apply mathematical models from biologists who thought that this whole approach threatened to obscure the

[22] See G. Israel, "The Emergence of Biomathematics and the Case of Population Dynamics: A Revival of Mechanical Reductionism and Darwinism," *Science in Context*, 6 (1993), 469–509; G. Israel, "On the Contribution of Volterra and Lotka to the Development of Modern Biomathematics," *History and Philosophy of the Life Sciences*, 10 (1988), 37–49.

[23] Kingsland, *Modeling Nature*, pp. 116–26.

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complexity of natural interactions. A leading opponent was William Robin Thompson (1887–1972), superintendent of the Imperial Institute of Entomology in Britain, whose objections were prompted by his enthusiasm for an antimechanist worldview inspired by his Catholic faith. Only in the 1950s did modeling gain a wider base in scientific ecology, under the influence of Robert Helmer MacArthur (1930–1972), who studied under the ecologist George Evelyn Hutchinson (1903–1991) at Yale and then taught at the University of Pennsylvania and at Princeton. MacArthur effectively refocused attention onto the questions and methods pioneered by Lotka and others, and his views were ably championed by Hutchinson at the Cold Spring Harbor Symposium on Quantitative Biology in 1957. Even so, controversies over the most appropriate way to model ecology have been active and often vitriolic, especially where professional rivalries have given added force to conceptual differences.[24]

## DEVELOPMENT AND FORM

Although the study of morphology extends back well through the nineteenth century, D'Arcy Wentworth Thompson's book *On Growth and Form* (1917) provides a good starting point for the mathematical modeling of growth and development. In the words of Frederick Churchill, "Thompson was intent on demonstrating that mathematical and physical analysis could lend a penetrating interpretation, perhaps the only accurate interpretation, to organic form."[25] In his chapter "The Rate of Growth," Thompson declared that "mathematically speaking, organic form itself appears to us as a *function of time*," which inspired Julian Huxley to take the next step.

In his book *Problems of Relative Growth* (1932), Julian Huxley (1887–1975) elaborated on the formula for relative growth rate he had described in the journal *Nature* in 1924. The power-law formula $Y = bX^k$ related the growth of a body part $Y$ to that of the whole $X$, with two constants $b$ and $k$. Huxley's formula was derived empirically and thus did not theoretically explain the constancy of $k$. The basic model has persisted, with many other workers extending it with additional constants to handle a variety of allometric relationships. More recently, G. B. West and associates have attempted to explain the constancy of $k$ and the particular values it has for different systems in terms of the apparent fractal nature of plant and animal circulatory systems.[26]

[24] On Thompson and MacArthur, see ibid., pp. 134–43 and chap. 8. On later disputes, see Paolo Palladino, "Defining Ecology: Ecological Theories, Mathematical Models, and Applied Biology in the 1960s and 1970s," *Journal of the History of Biology*, 24 (1991), 223–43.

[25] F. B. Churchill, "On the Road to the *k* Constant: A Historical Introduction," introduction to Julian Huxley, *Problems of Relative Growth* (London: Methuen, 1932; Baltimore: Johns Hopkins University Press, 1993, reprinted), pp. ix–xlv.

[26] G. B. West, J. H. Brown, and B. J. Enquist, "A General Model for the Origin of Allometric Scaling Laws in Biology," *Science*, 276 (1997), 122–6.

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At the turn of the century, German zoologist and anatomist Theodor Boveri (1862–1915) was studying the fertilization of sea urchin eggs. Occasionally, sea urchin eggs are fertilized by two (dispermy) or more (polyspermy) spermatazoa, resulting in abnormal development. Through elegant comparative arguments and experiments that ruled out other possible causes, Boveri determined that the abnormal cleavage in polyspermic zygotes coincided with the irregular division of chromosomes at the first division into three or four blastomeres rather than the normal two. In sea urchins, the egg and sperm each contribute eighteen chromosomes, so dispermic eggs have fifty-four chromosomes rather than the normal thirty-six. They also have an extra centromere (each sperm contributes one), so that in the single-cell egg, three or four spindles are formed, with the fifty-four chromosomes distributed unevenly between the three or four spindles. Prior experiments suggested that if each chromosome was represented in a nucleus at least once, normal development could ensue. Thus, Boveri reasoned that it was not the difference in number but rather the distribution of chromosomes that mattered. A viable cell could have more than one copy of a certain chromosome, but it must have at least one copy of each.

Boveri, on the advice of a physicist friend, Wilhelm Wein, poured fifty-four well-mixed wooden spheres (numbers 1–18 three times) into a round tray and placed a wooden crossarm on the tray, thus randomly dividing the fifty-four spheres into three or four groups representing the three or four spindles of the dividing egg cell. He repeated the procedure two hundred times to get an estimate of the percentage of the time one would expect to observe normal versus abnormal development. Boveri's model agreed reasonably well with the actual outcomes – providing more support for his hypothesis that different chromosomes carry different genes – and was especially prescient in its anticipation of modern Monte Carlo simulation methods and individual-based modeling.[27] Physicist-biologist Max Delbrück (1906–1981), working in mid-century, distrusted mechanical models – a distrust he appears to have inherited from his mentor, the physicist Niels Bohr. However, he strongly supported the mathematical analysis of numerical data and designed experiments specifically to be amenable to the analytical techniques of physics. Indeed, Delbrück entered biology specifically hoping to find something like the "atoms of biology" and spent his first year in the United States traveling in search of an organism and methodology conducive to quantitative research. He found this in Emory Ellis's bacteriophages in 1938 and continued to work on them thereafter.[28] Delbrück introduced sophisticated statistical sampling of data distributions and derived characteristic logistic models of growth for each of his bacteriophage populations. By demonstrating the analytical clarity achieved with such methods and by training students to

[27] F. Baltzer, *Theodor Boveri* (Berkeley: University of California Press, 1967); C. Stern, "The Continuity of Genetics," *Daedalus*, 99 (1970), 882–907.

[28] Ernst Peter Fischer and Carol Lipson, *Thinking about Science: Max Delbrück and the Origins of Molecular Biology* (New York: Norton, 1988).

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follow these techniques at the annual Cold Spring Harbor summer phage symposia on Long Island, Delbrück advanced the state of the art of mathematical description and mathematical modeling of data in microbiology.[29] Delbrück's 1938 paper "The Growth of the Bacteriophage" introduced statistical sampling techniques to assess assays and gave characteristic logistic growth curves for phages.[30] A series of papers in 1940 expanded these analyses and introduced the Poisson and binomial distributions into phage research. In addition, Delbrück calculated flow rates across membranes using techniques from hydrodynamics, finding more characteristic rates for different phage populations.[31]

In the previous sections, we outlined some of the major threads in the history of mathematical modeling in the life sciences. This history is little explored, so one of our aims has been to outline components of a framework to further aid historical investigation. In the next three sections, we discuss three perspectives that link the origins of mathematical modeling to more recent modeling.

## MATHEMATICAL STATISTICS

It is easy to overlook mathematical statistics as a branch of mathematical modeling, at least beyond the specific roles this branch has played in the development of physiological and evolutionary modeling. This is likely because of a tacit distinction. Although both mathematical and statistical models are mathematical, mathematical models are typically viewed as idealized representations of biological systems, processes, and mechanisms, whereas statistical models are viewed as trustworthy tools for (i) measuring actual statistical properties of biological systems and their relationships, (ii) assessing errors in measurements, and (iii) deciding which measurements and hypotheses we should accept as highly probable or true. This distinction is at best superficial. Statistical models are also idealized and subject to all of the external and internal factors that influence mathematical models – perhaps even more so!

Statistical models are the most common form of mathematical model in the life sciences today. Mathematical statistics as a synthesis of combinations of observations and the use of probability for inferential purposes emerged around 1810. However, it was not until the latter half of the nineteenth century that mathematical statistics blossomed in the life sciences, as a statistical revolution led by four men: Galton, Francis Ysidro Edgeworth, Pearson, and Yule. Their ideas and development of mathematical regression and correlation laid the foundations for modern parametric and nonparametric statistics,

[29] L. E. Kay, "Conceptual Models and Analytical Tools: The Biology of Physicist Max Delbrück," *Journal of the History of Biology*, 18 (1985), 207–46.
[30] Max Delbrück, "The Growth of the Bacteriophage," *Journal of General Physiology*, 22 (1938), 365–84.
[31] Kay, "Conceptual Models and Analytical Tools."

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and especially the nearly universal use of analysis of variance models (first explicitly formulated by R. A. Fisher) in the life sciences.[32]

The development of mathematical statistics nicely illustrates two of the features of mathematical modeling we discussed earlier. First, the development of correlation models by Galton, Edgeworth, Pearson, and Yule can be viewed as a phylogeny of models. More recently, the analysis of variance model variants developed are all related to the basic analysis of variance breakdown (first specified by Yule) of a set of observations into their variance components.[33] Second, the modeler's perspective and especially the model's psychological aspect have played a crucial role in the historical development of mathematical statistics. This is brought out clearly in Pearson's views on the usefulness and generality of Yule's formulation of linear regression in 1897. Stigler has interpreted this conflict in terms of their differing perspectives:

> Pearson's reactions to Yule's work were thus strongly affected by his somewhat limited view of the techniques [of linear regression] as tools in biology. From this perspective his concerns seem sensible (even insightful); from a broader perspective they may not.
>
> Yule, on the other hand, was attempting to go beyond biological problems into realms where a different type of relationship was very much the issue. And from this different perspective Yule saw things differently. He looked upon a regression line as a surrogate for a causal relation rather than as a mere characteristic of a frequency surface.[34]

It was Yule's perspective that allowed regression to move beyond a mere law of inheritance. Some of the broader influences on the work of Pearson, Fisher, and others were discussed earlier in the context of their contributions to evolution theory.

## INTEGRATIVE MODELING: AN EXAMPLE FROM THE NEUROSCIENCES

The development of mathematical modeling has depended in no small part on the integration of concepts, data, and mathematical methods from different fields. This is nowhere more true than in the history of mathematical modeling in the neurosciences. As a descendant of nineteenth-century mathematical modeling in physiology, it has been influenced by the Newtonian perspective and, through McCulloch and Pitts's (1943) logical model of the neuron, by the development of modern logic.[35]

32 S. M. Stigler, *the History of Statistics: The Measurement of Uncertainty before 1900* (Cambridge, Mass.: Harvard University Press, 1986); Theodore M. Porter, *The Rise of Statistical Thinking, 1820–1900* (Princeton, N.J.: Princeton University Press, 1986).

33 Ibid.

34 Ibid., p. 353.

35 W. S. McCulloch and W. H. Pitts, "A Logical Calculus of the Ideas Immanent in Nervous Activity," *Bulletin of Mathematical Biophysics*, 5 (1943), 115–33.

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The modeling of visuomotor coordination in frogs is a fascinating example of the emergence of integrative and interdisciplinary modeling, bringing together at least three historical threads and general perspectives: (i) the ethological and neuroethological perspectives of von Uexküll, Lorenz, and Tinbergen; (ii) the neurobiological perspective of Hubel and Wiesel and the seminal paper in this field, "What the frog's eye tells the frog's brain," of McCulloch and associates; and (iii) the computer simulation modeling perspective of Arbib and associates beginning in 1970 with the work of Didday. Since the 1970s, European and American ethologists, neurobiologists, and modelers have been developing computational models of visuomotor coordination in frog prey capture, highlighted by international workshops in 1981, 1987, and 1996. The field is rich in phylogenies of computational models and should provide a good case for the kind of historical analysis we have suggested. Indeed, Arbib has advocated following a strategy of model construction we described earlier as phylogenetic and piecemeal: "If a number of models have been established, further modeling should – to the *extent possible* – be incremental, in that new models should refine, modify and build upon prior models, rather than being constructed *ab initio*."[36] This is an area rich in cross-disciplinary interactions and should be a fascinating area for the study of social, personal, and technological (e.g., over the Internet) influences on the process of modeling.

## COMPUTERS AND MATHEMATICAL MODELING

No history of mathematical modeling would be complete without discussing the profound impact that computers are having on modeling. The early (mid-twentieth-century) influence of computers on mathematical modeling was through their speed of numerical calculation. Models as sets of analytically intractable differential equations could now be solved using brute force numerical calculations. Models from mathematical statistics could be applied to larger and larger datasets requiring not only rapid numerical calculations but also the development of algorithms for manipulating them.

The development of digital computers greatly enhanced the use of graphical representation for the visual analysis of mathematical models, making possible the discovery of novel formal properties in even simple mathematical models, such as the logistic growth equation, and the use of mathematical models for teaching undergraduates general principles of ecology.[37]

[36] M. A. Arbib, "Visuomotor Coordination: Neural Models and Perceptual Robotics," in *Visuomotor Coordination: Amphibians, Comparisons, Models, and Robots*, ed. J. P. Ewert and M. A. Arbib (Kassel: Plenum Press, 1989), pp. 121–71, at p. 125.

[37] See B. Jones, W. Sterner, and J. Schank, "Biota: An Object-Oriented Tool for Modeling Complex Ecological Systems," *Mathematical and Computer Modeling*, 20 (1994), 31–40; W. C. Wimsatt and J. C. Schank, "Modelling – A Primer," in *The BioQUEST Library*, vol. 4, ed. J. R. Jungck, N. Peterson, and J. N. Calley (New York: Academic Press, 1999), pp. 1–233.

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Computers have also allowed the general and massive implementation of Monte Carlo simulation methods – analogous to Boveri's physical simulations – for the analysis of mathematical models. Monte Carlo methods are often required for highly nonlinear mathematical integration problems and provide completely general estimates of quantitative uncertainties in datasets when assumptions of analytical statistics (e.g., normality of the underlying distribution) fail. However, in accomplishing these tasks, computers function mainly as tools for doing calculations and data and graphical manipulations faster than humans can.

A new style, or perhaps better a cluster of related styles, of experimental mathematical modeling began to emerge in the late 1960s and early 1970s called *individual-based* modeling (agent-based and cellular-automata models are prime examples).[38] We cannot possibly do justice to the variety of styles of individual-based modeling in the life sciences, but we can say that its focus is on modeling individuals (or their parts) and their interactions. The basic strategy of analysis is experimental: Run simulation experiments and analyze the resulting data over a range of parameter values. If simulation experiments are run over and over again under similar circumstances (with different initial conditions), one would expect to find the results converging (statistically) in the long run – a strategy of experimental modeling reminiscent of Boveri's physical simulation experiments of abnormal development in sea urchins. This new style of modeling has been made possible by the explosive growth in late twentieth-century computational power (which should continue well into the twenty-first century) and has the essential hallmarks of an experimental approach; it marks a fundamentally new style of modeling. In population ecology, individual-based modeling marks a qualitative change in the style of thinking about modeling, which contrasts sharply with traditional modeling, in which state variables are population densities and parameters represent aggregate properties of individuals such as growth rate, competition, or predation.[39]

## CONCLUSIONS

The history of mathematical models in the life sciences is not just the history of applying mathematics to living systems. It is rich in process and the myriad of factors influencing it. We have alluded to a number of factors – social,

[38] See D. L. DeAngelis and L. J. Grows, eds., *Individual-Based Models and Approaches in Ecology* (New York: Routledge, 1992); O. P. Judson, "The Rise of the Individual-Based Models in Ecology," *Trends in Ecology and Evolution*, 9 (1994), 9–14. For an earlier expression of this view, see S. A. Kauffman, "Articulation of Parts Explanation in Biology and the Rational Search for Them," in *PSA 1970*, ed. R. C. Buck and R. S. Cohen, Boston Studies in the Philosophy of Science, vol. 8 (Dordrecht: Reidel, 1971), pp. 257–72.

[39] Judson, "Rise of Individual-Based Models in Ecology," pp. 9–14.

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psychological, technological – that have influenced the style and process of modeling. The evolution of mathematical models in specific areas of the life sciences suggests, from a historical perspective, that they may be fruitfully viewed as species analogues that "evolve" and are subject to phylogenetic analysis. Because mathematical models require precise formulation, specific assumptions and mathematical relationships can be traced, yielding a phylogeny of intellectual descent. There are several possible approaches to constructing phylogenies of models. The simplest, where applicable, is mathematical identity under special and limiting conditions, but less formal approaches – such as the cladistic approach described by Stone[40] – will likely have wider application, especially with the introduction of computer technologies into modeling. Indeed, the introduction of computers has not merely facilitated mathematical modeling but introduced a new experimental style of modeling called *individual-based modeling*.

We have had to omit a variety of mathematical models and important historical threads in this chapter. We have almost entirely ignored the development of optimality models (e.g., game theory and optimal design) in biology, which have arisen from work in economics and engineering. Nor have we discussed Kauffman's computational models, which attempt to integrate gene control, development, and evolution;[41] the development of artificial life models; the development of quantitative genetic models from applied animal breeding, and their extensive elaboration by the "Chicago school" of quantitative genetics; the development of cladistic models for phylogenetic analysis;[42] or the mathematical modeling of epidemics, which starts perhaps with the work of William Ogilvie Kermack and Anderson Gray McKendrick. Finally, during the early and middle parts of the twentieth century, a number of new threads of modeling emerged, including models of biological time; various types of models of animal behavior, including social structure and pecking orders; signal detection models and information theory models; connectionist modeling; and models in biochemistry and pharmacology, biomechanics, and genetic coding. In all of these modeling episodes, there are many historical stories yet to be told.

[40] J. R. Stone, "The Evolution of Ideas: A Phylogeny of Shell Models," *American Naturalist*, 148 (1996), 904–29; see also J. C. Schank and T. J. Koehnle, "Modelling Complex Behavioral Systems," in *Modelling Biology: Structures, Behaviors, Evolution*, ed. M. D. Lamblicher and G. B. Muller (Cambridge, Mass.: MIT Press, 2007), pp. 219–44.

[41] S. A. Kauffman, *The Origins of Order: Self-Organization and Selection in Evolution* (Oxford: Oxford University Press, 1993).

[42] See David L. Hull, *Science as a Process* (Chicago: University of Chicago Press, 1988).

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# 23

# GENES

*Richard M. Burian and Doris T. Zallen*

In this chapter, we describe traditional historical accounts of the gene and gene concepts and raise some issues from recent revisionist historiography dealing with this topic. Histories of the gene and genetics are still in their infancy. Until the mid-1970s, most histories were written by scientists and reflected the viewpoints of the victors in scientific controversies.[1] Only recently have professional historians contested traditional accounts and probed deeply into lost aspects of the history of the gene.[2] Recent biological work has raised doubt whether there is such an entity as "the" gene. Historians now disagree about whether the gene should count as an invention or a discovery, whether the history involved is fundamentally continuous or discontinuous, and how technical and theoretical developments in genetics are connected to larger social issues, including eugenics, genome projects, genetic medicine, and biotechnological "interference" with nature.

## BEFORE MENDEL

From prehistoric times, people have recognized that like begets like and have believed in some form of inheritance of acquired characters, which was used

[1] One of the best of these is Elof A. Carlson, *The Gene: A Critical History* (Philadelphia: Saunders, 1966; repr. ed., Ames: Iowa State University Press, 1989). See also L. C. Dunn, *A Short History of Genetics* (New York: McGraw-Hill, 1965); L. C. Dunn, ed., *Genetics in the 20th Century: Essays on the Progress of Genetics during Its First 50 Years* (New York: Macmillan, 1951); Alfred Sturtevant, *A History of Genetics* (New York: Harper, 1965). On the origins of molecular biology, see John Cairns, Gunther S. Stent, and James D. Watson, eds., *Phage and the Origin of Molecular Biology* (Cold Spring Harbor, N.Y.: Cold Spring Harbor Laboratory of Quantitative Biology, 1966).

[2] See Jonathan Harwood, *Styles of Scientific Thought: The German Genetics Community, 1900–1933* (Chicago: University of Chicago Press, 1993); Robert C. Olby, *The Path to the Double Helix* (Seattle: University of Washington Press, 1974); Jan Sapp, *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics* (New York: Oxford University Press, 1986).



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to help explain familial inheritance of character traits and physique.[3] Later, it was used to explain susceptibility to particular diseases, such as syphilis and tuberculosis, and the adaptation of imported plants and domesticated animals to their new environments. The Hippocratics had already developed explicit theories in support of such inheritance,[4] but sustained efforts to develop particulate theories of heredity began with the introduction of the idea of evolution in the writings of such figures as Erasmus Darwin (1731–1802), Jean-Baptiste Lamarck (1744–1829), and, above all, Charles Darwin (1809–1882).[5] Around 1900, most biologists still thought that, whatever detailed principles or mechanisms were involved, a theory of heredity must find a way of explaining the inheritance of acquired characters.

Unlike most mid-nineteenth-century evolutionists, Charles Darwin drew deeply on breeding work. In the first chapter of *On the Origin of Species*, he used artificial selection as a model for natural selection. His later "provisional hypothesis of pangenesis" also drew on knowledge of breeding. According to that theory, each cell casts off minute particles ("gemmules") that circulate to the gonads. The process of fertilization activates most of the gemmules, which, if adequately nourished, begin to form cells and organs like those from which they were derived. Others remain latent. This doctrine allowed Darwin to sketch hypothetical explanations of many phenomena, including inheritance of oversized biceps by blacksmiths' sons, loss of eyes by cave animals, and (thanks to latent gemmules) atavisms or reversions to ancestral traits.[6]

## FROM MENDEL TO THE TURN OF THE CENTURY

Gregor Johann Mendel (1822–1884) was a scientifically trained Moravian monk. His influence on genetics stems from work concerning hybridization of plant "species" (or varieties – the German word *Art* is ambiguous between the two) to produce stable new "species," not work in genetics in *our* sense.[7] Mendel designed a powerful method for testing whether carefully

[3] For general background, see William Coleman, *Biology in the Nineteenth Century* (New York: Wiley, 1971); Ernst Mayr, *The Growth of Biological Thought: Diversity, Evolution, and Inheritance* (Cambridge, Mass.: Harvard University Press, 1982); Robert C. Olby, *Origins of Mendelism*, rev. ed. (Chicago: University of Chicago Press, 1985); Hans Stubbe, *History of Genetics, from Prehistoric Times to the Rediscovery of Mendel's Laws*, trans. T. R. W. Waters (Cambridge, Mass.: MIT Press, 1965).

[4] Conway Zirkle, "The Early History of the Inheritance of Acquired Characters and of Pangenesis," *Transactions of the American Philosophical Society*, n.s., 35 (1946), 91–151.

[5] See the biographies in the *Dictionary of Scientific Biography* for these and most individuals for whom we supply dates.

[6] Charles Darwin, *On the Origin of Species* (London: John Murray, 1859) and, for pangenesis, Charles Darwin, *The Variation of Animals and Plants under Domestication* (London: John Murray, 1868), chap. 27.

[7] Gregor Mendel, "Versuche über Pflanzen-Hybriden," *Verhandlungen des naturforschenden Vereines in Brün*, 4 (1866), 3–47, Engl. trans. Eva Sherwood in *The Origin of Genetics: A Mendel Source Book*, ed. Curt Stern and Eva Sherwood (San Francisco: W. H. Freeman, 1966). See Viteslav Orel, *Mendel* (New York: Oxford University Press, 1984), and, for a contrasting account, Olby, *Origins of Mendelism*,

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chosen sharp differences between varieties of garden peas were inherited discretely and for examining the distribution of those differences in subsequent generations. In the seven cases he tested, the first hybrid generation was uniform for one of a pair of alternating traits (e.g., green or yellow seed coat color); he called that trait "dominant" and the other "recessive." In the second generation, obtained by self-fertilizing plants from the first generation, one-quarter of the offspring had the dominant trait and, when self-fertilized, produced only the dominant trait, one-half had the dominant trait but produced recessives as well as dominants, and one-quarter had the recessive trait and produced only plants with that trait. Mendel theorized that the "elements" (also called "factors") causing the particular traits are preserved unaltered in the gametes (egg and pollen cells), providing continuity from one generation to the next. On the basis of the distribution of multiple traits through a series of generations (and employing statistical tools he learned from physics), Mendel proposed the now-famous laws of segregation and independent assortment for these elements. This proposal, ahead of its time if ever any proposal was, fell on only a few ears, all of them effectively deaf to it.

In the 1870s, developments in microscopy plus new dyes helped scientists visualize the internal parts of cells and improve cell theory. During the 1880s and 1890s, with great difficulty, microscopists worked out the dance of the chromosomes in mitosis (ordinary cell division) and meiosis (formation of gametes). By 1900, there was partial consensus that chromosomes divide longitudinally and that each gamete receives only one of each pair of chromosomes. These findings soon suggested a plausible mechanism that could yield Mendelian segregation of factors.[8] Shortly after the "rediscovery" of Mendel's paper in 1900, Theodor Boveri (1862–1915), Walter Sutton (1877–1916), and others emphasized this connection between the new knowledge of chromosomes and Mendel's theory, arguing that chromosomes are the bearers of Mendelian factors, with each gamete getting one of each pair of factors.[9]

Starting in 1883, August Weismann (1834–1914) argued strenuously that germ-line cells are segregated from somatic cells very early in development and thus cannot be altered by environmental changes that alter somatic

especially "Mendel no Mendelian," pp. 234–58. For general background, see Garland Allen, *Life Science in the Twentieth Century* (Cambridge: Cambridge University Press, 1975); Peter J. Bowler, *The Mendelian Revolution* (Baltimore: Johns Hopkins University Press, 1989).

8 But there is considerable evidence that the cytological findings were stabilized in interaction with Mendelian findings after 1900. See Alice Baxter and John Farley, "Mendel and Meiosis," *Journal of the History of Biology*, 12 (1979), 137–73.

9 Boveri used experiments with sea urchins to argue that each chromosome is a distinct individual and that a full complement of chromosomes is required to produce viable offspring. See Theodor Boveri, "Über mehrpolige Mitosen als Mittel zur Analyse des Zellkerns," *Verhandlungen der physikalischen-medizinischen Gesellschaft zu Würzburg*, 35 (1902), 67–90. Sutton started from the cytological behavior of chromosomes. See Walter S. Sutton, "The Chromosomes in Heredity," *Biological Bulletin*, 4 (1903), 231–51.

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cells.[10] Accordingly, characteristics acquired during the organism's life cannot be inherited. This marks a key turning point; it made it conceptually possible to separate the transmission of determinants from an account of how they accomplished their functions and encouraged experiments aimed at learning how hereditary traits are transmitted. Weismann's doctrines became the focus of enormous public controversy, remaining influential even after many of his specific doctrines were discredited, and helped set the context in which Mendel's work was rediscovered.[11]

## THE DEVELOPMENT OF GENETICS AND THE GENE CONCEPT UP TO WORLD WAR II

In 1900, three botanists explicitly acknowledged the importance of Mendel's findings: Hugo De Vries (1848–1925), Carl Correns (1864–1933), and Erich von Tshermak-Seysenegg (1871–1962). All three discovered Mendelian ratios in their own experiments and subsequently found Mendel's text.[12]

The response to this "rediscovery" was very rapid. William Bateson (1861–1926), originally trained as a traditional British Darwinian, played the role of Mendel's bulldog. Early on, he employed embryology and morphology in order to understand the course of evolution and phylogenetic histories, but he became disillusioned in the 1890s and became an advocate of the importance of discontinuous variation. Bateson denied that Mendelian factors (his term) could be material particles or substances because they had to direct the development of the organism, something that he was convinced mere material particles could not do. Instead, he apparently thought of them as some sort of stable harmonic resonance.[13]

Mendelism quickly became a large-scale enterprise, thanks partly to the controversies it evoked and partly to support from plant and animal

[10] See, for example, August Weismann, *Die Continuität des Keimplasmas als Grundlage einer Theorie der Vererbung* (Jena: Gustav Fischer, 1885), translated as chap. 4 in *Essays upon Heredity and Kindred Biological Problems*, vol. 1, ed. Edward B. Poulton, Selmar Schoenland, and Arthur E. Shipley (Oxford: Clarendon Press, 1889).

[11] See, for example, Jane Maienschein, "Preformation or New Formation – or Neither or Both?" in *A History of Embryology*, ed. T. J. Horder, J. A. Witkowski, and C. C. Wylie (Cambridge: Cambridge University Press, 1986), pp. 73–108; Jane Maienschein, "Heredity/Development in the United States, circa 1900," *History and Philosophy of the Life Sciences*, 9 (1987), 79–93; Jane Maienschein, "Cell Theory and Development," in *Companion to the History of Modern Science*, ed. R. C. Olby, G. N. Cantor, J. R. R. Christie, and M. J. S. Hodge (London: Routledge, 1990), pp. 357–73.

[12] See Stern and Sherwood, *Origin of Genetics*, which contains English translations of the de Vries and Correns papers. Twenty-seven original papers (including Mendel's) are reprinted in Jaroslav Krizenecky, ed., *Fundamenta Genetica* (Prague: Publishing House of Czechoslovakia Academy of Science; Brno: Moravian Museum, 1965).

[13] See Alan G. Cock, "William Bateson, Mendelism, and Biometry," *Journal of the History of Biology*, 6 (1973), 1–36; William Coleman, "Bateson and Chromosomes: Conservative Thought in Science," *Centaurus*, 15 (1970), 228–314; William B. Provine, *The Origins of Theoretical Population Genetics* (Chicago: University of Chicago Press, 1971). Coleman discusses Bateson's arguments against interpreting factors as particles.

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breeders[14] and from agricultural stations, especially in the United States. Much work went into delimiting traits inherited in Mendelian fashion, demonstrating that such "Mendelizing" traits are found in all sorts of plants and animals, and applying Mendelism to practical breeding. Much of the fundamental vocabulary of genetics was elaborated from 1900 to 1910, including such terms as "allele" (originally "allelomorph"), "homozygote" and "heterozygote," "genetics," "gene," "genotype," and "phenotype." During the same period, numerous theoretical conceptions of Mendelian factors were put forward, many of them quite vague. Although these pointed in different directions, most of them sought to link factors to the development of organisms and formation of species and varieties. Because the developmental and evolutionary consequences of Mendelism were not readily tested, those issues were gradually dismissed as speculative and set aside. By 1910, younger Mendelians (especially in the United States) came to focus increasingly on the phenomenology and mechanics of trait transmission and to require that theories of genetic change be testable. The resultant successes narrowed the concept of the gene toward transmitted causal factors whose differences are reflected in phenotypic differences inherited in a Mendelian pattern. These successes reinforced the belief that doctrines of heredity were finally making genuine progress.

Starting in 1910, T. H. Morgan (1866–1945) and his students developed the theory that came to dominate the field from about 1915 on – the classical theory of genes as linearly arrayed particles on chromosomes. Although there had been intimations of such a theory since the 1890s (especially Sutton's contributions), the Morgan group provided a detailed, closely reasoned, and testable way of combining cytological knowledge of the behavior of chromosomes and genetic knowledge of Mendelian factors. They worked with a very advantageous organism, the fruit fly *Drosophila melanogaster* – an insect with only four pairs of morphologically distinct chromosomes, easily raised in the laboratory, with a short generation time, high fecundity, and easily controlled crosses, allowing them to create and follow lineages.[15] In 1910, they found a white-eyed fly whose eye color was inherited in a sex-linked

[14] Jean Gayon and Doris Zallen, "The Role of the Vilmorin Company in the Promotion and Diffusion of the Experimental Science of Heredity in France, 1840–1920," *Journal of the History of Biology*, 31 (1998), 241–62; Barbara A. Kimmelman, "The American Breeder's Association: Genetics and Eugenics in an Agricultural Context, 1903–13," *Social Studies of Science*, 13 (1983), 163–204; Diane B. Paul and Barbara A. Kimmelman, "Mendel in America: Theory and Practice, 1900–1919," in *The American Development of Biology*, ed. Ronald Rainger, Keith R. Benson, and Jane Maienschein (Philadelphia: University of Pennsylvania Press, 1988), pp. 281–309; G. Olsson, ed., *Svalöf, 1886–1986, Research and Results in Plant Breeding* (Stockholm: LTS Förlag, 1986).

[15] See Carlson, *The Gene*; Garland E. Allen, *Thomas Hunt Morgan: The Man and His Science* (Princeton, N.J.: Princeton University Press, 1978). For the advantages and peculiarities of *Drosophila*, see Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994). For the interplay between cytology and genetics in constructing and testing the chromosome theory of the gene, see Lindley Darden, *Theory Change in Science: Strategies from Mendelian Genetics* (New York: Oxford University Press, 1991).

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pattern; that is, transmitted with the male-determining X chromosome. By 1912, they had found six different X-chromosome mutations. Some of these did not assort independently; they occurred together in specific frequencies greater than 50 percent. Given the pairwise combinations, the six formed a "linkage group" of factors that appeared together more often than expected.

In 1911, Morgan developed a key notion based on cytological findings by Frans Alfons Janssens (1863–1924), a Belgian cytologist. When the chromosomes twist around each other during meiosis, they sometimes break and rejoin, with a block of material "crossing over" from one chromosome to the other. If Mendelian factors occupied fixed places along the chromosome, as the Morgan group hypothesized, crossing over would allow testing of their relative locations. Using the hypotheses that factors are linearly arrayed on the chromosome and that the frequency of crossing over increases with distance along the chromosome, one of Morgan's undergraduates, Alfred Sturtevant (1891–1970), used the statistics of linkage (co-occurrence) among the six X-linked factors to construct the first genetic map.[16]

The major features of the chromosomal theory of the gene were fixed by about 1915, although most of them were still controversial. Genetics, the science of the gene, developed rapidly into a major biological discipline located, conceptually, near the heart of biology because it claimed to specify how key features of organisms are determined. The key event was the publication in 1915 of *The Mechanism of Mendelian Heredity*, a textbook produced by Morgan's group that covered many plants and animals, with special attention to examples from *Drosophila*.[17] This textbook synthesized findings from many sources, supporting the following claims, among others:

- Chromosomes are the bearers of the hereditary material.
- Genes (not "unit characters") are the fundamental units of heredity.
- Genes are arrayed linearly on chromosomes.
- The number of linkage groups of genes (with overlapping nonindependent assortment) equals the number of chromosomes.
- Although each distinct gene may have many alleles, it remains unchanged except by mutation.
- Environmental factors (e.g., temperature and nutrition) can influence the effects of some genes.
- Some genes can modify the effects of other genes, sometimes quite specifically.
- Genes themselves are not altered when their effects are changed by modifier genes.

[16] F. A. Janssens, "La theorie de la chiasmatypie," *La Cellule*, 25 (1909), 389; Thomas Hunt Morgan, "Random Segregation versus Coupling in Mendelian Inheritance," *Science*, 34 (1911), 384; Alfred H. Sturtevant, "The Linear Arrangement of Six Sex-Linked Factors in *Drosophila*, as Shown by Their Mode of Association," *Journal of Experimental Zoology*, 14 (1913), 43–59.

[17] Thomas Hunt Morgan, Alfred H. Sturtevant, Hermann J. Muller, and Calvin B. Bridges, *The Mechanism of Mendelian Heredity* (New York: Henry Holt, 1915, rev. ed., 1922). See also Thomas Hunt Morgan, *The Theory of the Gene* (New Haven, Conn.: Yale University Press, 1926).

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- Genes must cooperate in large numbers to yield observable traits.
- Many mutations have large effects, but many more have small effects.
- Even though the pathways from genes to characters are wholly unknown, Mendel's principles, interpreted via the gene theory, provide "a scientific explanation of heredity [that] fulfills all the requirements of any causal explanation" (rev. ed., p. 281).

Morgan and his colleagues deemphasized hereditary phenomena that could not be explained by their theory. For example, in *Mechanism of Mendelian Heredity* they argued that the few known cases of cytoplasmic inheritance could be explained as inheritance of (potentially) self-reproducing particles (e.g., chloroplasts) in the cytoplasm or as the delayed effects of maternal genetic input into the egg (e.g., the color of egg membranes).

The consolidation of the chromosomal theory is marked by a terminological change. Morgan et al. still employed the term "factor" in *Mechanism of Mendelian Heredity*. By 1920, they had switched to "gene," emphasizing the specific commitments of the chromosomal theory. Although numerous hard-fought debates took place over refinements and specific issues, that theory dominated genetics until after World War II.

One other important development, initiated in 1927 by Herman J. Muller (1890–1967), was the discovery that x-rays could drastically alter the rate of mutation.[18] The interaction of x-rays with genes provided a way of interfering with genes that promised to be helpful in studying their structure. It also stimulated interest in genetics among physicists, some of whom made lasting contributions to genetics starting in the 1930s.

As of 1940, three long-standing problems remained to plague genetics and give opponents from other disciplines grounds for raising objections to the new science.

The first of these problems was the chemical composition of the gene. Once the gene was classed as a material entity, it became necessary to analyze its material and structural properties. The requirements to be met were spelled out by Muller. He emphasized three remarkable properties that must be explained by the composition or structure of the gene. First, genes are "autocatalytic" (i.e., they can duplicate or reproduce themselves). Second, they are "heterocatalytic" (i.e., they can catalyze, direct, or otherwise control the formation of substances different from those of which they are composed, including all the proteins, lipids, and carbohydrates in the bodies of all organisms). Finally, they retain both their autocatalytic and heterocatalytic powers even after mutation, so they must allow structural changes that do not remove these powers.[19] Proteins were the primary candidates for the

[18] Hermann J. Muller, "Artificial Transmutation of the Gene," *Science*, 66 (1927), 84–7.

[19] Hermann J. Muller, "Variation Due to Change in the Individual Gene," *American Naturalist*, 56 (1922), 32–50. See also Hermann J. Muller, "The Gene," *Proceedings of the Royal Society B*, 134 (1947), 1–37.

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gene material because they were diverse enough in composition and structure, stable enough, and present in sufficient quantity on chromosomes to be able to provide the necessary specificity, stability, and structural variety. The only other component of chromosomes plentiful enough to be considered was nucleic acid. But DNA, long known to be present in chromosomes, seemed highly unsuitable. It had only four variable parts (the nucleotide bases adenine, guanine, cytosine, and thymine, abbreviated A, G, C, and T) and was thought to be structurally uniform, with a "boring" series of repeated nucleotides in a fixed order. Such a molecule could not serve a genetic role.[20]

The second problem that plagued genetics concerned the connections between genetics and evolution. Although R. A. Fisher (1890–1962), J. B. S. Haldane (1892–1964), and Sewall Wright (1889–1988) elaborated the mathematical foundations of theoretical population genetics in the 1920s and 1930s, it remained unclear whether the theory of the gene could be adequately reconciled with naturalists' theories of evolution. The so-called evolutionary synthesis, begun in the late 1930s, did not really take hold until the 1940s and 1950s (see Hodge, Chapter 14, this volume).

The final problem that raised objections to genetics was the relationship between genetics and embryology. Genetics still did not explain the development of the organism from fertilization through all the stages of its life history to the end of its life. The Morgan school by and large set this problem aside as intractable. Among the founders of genetics, Bateson and Wilhelm Johannsen (1857–1927) had been skeptical of the ability of the chromosomal theory of the gene to accomplish this task. Most embryologists and many European geneticists shared this skepticism. They maintained that an adequate theory of heredity had to explain how genetic factors guided or determined development, much of which was (or seemed to be) controlled by events in the cytoplasm, not the nucleus.

From the beginning, there was considerable tension between two philosophical poles in the interpretation of the gene. Some theorists held that the gene is a formal device for representing breeding results. If there was an entity, the gene, behind those representations, it was not yet adequately characterized. Johannsen, in 1909, intended the very term "gene" to capture this view. He meant the term to be theoretically uncommitted as a label for whatever was transmitted in the pattern characteristic of Mendelian factors. One could entertain hypotheses, but one had to remain agnostic, for example between a materialist view such as Morgan's and a dynamicist view such as Bateson's. L. J. Stadler (1896–1954), a major plant geneticist, propounded a similar view from his deathbed in 1954.[21] He distinguished between the "operational gene," which could be delimited by breeding criteria, and the "hypothetical

[20] Olby, *Path to the Double Helix*; Horace Freeland Judson, *The Eighth Day of Creation: Makers of the Revolution in Biology*, expanded ed. (Cold Spring Harbor, N.Y.: Cold Spring Harbor Laboratory Press, 1996).

[21] Lewis John Stadler, "The Gene," *Science*, 120 (1954), 811–19.

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gene," which required intrinsic characterization. Agreement about the "operational gene" could be achieved straightforwardly by applying a clear-cut calculational system to actual experimental results, but Stadler did not expect agreement on the "hypothetical gene" until the distant future at best.

## POSTWAR NOVELTIES: THE MATERIAL OF THE GENE AND GENE ACTION

At least two sorts of knowledge, neither available in 1940, were required to resolve the foundational problems of genetics: What are genes made of and how do they act? These questions were addressed during World War II as well as after, when large numbers of scientists, many trained in other disciplines (especially biochemistry and physics), entered the field, bringing new techniques and approaches with them.[22] By war's end, new approaches, findings, and tools made it possible to pursue genetics at the molecular level, thereby transforming the discipline. There were two major sorts of changes in tools and techniques. One was the utilization of radioactive tracers, electron microscopes, ultracentrifuges, and other tools that allowed geneticists to follow cellular organelles and molecular components through various reactions and processes. The second was the use of microorganisms. Most microorganisms had been unanalyzable via Mendelian techniques because they do not exhibit regular sexual crossing and because those that do were too small and hard to handle for analysis of their lineages. Until the late 1940s, most geneticists and bacteriologists thought that bacteria (which do not have a true nucleus) did not have a system of genes like those of higher organisms. The groundwork for removing this obstacle was laid during World War II.[23]

Oswald Avery (1877–1955) and his colleagues pursued a line of work with a bacterium unfamiliar to geneticists. They showed that transfer of a substance identified as deoxyribonucleic acid (DNA) could transform *Pneumococcus pneumoniae* from one antigenic structure to another – and from nonvirulence to virulence.[24] Work on the nutrition of bacteria and microorganisms with nuclei (e.g., yeasts, fungi, and protozoa), begun in the 1930s, showed that basic nutritional requirements are universal. During World War II, George Beadle (1903–1989) and his colleagues employed the bread mold *Neurospora* to screen for and study mutations affecting nutritional needs. By 1945 they had

[22] The most important general histories covering this period are Judson, *Eighth Day of Creation*, and Michel Morange, *A History of Molecular Biology*, trans. Matthew Cobb (Cambridge, Mass.: Harvard University Press, 1998).

[23] Thomas D. Brock, *The Emergence of Bacterial Genetics* (Cold Spring Harbor, N.Y.: Cold Spring Harbor Laboratory Press, 1990).

[24] Oswald T. Avery, Colin M. MacLeod, and MacLyn McCarty, "Studies on the Chemical Nature of the Substance Inducing Transformation of Pneumococcal Types. I. Induction of Transformation by a Deoxyribonucleic Acid Fraction Isolated from *Pneumococcus* type III," *Journal of Experimental Biology and Medicine*, 79 (1944), 137–58.

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shown that each of the *Neurospora* genes affecting nutrition are responsible for directing the formation of a single enzyme that enters into the metabolism of the organism. This claim was soon generalized to yield the hypothesis that one gene produces one enzyme.[25] In France, Jacques Monod (1910–1976) showed how to separate the genetic capacity of certain bacteria to digest certain sugars from the actual presence of the enzymes required to do the digestion. Some bacteria have the ability to control the production of enzymes to digest particular sugars. For example, some bacteria do not produce enzymes to digest lactose when glucose is present or lactose is absent but, as Monod showed, have the genetically determined ability to switch by producing the enzymes necessary to digest lactose when it is present but glucose is not.[26] In retrospect, though it was not obvious at the time, this was a first step toward understanding the regulation of gene action. Max Delbrück (1906–1981) and Salvador Luria (1912–1991) began working with viruses that attack bacteria ("bacteriophages" or just "phages") and were able to show that a few bacteria in a large culture have preexisting genes that enable them to resist phages.[27]

The identification of the genetic material was the first major problem to fall. After World War II, work that built on the results of Avery et al. and many others made it clear that both DNA and RNA are more intimately involved in gene physiology than had previously been recognized[28] and that the DNA molecule was massively larger than had been anticipated. By 1952, a small group of geneticists and biochemists were convinced that DNA is the genetic material; they analyzed its chemistry, its roles in the cell, and its structure in every possible way. The most famous biological discovery of the era is James Watson (b. 1928) and Francis Crick's (1916–2004) discovery of the double-helical structure of DNA in 1953, which they accomplished by means of x-ray crystallography and model building.[29] A key aspect of this structure was the complementary pairing of A with T and G with C in the interior of the helix. The structure suggested a solution for the problem of

[25] George W. Beadle and Edward L. Tatum, "Genetic Control of Biochemical Reactions in *Neurospora*," *Proceedings of the National Academy of Sciences USA*, 27 (1941), 499–506; George W. Beadle, "The Genetic Control of Biochemical Reactions," *Harvey Lectures*, 40 (1945), 179–94. See also Norman H. Horowitz, "Fifty Years Ago: The *Neurospora* Revolution," *Genetics*, 127 (1991), 631–5; Lily Kay, "Selling Pure Science in Wartime: The Biochemical Genetics of G. W. Beadle," *Journal of the History of Biology*, 22 (1989), 73–101.

[26] Jacques L. Monod, "The Phenomenon of Enzymatic Adaptation and Its Bearing on Problems of Genetics and Cellular Differentiation," *Growth Symposium*, 11 (1947), 223–89.

[27] Salvador E. Luria and Max Delbrück, "Mutations of Bacteria from Virus Sensitivity to Virus Resistance," *Genetics*, 28 (1943), 491–511.

[28] Alfred D. Hershey and Martha Chase, "Independent Functions of Viral Protein and Nucleic Acid in Growth of Bacteriophage," *Journal of General Physiology*, 36 (1952), 39–56. Other work pointed in the same direction, such as Jean Brachet, "The Localization and the Role of Ribonucleic Acid in the Cell," *New York Academy of Sciences*, 50 (1950), 861–9. See Olby, *Path to the Double Helix*; Franklin H. Portugal and Jack S. Cohen, *A Century of DNA* (Cambridge, Mass.: MIT Press, 1979).

[29] James D. Watson, *The Double Helix* (New York: Atheneum, 1968), or the Norton Critical Edition, ed. G. Stent (New York: Norton, 1980).

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gene replication (autocatalysis). All that was needed to copy the double helix was to open up the original double helix and use each of the complementary strands as a template for making a new strand.

The problem of how DNA specifies a product, however, remained unsolved. Crick was perhaps the most important theoretician to attack this problem. He and various colleagues showed that a genetic code would probably employ a sequence of three nucleotides (a "codon") to specify one amino acid, with the sequence of codons specifying the sequence of amino acids needed to yield a protein. But in the end the detailed solution of the code was found by the techniques of wet biochemistry. Those details depended on the mechanism of protein synthesis (a long-standing biochemical problem) and required two major intermediate steps.[30]

One of these was the discovery of small RNA molecules that link specific codons to specific amino acids. They were discovered biochemically by the combined work of many groups, especially that of Paul Zamecnik.[31] He and his colleagues discovered "soluble RNAs" required to "activate" amino acids – that is, provide them with the energy to be added to a protein. These molecules, now called transfer RNAs (tRNAs), are intermediary molecules that link codons and amino acids. Crick had predicted the existence of such intermediaries, calling them "adaptors."

The second step concerned the way the information contained in the sequence of DNA nucleotides is brought to ribosomes, the units in the cytoplasm where proteins are assembled. François Jacob (b. 1920) and Monod produced the key findings between 1958 and 1961. In brief, a "messenger RNA" (mRNA) is "transcribed" from the DNA and "read" in the cytoplasm by the ribosomes. As a ribosome proceeds along a strand of mRNA, it "translates" the mRNA nucleotide sequence into an amino acid sequence by moving along the mRNA one codon at a time, picking off the amino acid on a transfer RNA linked to that codon and adding it to the growing protein chain.[32]

Once the differences between mRNA and tRNA were understood and techniques for making proteins in vitro were developed, it was possible to solve the genetic code. This was done, between 1961 and 1966, by difficult biochemical experiments performed in many laboratories. In essence, these workers made highly repetitious synthetic mRNAs and analyzed the resulting protein chains. By matching each RNA codon to an amino acid, they slowly filled in the conversion table from nucleic acid to protein.[33]

[30] Judson, *Eighth Day of Creation*, chap. 8.

[31] Hans-Jörg Rheinberger, *Towards a History of Epistemic Things: Synthesizing Proteins in the Test Tube* (Stanford, Calif.: Stanford University Press, 1997).

[32] See Judson, *Eighth Day of Creation*, chap. 7; Morange, *History of Molecular Biology*, chap. 13.

[33] The breakthrough was the first production of a protein from an artificial mRNA; see Marshall W. Nirenberg and J. Heinrich Matthei, "The Dependence of Cell-Free Protein Synthesis in *E. coli* upon Naturally Occurring or Synthetic Polyribonucleotides," *Proceedings of the National Academy of Sciences USA*, 47 (1961), 1588–1602. See also Judson, *Eighth Day of Creation*, chap. 8; Morange, *History of Molecular Biology*, chap. 12.

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In related experiments from 1958 to 1961, Jacob and Monod solved another major problem – describing a key mechanism by means of which gene action is regulated. They constructed the "operon" model, according to which a DNA region called the operator, which works like a switch that is opened and closed by environmental signals, determines whether a group of genes is transcribed to mRNA or not. Such a group of genes typically controls a significant trait (e.g., the ability to digest a particular sugar). Jacob and Monod divided genes into different classes – some that produce enzymes and others that regulate the expression of other genes. Their model thus provided a clear understanding, for bacteria at least, of the difference between genetic potential (what protein-producing genes are present) and the regulation of genes and gene action (how the control system determines which genes are expressed and when).[34]

Starting about 1975, new findings concerning the control of gene expression in eukaryotes (organisms with true nuclei) complicated this picture considerably. The correspondence between the nucleotide sequence in DNA (or the RNA of RNA viruses) and the eventual product is subject to enormous physiological modulation. A few examples illustrate these new complexities.

- *Variations in the genetic code.* The genetic code depends on the pairing of nucleotide triplets on mRNA with amino acids on tRNAs. A few organisms and the mitochondria of many organisms have tRNAs with nonstandard pairings. For example, in *Drosophila*, whether the codon AGA is translated as the amino acid serine or the amino acid arginine depends on whether it is translated inside a mitochondrion or in the cytoplasm of the cell. Thus, the protein product made from a given nucleotide sequence is context dependent.
- *Genes in pieces.* Typical eukaryotic genes that encode proteins have many more nucleotides than are expressed in the protein product. Noncoding segments ("introns") interrupt the coding material. To get from the DNA nucleotide sequence to the protein, one must understand the regulatory apparatus that excises the introns from the mRNA transcript. Many factors, including cell type, environmental conditions, and developmental stage, can influence the pattern of excisions.
- *Shuffling of parts of genes.* Both in organismal development (e.g., in immune system genes) and on an evolutionary timescale, parts of genes are moved around as units and recombined to yield novel products. On an organismal scale, this means that the DNA of a fertilized egg does not contain the entire structure of the adult genome. On an evolutionary scale, it means that the units of evolutionary change include parts of genes (sometimes corresponding to functional protein domains) and factors that control gene organization and expression.
- *Processing of mRNA transcripts.* In many circumstances, after mRNA reaches the cytoplasm, it is altered in ways that change its message. The alterations are

[34] François Jacob and Jacques L. Monod, "Genetic Regulatory Mechanisms in the Synthesis of Proteins," *Journal of Molecular Biology*, 3 (1961), 318–56; François Jacob and Jacques L. Monod, "On the Regulation of Gene Activity: $\beta$-galactosidase Formation in *E. coli*," *Cold Spring Harbor Symposia on Quantitative Biology*, 26 (1961), 193–211. See also Judson *Eighth Day of Creation*, chap. 7.

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often significant – in different organs, the same mRNA transcript yields *different* proteins. The reason is that a specific change in the transcript, made after the transcript has reached the cytoplasm of the relevant cells, alters the protein. For example, in the intestines and liver of mammals, a T is substituted for a C in the transcript coding for a protein called apolipoprotein-B, with the result that the protein is truncated earlier (and works differently) in the intestine than in the liver.[35]

These illustrations support a straightforward point. If a gene is identified by reference to its product or to what it does, it cannot be identified simply by its nucleotide sequence. On the other hand, if a gene is identified by a nucleotide sequence, more is required to infer what it makes or what it does. Because the genome is dynamic, because the correspondence between structure and function depends on context, and because genes are identified by mixed structural and functional criteria, there is no single correct way to delimit them. But this means that scientists who delimit genes in different ways will not agree about which changes to the genetic material should count as gene mutations. This complication of going from a description of the genetic material to an account of the behavior of genes appears to be unavoidable.

## THE GENE IN THE LIGHT OF RECENT HISTORIOGRAPHY

Most traditional histories of the gene emphasize two striking characteristics: homogeneity and linearity. That is, the historical development of the field is represented as if there were a single mainstream tradition. It thus seems that, thanks to a reductionist agenda, research moved in a relatively straight intellectual line from the rediscovery of Mendel's work to the current detailed understanding of the precise chemical nature of the genetic material and of how genes function. In general, according to such accounts, the study of the gene proceeded in a reasonably uniform manner wherever it was an object of study.

Newer historical studies of genetic research differ sharply from this picture. They emphasize the importance of conflicting traditions in the scientific literature and challenges to conceptions of the gene that were previously overlooked or dismissed as "dead ends" by historians and scientists. Recent accounts of the era after World War II also emphasize interactions among workers from many disciplines, each tugging genetics in different ways. Much work has gone into understanding the various factors involved in re-forming genetic research.

It is now clear that genetic research moved in different directions in different places. Comparative studies have shown that genetics in one country

[35] An early textbook description of this result is provided by Benjamin Lewin, *Genes IV* (Oxford: Oxford University Press, 1990), pp. 606–7.

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is not, as L. C. Dunn thought it was after World War I, "virtually indistinguishable from genetics in any other country."[36] In general, geneticists in different countries focus on different problems and utilize different concepts and techniques. These differences depend on such factors as the problems investigated and the organisms employed.[37] These are influenced, in turn, by the intellectual roots established by the founders of research traditions, the educational systems that celebrate national accomplishments or promote distinctive styles of investigation,[38] and the long-term commitments of key research institutions. Together, these factors have created different standards of legitimacy for research questions and for answers to those questions.

Among the national traditions whose patterns of research differ distinctly from those of the United States, the example of France is striking.[39] Except in a few practically oriented agronomic institutions, research programs in France were not built on Mendelian concepts and did not rely on standard Mendelian research practices. French biologists were well aware of Mendelian contributions and were not reluctant to exploit new experimental systems. Nonetheless, the dominant research traditions in France undercut acceptance of Mendelism as a major key to understanding heredity. Research traditions that eventually contributed to the development of genetics in France included physiology (from Claude Bernard), causal embryology (linked to Yves Delage and Emmanuel Fauré-Frémiet), and microbiology (begun by Louis Pasteur). These traditions led French biologists to emphasize the importance of understanding the development of the whole organism from a single fertilized egg and the maintenance of harmonious functioning (which Mendelism could not explain) rather than the inheritance of individual traits. They also fostered acceptance of the standards of French positivism, particularly the insistence that theories had to follow behind the step-by-step acquisition of "positive knowledge" of the relevant empirical facts. Consequently, Mendelian genetics barely entered French university curricula until after World War II. At the same time, such French research institutions as the Pasteur Institute fostered long-term commitments to investigations in the dominant research traditions. Against this background, it is not surprising that, when French researchers entered molecular genetics after World War II, they played a leading role in the analysis of gene regulation.

[36] Leslie Clarence Dunn, "The Reminiscences of L. C. Dunn," typescript from the Columbia University Oral History Project (1960), p. 935, distributed by Microfilming Corp. of America, Glen Rock, N.J., 1975.

[37] Adele E. Clarke and Joan H. Fujimura, eds., *The Right Tools for the Job: At Work in 20th Century Life Sciences* (Princeton, N.J.: Princeton University Press, 1991).

[38] See, for example, Harwood, *Styles of Scientific Thought*.

[39] For French genetics and further references, see Richard M. Burian, Jean Gayon, and Doris Zallen, "The Singular Fate of Genetics in the History of French Biology, 1900–1940," *Journal of the History of Biology*, 21 (1988), 357–402; Richard M. Burian and Jean Gayon, "The French School of Genetics: From Physiological and Population Genetics to Regulatory Molecular Genetics," *Annual Review of Genetics*, 33 (1999), 313–49; Sapp, *Beyond the Gene*.

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German genetics also yielded a distinctive national tradition, far more oriented to grand synthetic theories, which provided the touchstone for the elaboration of unique research programs. Research programs established by such figures as Erwin Baur, Carl Correns, Richard Goldschmidt, Valentin Haecker, Alfred Kühn, and Fritz von Wettstein led to an intermediate role for Mendelian genetics in comparison with France and the United States. Most of these founders sought to incorporate Mendelian genetics into an overarching theory of the organism and evolution. Thus, those who taught genetics sought – far more strongly than their U.S. colleagues – to integrate it with embryology and developmental processes. Partly because of this, there was considerable interest in determining the contribution of the cytoplasm to heredity and development. As a result, recognition of cytoplasmic inheritance and the role of the cytoplasm in establishing templates for development first emerged from German laboratories.[40] During that same period, geneticists working in some other countries, such as England and the United States, generally frowned on such investigations.

Many factors other than national traditions have contributed to the remarkable diversity of approaches to genetics. These include the influence of research funding, choice of experimental organisms and investigative tools, and focal problems to investigate.

In order for scientific work to proceed, support is required to pay for equipment, facilities, reagents, salaries, and the like. Recently, historians have argued that patrons – who typically bring their own agendas to scientific work – have pushed genetics in specific directions. There have been many different types of patrons. Among them are universities,[41] government agencies (such as the U.S. Department of Agriculture, National Science Foundation, and National Institutes of Health; the Centre National de la Recherche Scientifique in France;[42] and the Medical Research Council in the United Kingdom), government-supported independent agencies (such as the Kaiser Wilhelm and Max Planck Institutes in Germany), professional associations,[43] foundations[44] (e.g., the Rockefeller Foundation and the Wellcome Trust), private research organizations (e.g., the Pasteur Institute and the Cold Spring Harbor Laboratory), consumer groups for the study of human disorders (such as the National Foundation for Infantile Paralysis and the Huntington Disease Foundation),[45] and private companies (such as breweries, seed companies, pharmaceutical companies, and biotechnology firms). The bonds

[40] Harwood, *Styles of Scientific Thought*; Sapp, *Beyond the Gene.*

[41] For the contributions of just one university, see Lily Kay, *The Molecular Vision of Life: Caltech, The Rockefeller Foundation, and the Rise of the New Biology* (New York: Oxford University Press, 1992).

[42] Jean-François Picard, *La République de Savants: La recherche française et le CNRS* (Paris: Flammarion, 1990).

[43] Kimmelman, "The American Breeder's Association."

[44] Robert E. Kohler, *Partners in Science: Foundations and Natural Scientists, 1900–1945* (Chicago: University of Chicago Press, 1991).

[45] Doris T. Zallen, *Does It Run in the Family?* (New Brunswick, N.J.: Rutgers University Press, 1997).

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between patrons and researchers are strong and have encouraged different approaches to the study of the gene in the recipient research laboratories. In the 1930s and 1940s, the Rockefeller Foundation invested aggressively in research that incorporated the tools of the physical sciences into biology. It provided researchers with powerful investigative tools, promoted the development of a molecular mind-set, and fostered a view of the gene as a discrete molecule whose nature could be determined in isolation from the organism itself. Pharmaceutical and biotechnology patrons tend to treat the gene as a structural unit for producing a protein product.[46] In contrast, much of the support for agricultural research emphasized the genetics of complex features such as milk production, disease and pest resistance, muscle density, and nutritional quality. Many of these traits are quantitative and depend directly on very many (sometimes indeterminately many) genes. Thus, work of this sort favored a view of a gene as just one somewhat indeterminate entity in a complex array.

The type of organism studied also turns out to be crucial.[47] Ever since the rediscovery of Mendel's work, certain organisms have become – and remain to this day – the workhorses of genetic research. The originators of genetics worked mainly with plants. Morgan and his coworkers chose *Drosophila*. Others, such as Leonard Darbishire in the United Kingdom, William Ernest Castle in the United States, and Lucien Cuénot in France, used mammals such as mice.[48] As genetic studies took hold, a variety of other organisms were selected. With each new experimental organism, opportunities arose to pursue some new questions, while others were closed off. Some organisms possessed properties that helped draw genetics in new directions and provided insights that would not have been possible otherwise. To recognize the importance of choice of organisms is to challenge the traditional lockstep linear view of genetic study. For example, certain unicellular organisms with true nuclei, such as the yeast *Saccharomyces cerevisiae* and the green alga *Chlamydomonas reinhardti*, helped reveal the existence of *non*-nuclear genes – genes residing in the mitochondria and the chloroplast – and the roles such genes play in the functioning of the cell and organism. The maize genetics work of Barbara McClintock (1902–1992) revealed the existence of movable genetic elements and opened up the possibility that the genetic material contains dynamic components active in the regulation of the development of the organism.[49] Studies of bacteria permitted the recognition of

[46] Morange, *History of Molecular Biology*; Arthur Kornberg, *Golden Helix: Inside Biotech Ventures* (Sausalito, Calif.: University Science Books, 1995).

[47] Muriel Lederman and Richard M. Burian, eds., "The Right Organism for the Job," a special section of the *Journal of the History of Biology*, 26, no. 2 (Summer 1993), 235–367.

[48] Some workers, including Bateson, Castle, and Morgan, used many organisms, but most specialized on one organism. The specialized knowledge and practices required to profit from a particular organism generally kept individuals from working with multiple organisms.

[49] See Evelyn Fox Keller, *A Feeling for the Organism: The Life and Work of Barbara McClintock* (San Francisco: W. H. Freeman, 1983); Nathaniel Comfort, *The Tangled Field: Barbara McClintock's Search for the Patterns of Genetic Control* (Cambridge, Mass.: Harvard University Press, 2001).

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mechanisms that turn genes off and on, thereby regulating their expression. Butterflies, moths, and snails helped reveal evolutionary effects and the role of the natural environment in enhancing or diminishing gene function as well as the role of the egg cytoplasm, which contains genetic signals from the mother that determine patterns of development but do not correspond with the genetic makeup of the fertilized egg itself.[50]

Thanks to detailed family studies, human geneticists have been able to recognize small variations in phenotype (often connected with a health problem) and to trace the inheritance of those variations in greater detail than was feasible in other organisms. Nonetheless, human genetics developed slowly because human generation times are so long that even "natural experiments" – crosses between individuals with particular traits – are extremely hard to evaluate and because geneticists were practically and morally unable to employ traditional crossing techniques with (or to construct) defined genetic stocks.[51] Thus, until recently, the primary tool of geneticists studying humans was pedigree analysis based on detailed family studies. In some special cases, the availability of extensive pedigree data permitted specific variations in phenotype – often small or health related – to be followed in greater detail than is feasible in other organisms. With the development of tools of biochemical and molecular analysis, DNA could be isolated and studied independently of any requirement for sexual reproduction. Over the last two decades, new technologies have made it possible to trace genetic disorders, previously known only from pedigree studies, to mutations in specific genes. This has been done, for example, for genes leading to sickle-cell anemia, cystic fibrosis, Huntington disease, and breast-cancer susceptibility. As a result, humans have been brought from the periphery to the center of research programs. Since 1990, there has been an international, thirteen-year human genome project to map and sequence all human genes;[52] its findings are leading to significant revisions in our understanding of gene action and the interaction of the genetic material with its molecular and larger-scale environments.

Even here, in spite of international cooperation, national differences remain.[53] In the United States, the emphasis has been on the study of individual genes. In the United Kingdom, where historical connections to ecological

[50] For butterflies, see Doris T. Zallen, "From Butterflies to Blood: Human Genetics in the United Kingdom," in *The Practices of Human Genetics*, ed. Michael Fortun and Everett Mendelsohn (Dordrecht: Kluwer, 1999), pp. 197–216.

[51] Victor A. McKusick, "History of Medical Genetics," in *Emery-Rimoin Principles and Practices of Medical Genetics*, 3rd ed., ed. D. L. Rimoin, J. M. Connor, and R. E. Pyeritz (Edinburgh: Churchill Livingstone, 1996), pp. 1–30. See also Arno G. Moulsky, "Presidential Address: Human and Medical Genetics, Past, Present and Future," in *Human Genetics*, ed. R. Vogel and K. Sperling (Berlin: Springer, 1987), pp. 3–13.

[52] Daniel J. Kevles and Leroy Hood, eds., *The Code of Codes: Scientific and Social Issues in the Human Genome Project* (Cambridge, Mass.: Harvard University Press, 1992); Robert M. Cook-Deegan, *The Gene Wars: Science, Politics and the Human Genome* (New York: Norton, 1994).

[53] Krishna Dronamraju, ed., *History and Development of Human Genetics* (London: World Scientific, 1992).

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genetics are strong, there has been a greater emphasis on complex diseases such as cancer and on the interaction of genes with environmental factors. In France, cytogenetic and immunological studies have predominated, and in Germany, the horrors of the Nazi period have created barriers to human genetic research.

Historians have recently emphasized the strong influence that instruments and techniques used in the laboratory have had in shifting genetic research in new directions. This is especially clear in the early stages of the development of research tools, when instruments are not commercially available and procedures have not yet stabilized. Many of the pioneers who developed new research tools created the instruments themselves and painstakingly perfected the relevant procedures. Other tools of analysis required training in mathematics, chemistry, or physics and were not readily employed by most geneticists. Thus, local zones of expertise, often tied to particular theoretical perspectives, were created. It often took considerable time before such tools became widely dispersed in the genetics community and an even longer time before the results they yielded could be fully reconciled with preexisting perspectives and the results obtained by other techniques.[54]

The combination of differences in research organisms and research tools precipitated a wide variety of research practices. This is reflected in the appearance of many different subdisciplines within the field. Cytogenetics, for example, was founded by researchers who investigated details of the structure of the genetic material while relying on the microscope and associated staining techniques. For these investigators, the genetic material was divided into regions identifiable by bands of greater and lesser staining. For organisms such as sea urchins and humans, with many small chromosomes, these techniques were not sufficient to distinguish one chromosome from another. In contrast, they worked very well for *Drosophila*, lilies,[55] and maize. Until more precise staining procedures, based on gene biochemistry, emerged in the 1980s, individual genes could not be visualized. Meanwhile, investigators who relied on chemical approaches or used chromatography and radioisotope labels coalesced into communities of biochemical and physiological geneticists. Those who applied mathematical and statistical tools to study the features of groups of organisms formed distinct communities of population and quantitative geneticists. These differed over the number of genes in concrete cases, partly because their calculations had different starting points – gene up for population geneticists and phenotype down for quantitative geneticists. As the subdisciplines proliferated, so did the accounts of the gene. Developmental geneticists count genes in terms of units that affect development, cytogeneticists in terms of regions of chromosomes, physiological geneticists in terms

[54] Clarke and Fujimura, *Right Tools for the Job*.

[55] For example, the cytogeneticist John Belling believed he could see discrete genes on the chromosomes of certain lilies. See John Belling, "The Ultimate Chromomeres in Lilium and Aloe with Regard to the Numbers of Genes," *University of California Publications in Botany*, 14 (1928), 307–18.

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of regulatory units that interact with others and with environmental cues to produce stable function, population geneticists in terms of units in the genome with long-term stability, and so on. In this respect, there are many irretrievably distinct criteria for identifying and individuating genes.[56] These disciplinary differences created barriers that distanced researchers from one another, so that, even within a single country, the genetics community often was not homogeneous.

## CONCLUSION

Standard histories of genetics have often served to provide "myths of discovery." This role is worrisome to historians because it often results in misportrayals of the positions taken by pioneers in a discipline and the interpretations they put forward of their experiments and theories. There were always tensions in genetics between those who focused on the functions that genes were supposed to play and those who thought of them as material structures, between those who treated genes as units of calculation and those who believed that Mendelian analysis had discovered fundamental units, delimited as firmly as electrons and nuclei were in physics. The various traditions and disciplines surveyed in this chapter show that the notion of a gene was always open, at least to some extent, reflecting the tension between the approaches taken in different disciplines and contexts. Recent work in genetics has cast doubt on the idea that there is a unique resolution, dictated by scientific findings, of the proper delimitation of genes and gene concepts. We believe that it is important to keep alive the rich history of disagreements over the concept of the gene and its proper application – not only to keep alive some of the issues raised by outstanding scientists through the history of genetics, but also to remind ourselves of the rich field of alternative interpretations of that history, which is in need of continuing analysis, debate, and (re)interpretation. An appreciation of the struggles over the concept of the gene and the interpretation of patterns of inheritance will yield an appreciation of the multiple strands of work and the victories and defeats that went into the formation of current genetics. It will also remind us forcefully of the open-ended character of our current knowledge of genetics.[57]

[56] See Section 3 of Hans-Jörg Rheinberger, "Experimental Complexity in Biology: Some Epistemological and Historical Remarks," *Philosophy of Science*, 64 (suppl.) (1997), S279–S291. See also Sahotra Sarkar, ed., *Foundations of Evolutionary Genetics: A Centenary Reappraisal* (Dordrecht: Kluwer, 1996).

[57] Since submission of this chapter, the literature on the history of the gene and on gene concepts has moved rather rapidly. To assist interested readers, the authors of this chapter have established and will maintain a Web site listing recent bibliography bearing on these topics. The URL is http://www.phil.vt.edu/Burian/GeneConcepts/Bibliography.html. Readers who wish to suggest additions to this bibliography should email them to rmburian@vt.edu or dtzallen@vt.edu.

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# 24

# ECOSYSTEMS

*Pascal Acot*

The word "ecosystem" (from the Greek *oïkos*, meaning "house" or "habitat," and *sustêma*, meaning "set") was coined in 1935 by the British plant ecologist Arthur George Tansley (1871–1955):

> [T]he more fundamental conception is, as it seems to me, the whole system (in the sense of physics), including not only the organism-complex, but also the whole complex of physical factors forming what we call the environment of the biome – the habitat factors in the widest sense. . . . These ecosystems, as we may call them, are of the most various kinds and sizes.[1]

This definition synthesized three main features of scientific ecology during the interwar years: This new branch of biology was to be devoted to the study of the relations between biotic (i.e., plant and animal) communities and their environment, the ontological status of these communities was still debated, and the question of the true nature of their interdependence with purely physical factors, such as solar energy, was gradually coming to the forefront. The fact that Tansley's concept was not even mentioned in J. R. Carpenter's famous ecological glossary (1938), which contains one of the earliest historical surveys of scientific ecology, suggests that this novelty went largely unnoticed before the Second World War.[2] In the 1940s, however, the young North American limnologist Raymond Laurel Lindeman (1916–1942) developed an innovative ecosystems theory close to the presently accepted paradigm.

In 1956, after E. P. and H. T. Odum's works had ushered in the golden age of systems ecology, the American ecologist Francis C. Evans drew attention to three other words supposed to have previously denoted the entity at issue: "microcosm" (1887), "naturkomplekse" (1926), and "holocoen"

[1] Arthur George Tansley, "The Use and Abuse of Vegetational Concepts and Terms," *Ecology*, 16 (1935), 284–307, at p. 299. A "biome" is a biotic community; the term is usually applied to natural communities under the control of similar or identical climates.

[2] See John Richard Carpenter, *An Ecological Glossary* (Norman: University of Oklahoma Press, 1938).



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(1927).[3] In 1969, Jack Major even traced the ecosystem concept back to antiquity;[4] this controversial idea, according to which the word "ecosystem" has a short history while the concept itself would be much older, has been implicitly examined by Frank Benjamin Golley, who nevertheless found its distant roots in S. A. Forbes's (1844–1930) "microcosm" concept.[5] However, as a general rule, these roots may be traced back as far as Alexander von Humboldt's (1769–1859) foundational research on plant geography at the very beginning of the nineteenth century.

General histories of ecology have been written from widely differing perspectives. Donald Worster's pioneering book *Nature's Economy* deals with ecological ideas rather than scientific ecology and advocates an "arcadian" vision of nature as opposed to ecological "imperialism": the former view devoted to "the discovery of intrinsic value and its preservation," the latter to "the creation of an instrumentalized world and its exploitation."[6] At the opposite extreme, a general history of ecological theory has been provided by the professional ecologist Robert P. McIntosh, who stressed the scientific side of the question while playing down its ideological dimension.[7]

Several general histories of scientific ecology have been published in the last two decades, while other studies have focused on particular areas of ecology, on ecology in particular national contexts, or on the link with environmental concerns.[8] This literature reveals that the shaping and development of the ecosystem concept is too recent a field of historical research to have given

[3] Francis C. Evans, "Ecosystem as the Basic Unit in Ecology," *Science*, 123 (1956), 1127–8. Further information on these words and the related concepts of "biosystem" (1939) and "biogeocoenosis" (1942) is given later in this chapter.

[4] Jack Major, "Historical Development of the Ecosystem Concept," in *The Ecosystem Concept in Natural Resource Management*, ed. G. M. Van Dyne (New York: Academic Press, 1969), pp. 9–22.

[5] Frank Benjamin Golley, *A History of the Ecosystem Concept in Ecology: More than the Sum of the Parts* (New Haven, Conn.: Yale University Press, 1993).

[6] Donald Worster, *Nature's Economy* (San Francisco: Sierra Club Books, 1977; new edition, Cambridge: Cambridge University Press, 1984), p. xi.

[7] Robert P. McIntosh, *The Background of Ecology* (Cambridge: Cambridge University Press, 1985). For a more general account, see Peter J. Bowler, *The Fontana/Norton History of the Environmental Sciences* (London: Fontana; New York: Norton, 1992).

[8] There are several important surveys in French. See Pascal Acot, *Histoire de l'écologie*, foreword by Michel Godron (Paris: Presses Universitaires de France, 1988); Pascal Acot, *Histoire de l'écologie* (Paris: Presses Universitaires de France, 1994); Jean-Marc Drouin, *Réinventer la nature, l'écologie et son histoire*, foreword by Michel Serres (Paris: Desclée de Brouwer, 1991); Jean-Paul Deléage, *Histoire de l'écologie, une science de l'homme et de la nature* (Paris: La Découverte, 1991). On specific aspects of ecology, see Sharon E. Kingsland, *Modeling Nature: Episodes in the History of Population Ecology* (Chicago: University of Chicago Press, 1985); Leslie A. Real and James H. Brown, eds., *Foundations of Ecology: Classic Papers with Commentaries* (Chicago: University of Chicago Press, 1991). On American ecology, see Ronald C. Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895–1955* (Berkeley: University of California Press, 1981); Sharon Kingsland, *The Evolution of American Ecology, 1890–2000* (Baltimore: Johns Hopkins University Press, 2005); Chung Lin Kwa, *Mimicking Nature: The Development of Systems Ecology in the United States, 1950–1975* (PhD thesis, University of Amsterdam, 1989). On wider issues, see Gregg Mitman, *The State of Nature, Ecology, Community and American Social Thought, 1900–1950* (Chicago: University of Chicago Press, 1992); Stephen Bocking, *Ecologists and Environmental Politics: A History of Contemporary Ecology* (New Haven, Conn.: Yale University Press, 1997).

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rise to important controversies, the differences of opinion being expressed by choices of different standpoints rather than by clearly focused debates. However, all these studies contain useful information about the ecosystem theory. They also deal with the epistemological issue that underlies the process of its constitution, namely the opposition between the holistic view and a more reductionist perspective. One topic that will emerge in the course of this chapter is precisely the historical development of the debates arising from this crucial question.

## THE STUDY OF PLANT COMMUNITIES

Recognition of the interactions between the various organisms inhabiting an area goes back at least as far as the Swedish naturalist Carl Linneaus. More systematic work, however, began in the early nineteenth century. While traveling in "equinoxial America" between 1799 and 1804, the Prussian scientist Alexander von Humboldt (1869–1859) and his French companion, the marine surgeon and botanist Aimé Bonpland (1773–1858), had attempted to ascend Mount Chimborazo in the Peruvian altiplano. They had observed the parallel strips of vegetation that covered the sides of the mountain up to the limits of perpetual snow. As the vegetation's physiognomy changed with the altitude, Humboldt and Bonpland were brought to the conclusion that the climatic gradient, dependent on altitude, was the main environmental factor governing the phenomenon.

Later, they established a parallel between these observations and the physiognomic changes of vegetation depending on latitude. In both cases, they considered that vegetational landscapes resulted from plant "associations." Humboldt introduced the phrase "geography of plants" to denote the science that considered the relations between vegetation and climate.

From then on, much of the research inspired by the programmatic outlines of Humboldt's *Essai sur la géographie des plantes* (Essay on the Geography of Plants) was focused on plant and animal communities and not on individuals or Linnaean species. However, the studies of the physical factors of the environment were not neglected; as early as 1820, the Genevan botanist Augustin-Pyramus de Candolle (1778–1841) took into consideration factors such as light, the importance of which had been underestimated.

Another landmark in the study of plant communities was the physiognomic conception of the plant formation introduced by the Göttingen botanist August Heinrich Rudolf Grisebach (1813–1879), who defined the phytogeographical formation as a group of plants possessing a definite physiognomical characteristic, such as a meadow, a pine forest, or a tundra.[9]

[9] August Heinrich Rudolf Grisebach, "Über den Einfluss des Climas auf die Begränzung der Natürlichen Floren," *Linnea*, 12 (1838), 160.

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This trend of research was to be followed by many plant geographers, gradually emancipating phytogeography from exhaustive floristic surveys. In 1855, Alphonse de Candolle (1806–1887) emphasized the importance of ancient vegetation in the explanation of present ones and elaborated in 1874 a classification of plant communities on a physiological basis.[10] During the same period, another Genevan botanist, Gaston de Saporta (1823–1895), discovered close analogies between the Tertiary vegetation of Provence, in the south of France, and existing tropical plant associations. The works of the Austrian botanist Anton Joseph Ritter Kerner von Marilaün (1831–1898) on plant communities of the Danube basin also illustrate the general tendency of nineteenth-century plant geographers to study the relationships between vegetation and its environment rather than the geographical distribution of plants. Josias Braun-Blanquet (1883–1980), the founder of the Zürich-Montpellier school of phytosociology, even considered Kerner's research as prefiguring plant sociology.[11]

## THE CONCEPT OF "BIOCOENOSIS"

In addition to studies of plant communities, some late nineteenth-century scientists, mainly zoologists, turned to the interrelationships between the plant world and animal life. Alexander von Humboldt had already drawn the outlines of a "geography of animals" in the *Essai*, and Charles Lyell (1797–1875) had secularized Linnaeus's providential conceptions of the "balance of nature" in research that would nowadays be included within population ecology. This is obvious in the following extracts from Lyell's *Principles of Geology*, where he imagines what might have happened when the first polar bears reached Iceland on drifting icebergs detached from the east Greenland ice barrier:

> The deer, foxes, seals, and even birds, on which these animals sometimes prey, would be soon thinned down. . . . The plants on which the deer fed being less consumed in consequence of the lessened numbers of that herbivorous species, would soon supply more food to several insects, and probably to some terrestrial testacea, so that the latter would gain ground. . . . The diminution of the seals would afford a respite to some fish which they had persecuted; and these fish, in their turn, would then multiply and press upon their particular prey. . . . Thus the numerical proportions of a great number of inhabitants,

[10] Alphonse de Candolle, *Géographie botanique raisonnée*, 2 vols. (Paris: Masson, 1855); Alphonse de Candolle, *Constitution dans le règne végétal de groupes physiologiques applicables à la géographie botanique ancienne et moderne* (Geneva: Archives des Sciences de la Bibliothèque Universelle, 1874).

[11] See Anton Joseph Ritter Kerner von Marilaün, *Das Pflanzenleben der Donauländer* (Innsbrück: Vierhapper, 1863), English trans. H. S. Conard, *The Background of Plant Ecology: The Plants of the Danube Basin* (Ames: Iowa State College Press, 1951). On the Zürich-Montpellier school of phytosociology, see Malcolm Nicholson, "National Styles, Divergent Classifications: A Comparative Case Study from the History of French and American Plant Ecology," *Knowledge and Society*, 8 (1989), 139–86.

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> both of the land and sea, might be permanently altered by the settling of one new species in the region; and the changes caused indirectly might ramify through all classes of the living creation, and be almost endless.[12]

The famous "cats to clover" chain described in the third chapter of Darwin's *On the Origin of Species* is scientifically related to these descriptions of the trophic links that structure plant and animal communities: "[I]t is quite credible that the presence of a feline animal in large numbers in a district might determine, through the intervention first of mice and then of bees, the frequency of certain flowers in that district!"[13]

A few years later, in 1877, this process of development matured when the German zoologist Karl August Möbius (1825–1908), professor of zoology at Kiel University, coined the word "biocoenosis" (from the Greek *bios*, meaning "life," and *koinos*, meaning "in common"). Möbius had studied the fauna of the Gulf of Kiel and published the first volume of his *Fauna der Kieler Bucht* (Fauna of Kiel Bay) in 1865. Topography and variations of depth, as well as animal and plant life, were studied, leading Möbius to propose the concept of "life community" (*Lebensgemeinschaft*).

The actual term "biocoenosis" was introduced later. In 1869, the depletion of the Schlesvig-Holstein oyster beds had worried the Prussian government, and Möbius had been commissioned to inquire about the possibility of developing oyster culture along the coasts. His final report, *Die Auster und die Austernwirtshaft* (The Oyster and Oyster-Culture) was completed after long-term research (he had to study the French and British experiences). Möbius concluded that overexploitation of oyster beds resulted from the development of railways, which made possible a considerable extension of the market. Because he had to take into account all the factors that make up the environments of oyster beds, he was led to elaborate the concept of biocoenosis:

> Science possesses, as yet, no word by which . . . a community of living beings may be designated; no word for a community where the sum of species and individuals, being mutually limited and selected under the average external conditions of life, have, by means of transmission, continued in possession of a certain definite territory. I propose the word *biocoenosis* for such a community.[14]

Ten years later, in 1887, the North American zoologist Stephen Alfred Forbes used a similar concept – "microcosm" – in order to embrace all living beings that form what we nowadays call a "biotic community": "A lake . . . forms a little world within itself – a microcosm within which all the elemental forces are at work and the play of life goes on in full, but on so small a scale

[12] Charles Lyell, *Principles of Geology*, 3 vols. (London: John Murray, 1830, 1832, 1833), vol. 2, p. 144.
[13] Charles Darwin, *On the Origin of Species* (London: John Murray, 1859), p. 74.
[14] Karl Möbius, *Die Auster und die Austernwirtshaft* (Berlin: Verlag von Wiegandt, Hempel und Parey, 1877), English trans. "The Oyster and Oyster-Culture," *Report of the U.S. Commission of Fish and Fisheries* (Washington, D.C.: U.S. Government Printing Office, 1883), p. 723.

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as to bring it easily within the mental grasp."[15] The development of the community concept was important because it initiated a process in which, over several decades, the traditional distinction between living organisms and a biotic environment would be radically questioned.

## THE INTEGRATION OF PHYSICAL FACTORS

Agrochemistry was developing in the 1840s in response to pressing social demands. This took place within the transdisciplinary field of study that would later become scientific ecology. At this stage, the fine mechanisms of plant life still needed to be understood. The German baron Justus von Liebig (1803–1873) discovered the "minimum law" according to which the growth of a plant depends on the nutritive element available to it in minimum quantity in the same way that the solidity of a chain depends on its weakest link. This law was extended in 1905 by the English botanist Frederick Frost Blackman (1866–1947), who emphasized the limiting effects of the maximum, too.[16] Contemporaneously with Liebig, the French chemist and engineer of the Ecole des Mines Jean-Baptiste Boussingault (1802–1887) – a former lieutenant-colonel of the South American revolutionary and statesman Simón Bolívar (1783–1830) – studied plant nitrogen absorption, thus playing a major part in research on plant "autotrophy," the complex process by which plants assimilate inorganic elements such as nitrogen or atmospheric carbon.

## THE FIRST QUALITATIVE OUTLINE OF AN ECOLOGICAL SYSTEM

It is to a Swiss naturalist, François-Alphonse Forel (1841–1912), that we are indebted for the first exhaustive description of an ecological system. The fact that Stephen Forbes had previously used the expression "system of aquatic animal life" in *The Lake as a Microcosm* has often been brought to historians' attention. But Forbes's classic essay was essentially devoted to the study of biotic communities and failed convincingly to handle the relationships of the latter with the physical factors of their environments – a point that undermines the claim that he should be treated as the founder of the ecosystem concept.

Forel was born at Morges, on the shore of Lac Léman (Lake Geneva), ten miles west of the city of Lausanne. He had devoted his whole life to studying

[15] Stephen Alfred Forbes, "The Lake as a Microcosm," *Illinois Natural History Survey Bulletin*, 15 (1925), 537 (first read in 1887 before the Scientific Association of Peoria and published the same year in its *Bulletin*). See also Stephen Alfred Forbes, *Ecological Investigations of Stephen Alfred Forbes* (New York: Arno Press, 1977).

[16] Frederick Frost Blackman, "Optima and Limiting Factors," *Annals of Botany*, 19 (1905), 281–98.

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the lake, thus founding and christening the science of "limnology" as "the oceanography of lakes." Forel, professor of general anatomy and physiology at the faculty of the Académie de Lausanne but also zoologist, geologist, and archaeologist, had, in particular, discovered the benthic fauna of the lake. He invented many measuring instruments, including the "Forel xanthometer" – used to discern the shades of lake waters – and the "limnograph," a device to measure the "seiches" (the oscillations of the lake level).

In the eleventh part of his book on Lac Léman, the seventh chapter was entitled "The Circulation of the Organic Matter." This contained an exhaustive description of those elements and functions that constitute what we now call an "ecosystem." Here we can recognize some features of the scientific achievements that have already been outlined, including autotrophy, food chains, and plant and animal communities:

> The lacustrine plants feed on mineral nutrients dissolved in surrounding waters. . . . The animals of the lake assimilate directly some of the dissolved elements and transform them into the structure of a living organism. . . . But the greatest part of their feeding comes from plant organisms. . . . An algae, for example, is eaten by a diatomea, which is eaten by a rotator which is eaten by a copepoda, which is eaten by cladocera which is eaten by a féra, which is eaten by a pike which is eaten by an otter or by a man.

A most important characteristic of Forel's thought lies in his recognition that the food chains intertwine and form a loop structure that makes possible a partial recycling of organic matter:

> [W]hile small and large organisms which devour each other in the lake waters make the living matter through more and more complex and higher successive incarnations, microbes represent the reverse function. . . . The function of the microbian agents of putrefaction closes the transmutations cycle of organic matter, by making it back to its primitive form or starting point.[17]

However, in spite of the exhaustive nature of the description, further developments within scientific ecology during the first half of the twentieth century suggest that Forel cannot be considered as the founder of the ecosystem theory.

## FROM PLANT SUCCESSIONS TO ORGANICISM IN ECOLOGY

Henry Chandler Cowles (1869–1939) was an important pioneer of American ecology. He studied the dunes on the shores of Lake Michigan and their

[17] François-Alphonse Forel, *Le Léman, Monographie Limnologique*, 3 vols. (Lausanne: F. Rouge, 1892–1901; Geneva: Slatkine Reprints, 1969), vol. 3, pp. 364, 367 (translated by Pascal Acot).

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vegetation. Until then, ecologists had tried to analyze static situations, but because dunes are very unstable topographical forms, their vegetation is subject to rapid transformation. This is presumably one of the reasons why Cowles undertook to consider "successional" vegetation movements: "There must be . . . an order of succession of plant societies just as there is a succession of topographic forms in the changing landscape. As the years pass by, one plant society must necessarily be supplanted by another, though the one passes into the other by imperceptible gradations."[18] At the ultimate stage, he believed, the process reaches a dynamic equilibrium called the "climax" (from the Greek *klimaktêrikos*, meaning "which progress by rungs").

The concepts of association, competition, migration, and "ecesis" – the latter being the settlement of a community – rapidly became central in ecology. It was this trend of research that inspired Frederic Edward Clements's (1874–1945) organismic conceptions of plant communities:

> Vegetation exhibits certain phenomena which are characteristic manifestations of the forces which lie at its foundation. Such phenomena are peculiar to it, and are entirely distinct from those primary activities of the individual that are termed functions. This conception will be clearer if we consider vegetation as an entity, the changes and structures of which are in accord with certain basal principles in much the same fashion that the functions and structures of plants correspond to definite laws.[19]

The idea of a correspondence between the organization of the individual as a being whose parts form a definite unity and the organization of a community, the parts of which form a whole, dates back to antiquity. Plato's philosophy may be held as organicist on the basis of its classical set of correspondences between psychology, epistemology, sociology, and politics. Indeed, the whole history of philosophy is marked out with such attempts to *organize* communal entities starting from the assumption of an underlying unity. The evolutionary thinker Herbert Spencer (1820–1903), who had maintained the idea of a real analogy between individual and "social" organisms,[20] may have been the link between philosophical organicism and Clements's effort to conceptualize plant formations (Cowles's "plant societies") as definite entities. In Clements's view: "The plant formation is an organic unit. It exhibits activities or changes which result in development, structure, and reproduction. . . . According to this point of view, the formation is a complex

[18] Henry Chandler Cowles, "The Physiographic Ecology of Chicago and Vicinity; a Study of the Origin, Development, and Classification of Plant-Societies," *Botanical Gazette*, 31, no. 2 (1901), 73–108, 145–82, at p. 79.

[19] Frederic Edward Clements, "The Development and Structure of Vegetation," *Nebraska Botanical Survey* (1904), 5–31, at p. 5. In August 1901, this paper had been read in Denver before the Botanical Society of America.

[20] On social thought in nineteenth-century biology, see Peter J. Bowler, *Biology and Social Thought: 1850–1914* (Berkeley: Office for History of Science and Technology, University of California, 1993).

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organism, which possesses functions and structure, and passes through a cycle of development similar to that of the plant."[21]

## THIRTY YEARS OF CONTROVERSIES

Arthur Tansley almost immediately began a thirty years' war against Clements's organicist viewpoint. Besides his botany professorship at Oxford University, Tansley was concerned with a wide range of subjects, including nature conservancy, psychology, psychoanalysis, and philosophy – the latter possibly explaining his interest in close epistemological discussion. By elaborating the ecosystem concept, he suggested a solution to the crisis initiated by Clements's effort to treat the community as though it were an individual organism.

As early as 1905, Tansley argued in a review of Clements's *Research Methods in Ecology* that because the functions of a plant formation (i.e., the functions of association, invasion, and succession) are not present at the climax, they must be considered as processes rather than functions. Hence, the term "quasi-organism" is preferable. However, organicist ideology was still rampant within the prevailing, rather ambiguous, conceptual framework. Clements reiterated his views at the very beginning of his classic *Plant Succession* of 1916: "As an organism, the formation arises, grows, matures and dies."[22]

Organicism gained ground among such biocoenologists as Victor Elmer Shelford (1877–1968): "Ecology is that branch of general physiology which deals with the organism as a whole, with its general life processes as distinguished with the more special physiology of organs."[23] This background led Tansley to qualify his thought in 1920: "It does not follow, because vegetation units may be usefully treated as organic entities, that they are organisms. . . . On the other hand, it does not follow, because such deductions are inadmissible that the comparison with organisms is valueless."[24]

The crisis was reached in 1934–5 with the publication in the *Journal of Ecology* of a paper entitled: "Succession, Development, the Climax, and the Complex Organism: An Analysis of Concepts."[25] The author, John Phillips, had carried out ecological studies, both applied and theoretical, in Central, East, and South Africa. In the third part of his paper, he maintained that

[21] Frederic Edward Clements, *Research Methods in Ecology* (Lincoln, Neb.: The University Publishing Co., 1905), p. 199.

[22] Frederic Edward Clements, *Plant Succession: An Analysis of the Development of Vegetation* (Washington, D.C.: Carnegie Institution, 1916), p. 3.

[23] Victor Elmer Shelford, "Principles and Problems of Ecology as Illustrated by Animals," *Journal of Ecology*, 3, no. 1 (1915), 1–23, at p. 2.

[24] Arthur George Tansley, "The Classification of Vegetation and the Concept of Development," *Journal of Ecology*, 8, no. 2 (1920), 118–44, at p. 122.

[25] John F. V. Phillips, "Succession, Development, the Climax, and the Complex Organism: An Analysis of Concepts," *Journal of Ecology*, 22 (1934), 554–71; 23 (1935), 210–46, 488–508.

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nature reveals an intrinsic tendency to constitute "wholes" under the influence of a rather obscure factor called "holism." This term had been introduced in 1926 by the general and future prime minister of South Africa Jan Christian Smuts (1870–1950): "Holism is the term here coined (from *holos* = whole) to designate this whole-ward tendency in Nature, this fundamental factor operative towards the making or creation of wholes in the universe."[26] The holistic doctrine is a perfect opposite to reductionism and therefore appears as a kind of metaphysical foundation of organicism in ecology: Plant societies represent more than the individual plants that form them *because* holism makes the whole more than the sum of its parts.

Tansley did not hide why he particularly disliked this sort of reasoning:

> Phillips' articles remind one irresistibly of the exposition of a creed – of a closed system of religious or philosophical dogma. Clements appears as the major prophet and Phillips as the chief apostle, with the true apostolic fervour in abundant measure. Happily . . . the heresiarchs, and even the infidels, are treated with perfect courtesy. But while the survey is very complete and almost every conceivable shade of opinion which is or might be held is considered, there is a remarkable lack of any sustained criticism of opponents' arguments.[27]

He had then to overcome a real difficulty: to depose the organicist ideology in ecology while keeping the heuristic value of "biotic communities" regarded as definite and structured entities. In order to understand both the succession process and its culmination at the climax, Tansley was therefore led to integrate biotic and physical factors within a new entity: "In an ecosystem the organisms and the inorganic factors alike are components which are in relatively stable dynamic equilibrium." Thus, by introducing the ecosystem concept in scientific ecology, Tansley emancipated this field of research from the metaphysical burden he had already harshly criticized and, at the same time, provided a conceptual means by which communities could be treated as relatively isolated units.

As is often the case in the development of science, the new concept had already been described by others working within the same context. Apart from the early use of the term "microcosm," it had been called "naturcomplex" in 1926 and "holocoen" in 1927. Later, the words "biosystem" and "biogeocoenosis" were proposed respectively by the German limnologist August Thienemann (1882–1960) and the Soviet ecologist V. N. Sukatchev.[28]

[26] Jan Christian Smuts, *Holism and Evolution* (London: MacMillan, 1926), p. 100.
[27] Tansley, "Use and Abuse of Vegetational Concepts and Terms," pp. 285, 306.
[28] See E. Markus, "Naturcomplekse," *Sitzungsberichte der Naturforscher-Gesellschaft bei der Universität Tartu*, 32 (1926), 79–94; K. Friederichs, "Grundzätzliches Über die lebenseinheiten hörerer Ordnung und der ökologischer Einheitsfaktor," *Die Naturwissenschaften*, 15 (1927), 153–7, 182–6; A. Thienemann, "Grundzüge einen allgemeinen Oekologie," *Archiv für Hydrobiologie*, 35 (1939), 267–85; V. N. Sukatchev, "On the Principles of Genetic Classification in Biocoenology" (in Russian), *Zhurnal Obscej Biologii, Akademija Nauk SSSR*, 5 (1944), 213–27, translated and condensed by F. Raney; R. Daubenmire, *Ecology*, 39 (1958), 364–7.

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The common theme of these concepts was the attempt to understand the mechanisms of biocoenotical equilibria and, correlatively, to elucidate the relations between the organic and inorganic components of ecological systems. These questions stand at the core of the important trend of research that, gradually incorporating ecosystem energetics, studied trophic structures and population dynamics and gave birth several years later to an ecosystem theory based on new foundations.

## POPULATION DYNAMICS

At the start of the twentieth century, little was known of the mechanisms governing the fluctuation of populations. The concept of the pyramidal population structure of a biocoenosis is the work of the German zoologist Karl Semper (1832–1893).[29] This was further developed by the British zoologist Charles Elton (1900–1991), which is why it is often called the "Eltonian pyramid." Elton also gave the concept of "ecological niche" a functional definition rather than a spatial one.[30]

The young Belgian mathematician Pierre-François Verhulst (1804–1849), a disciple of the statistician Adolphe-Lambert Jacques Quetelet (1796–1874), had previously elaborated the famous "logistic" equation that describes the growth of a population – tendencially exponential but gradually slowed down by saturation of the environment.[31] However, the logistic curve had gone almost unnoticed until it was rediscovered in 1920 by the North American zoologist Raymond Pearl (1879–1940) and his colleague Lowell J. Reed, the former acknowledging, a year later, Verhulst's priority.

The American physicist Alfred James Lotka (1880–1949) proposed in 1925 a differential equation system to calculate periodical fluctuations of two species, where one is the predator of the other.[32] This work was to be developed by the great Italian mathematician Vito Volterra (1860–1940) following a precise social demand concerning fishing in the Adriatic Sea after the relative interruption of the First World War.[33] This research was subject to a great number of experimental verifications – most of them being carried out by the Russian biologist Georgii Frantsevitch Gause (1910–1986). They contributed to the

[29] Karl Semper, *Die natürlichen Existenzbedingungen der Thiere* (Leipzig: A. Brockhaus, 1880), English trans., *Animal life as Affected by the Natural Conditions of Existence* (New York: Appleton, 1881).

[30] On Charles Elton, from an institutional point of view, see Peter Crowcroft, *Elton's Ecologists: A History of the Bureau of Animal Population* (Chicago: University of Chicago Press, 1991).

[31] On Verhulst's works, see G. E. Hutchinson, *An Introduction to Population Ecology* (New Haven, Conn.: Yale University Press, 1978), chap. 1.

[32] See Kingsland, *Modeling Nature*; Alfred James Lotka, *Elements of Physical Biology* (Baltimore: Williams and Wilkins, 1925), revised and enlarged edition published as *Elements of Mathematical Biology* (New York: Dover, 1956).

[33] Vito Volterra, "Variazioni e fluttuazioni del numero d'individui in specie animali conviventi," *Atti delle Academia nazionale dei Lincei, Memorie*, 6, no. 2 (1926), 31–113; Vito Volterra, "Population Growth, Equilibria and Extinction under Specified Breeding Conditions: A Development and Extension of the Theory of the Logistic Curve," *Human Biology*, 10 (1938), 1–11.

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enrichment of knowledge concerning the trophic side of the biocoenosis[34] – the latter from then on conceived as being structured by the whole set of its ecological niches.

## THE TROPHIC-DYNAMIC ASPECT OF ECOSYSTEMS

There remained the problem of understanding how the ecosystems function: Why – having reached the climax – do they last indefinitely if undisturbed? In 1940, the North American limnologist Chancey Juday (1871–1944) put forward the fundamental function of solar energy in primary plant production using the same unit – the calorie – to measure the quantity of energy received by the lake during the year and the energetic equivalence of the organic matter produced during the same period. This explained also the theoretically perennial nature of such ecological systems as long as they remained undisturbed. The rather unusual title and content of Juday's paper – "Annual Energy Budget of an Inland Lake"[35] – called to mind an industrial plant, with its production depending both on raw materials and energy consumption. This language of community economics clearly took ecosystem theory away from organicism and brought it closer to the approaches that were widely developed by physicists after the Second World War.

In 1941, another American limnologist, Raymond Laurel Lindeman (1916–1942), a former student of the British-trained ecologist George Evelyn Hutchinson (1903–1991), published an article "in partial fulfillment of the requirements for the degree of Doctor of Philosophy" containing an exhaustive description of the Cedar Bog Lake (Minnesota) ecosystem:

> *Dissolved nutrients*, autochthonous and allochthonous, are incorporated into organic substances by producers, of which three types are common in lakes: *autotrophic bacteria*, *algae* and *pondweeds*. Each of these may die and decompose by bacterial action into ooze, or may be eaten by some consumer. *Zooplankters* feed as primary consumers upon phytoplankton algae, bacteria and particulate organic matter; they in turn may be eaten by secondary consumers, such as *plankton predators* and small *swimming predators*, or they may die and contribute to the benthic ooze.[36]

The description continued, presenting all features that define an ecosystem – autotrophy, the relationships of consumers' trophic habits to community structure, and the recycling of organic matter by microorganisms:

[34] G. F. Gause, "Experimental Analysis of Vito Volterra's Mathematical Theory of the Struggle for Existence," *Science*, 79, no. 2036 (1934), 16–17. Just before he dedicated himself to the study of medieval science, Alistair Crombie (1915–1996) repeated Gause's experiments, thus contributing to the elucidation of the problems at issue. See Alistair Cameron Crombie, "Interspecific Competition," *Journal of Animal Ecology*, 16, no. 1 (1947), 44–73.
[35] Chancey Juday, "Annual Energy Budget of an Inland Lake," *Ecology*, 21, no. 4 (1940), 439–50.
[36] Raymond L. Lindeman, "Seasonal Food-Cycle Dynamics in a Senescent Lake," *The American Midland Naturalist*, 26 (1941), 636–73, at pp. 637–8.

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"The substance of each group upon death of the organisms contributes to the benthic ooze, from which plant nutritives are again dissolved." The energy budgets were also worked out, after conversion of weight values to calorific values.

The following year, Lindeman generalized his analysis to all ecosystems, thus renewing the theory and, according to most of the historians of scientific ecology, actually founding it: "Analyses of food-cycle relationships indicate that a biotic community cannot be clearly differentiated from its abiotic environment; the *ecosystem* is hence regarded as the more fundamental ecological unit."[37] From then on, ecosystems of any kind – land, sea, or lake – were to be held as structures along which exchanges of matter and energy take place: "The basic process in trophic dynamics is the transfer of energy from one part of the ecosystem to another."[38]

Lindeman's foundational paper was initially rejected by two distinguished referees, the American limnologists Paul Welch and Chancey Juday (1871–1944), on the basis that it was too theoretical. They were convinced, like Forel, that lakes were "individuals" and hence that Lindeman's generalizations were at least premature. They also thought that theoretical papers were not appropriate for the review *Ecology*. The fourth draft was finally accepted, after George Evelyn Hutchinson, who had independently obtained some of Lindeman's findings, had strongly recommended its publication to Thomas Park, editor of *Ecology*. Raymond Lindeman died when the paper had gone to press, and Hutchinson had the sad duty to complement it with an obituary notice: "[I]t is to the present paper that we must turn as the major contribution of one of the most creative and generous minds yet to devote itself to ecological science."

## ODUM'S FUNDAMENTALS OF ECOLOGY

In 1944, the Austrian physicist Erwin Schrödinger (1887–1961) published a small book describing living beings from a thermodynamical standpoint. He had observed that they seem not to be governed by the second law of thermodynamics, a consequence of which being that a nonliving system reaches a permanent state in which one cannot observe any movement when isolated or placed under uniform conditions. This state is called the "thermodynamic equilibrium state" or state of "maximum entropy." On the contrary, living beings possess the marvelous property of being able to delay by a metabolic process the moment when they reach their maximal entropy, which is the moment of their death.[39]

[37] R. L. Lindeman, "The Trophic-Dynamic Aspect of Ecology," *Ecology*, 23 (1942), 399–418, at pp. 415.
[38] Ibid., p. 400.
[39] See E. Schrödinger, *What Is Life? The Physical Aspects of the Living Cell* (Cambridge: Cambridge University Press, 1944).

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These ideas were shared by Hutchinson and inspired two of his former American students, the brothers Eugene P. (b. 1913) and Howard Tresor Odum (b. 1924). Both were trained as ornithologists, but Howard T. Odum soon turned to biochemistry and radioecology (the subject of his PhD, directed by Hutchinson, was the biogeochemical cycle of strontium). Both later dedicated themselves to radioecology (a term coined by Howard T. Odum). The two brothers had a deep influence on scientific ecology, in particular through the models they constructed, setting up analogies between circulation of energy and matter in ecosystems and flow of electricity in circuits[40] or of resources in an economy.

They established in their famous textbook *Fundamentals of Ecology* that ecosystems behave, in thermodynamic terms, like living organisms:

> Organisms, ecosystems and the entire biosphere possess the essential thermodynamic characteristic of being able to create and maintain a high state of internal order, or a condition of low entropy (a measure of disorder or the amount of unavailable energy in a system). Low entropy is achieved by a continual dissipation of energy of high utility (light or food, for example) to energy of low utility (heat, for example). In the ecosystem, "order" in terms of a complex biomass structure is maintained by the total community respiration which continually "pumps out disorder."[41]

These conceptions were elaborated within a rather complex epistemological framework. On the one hand, the ecosystem was grasped as an intermediary level between entities that fit together, all of them possessing characteristic features of living beings: At the first level, one finds organisms; at the second level – which integrates the organisms of the lower level – one finds ecosystems; and at the third level is the entire biosphere. This is clearly a holistic conception, according to which emergent properties arise at the upper integration levels. Eugene and Howard Odum claimed to be "holists" in this nonmetaphysical sense. But, on the other hand, the ecosystem was also understood as a thermodynamical *machine* able to maintain itself at a state of oscillatory equilibrium around the climax. This point of view was reductionist and somewhat opposed to the former.

## FROM ECOSYSTEMS TO GLOBAL ECOLOGY

Hence, the most fundamental concept of modern ecology has been marked, from its very origin, by the classical epistemological tension between holism and reductionism. It is even legitimate to wonder if the reemergence of this

[40] See H. T. Odum, "Ecological Potential and Analogue Circuits for the Ecosystem," *American Scientist*, 48 (1960), 1–8.

[41] E. P. Odum and H. T. Odum, *Fundamentals of Ecology*, 3rd ed. (Philadelphia: Saunders, 1971), p. 37.

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contradiction is not periodical in ecology, as suggested by the actual debates regarding "global ecology."

The word "biosphere," as denoting the terrestrial zone containing life, was coined by the Viennese geologist Eduard Suess (1831–1914) in a small book dealing with the origins of the Alps; however, "biosphere" is generally associated with the Soviet mineralogist Vladimir Ivanovitch Vernadsky (1863–1945), who used it in his major book, *Biosfera*, so as to conceptualize his holistic point of view regarding life on earth.[42]

In the 1930s, while teaching at Yale University, Hutchinson began his studies of biogeochemical cycles. He was familiar with Vernadsky's ideas all the more because Vernadsky's son, George Vernadsky, was a Yale professor of history, and because he was a colleague of the Russian arachnologist Alexander Petrunkevitch, who had been a student of V. I. Vernadsky at Moscow University. Regarding the latter, G. E. Hutchinson wrote in his autobiography: "I did my best to help Petrunkevitch and George Vernadsky make his ideas about the biosphere better known in English-speaking countries."[43]

Having studied in particular the effects of human activities on carbon and phosphorus cycles and having moreover inspired Lindeman's and Odum's research on trophic and biogeochemical cycles, Hutchinson is consequently not only "the missing link between Vernadsky's work and ecology"[44] but also the common denominator to Vernadsky's biospheral conceptions, systems ecology, and today's "global change" ecology:

> Apart from a slight rise in agricultural productivity caused by an increase in the amount of carbon dioxide in the atmosphere, it is difficult to see how the various contaminants with which we are polluting the atmosphere could form the basis for a revolutionary step forward. Nonetheless, it is worth noting that when the eucaryotic cell evolved in the middle Precambrian period, the process very likely involved an unprecedented new kind of evolutionary development. Presumably, if we want to continue living in the biosphere we must also introduce unprecedented processes.[45]

Indeed, the ecosystem concept has become, at the start of the twenty-first century, the basic unit used for modeling the potential changes we may expect in earth's global ecology. And it is significant that the epistemological background of this new era of scientific ecology is still related to the old conflict between holism and reductionism. A holistic approach would, for example, consider the atmosphere as a global "circulatory system" of the biosphere,

[42] See Eduard Suess, *Die Entstehung der Alpen* (Vienna: W. Braumüller, 1875); V. I. Vernadsky, *Biosfera* (Leningrad: Nauchnoe Khimikoteknicheskoe Izdatelstvo, 1926).

[43] G. E. Hutchinson, *The Kindly Fruits of the Earth: Recollections of an Embryo Ecologist* (New Haven, Conn.: Yale University Press, 1979), p. 233.

[44] Nicholas Polunin and Jacques Grinevald, "Vernadsky and Biospheral Ecology," *Environmental Conservation*, 15, no. 2 (1988), 119.

[45] G. E. Hutchinson, "The Biosphere," *Scientific American*, 223 (1970), 45–54, at p. 53.

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while a reductionist method would proceed by gradually integrating the ecological characteristics of local systems.

In the future, these approaches will presumably become more and more complementary as in both cases the current paradigm remains the same: the earth's thin film of living matter being grasped as a patchwork of ecological systems linked up and sustained by grand biogeochemical cycles.

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# 25

# IMMUNOLOGY

*Thomas Söderqvist, Craig Stillwell, and Mark Jackson*

## IMMUNOLOGY

"Immunity," taken broadly, refers to a cluster of natural phenomena observed first in the field, then in the clinic, and finally in the laboratory. It had been known since antiquity that injections of small doses of poison could prevent unexpected larger doses from causing harm (preventive immunity), that there were some diseases that never afflicted a person more than once (acquired immunity), and that certain individuals were more disposed than others to stay free from infectious diseases (natural immunity). Although it is customary to credit the British physician Edward Jenner with the invention of the first effective preventive procedure against smallpox (later known as vaccination), inhalation or inoculation of powdered scabs from smallpox lesions seems to have been part of ethnomedical practice long before then and was even practiced by the European gentry throughout most of the eighteenth century. Jenner's technique – inoculating cowpox matter to prevent smallpox – was first published in 1798 and won rapid acceptance, probably because his methodical investigation suited an age permeated by Enlightenment optimism toward science. Even so, the next advance in understanding immunity came nearly a century later within the context of the new germ theory of disease.[1]

In the first three sections of this chapter, we focus on the history of the concept of immunity and the emergence of the science of immunology as it relates to the laboratory at the expense of the field (epidemiology and public health) and clinic (preventive and therapeutic treatment) up to the 1970s.

[1] Genevieve Miller, *The Adoption of Inoculation for Smallpox in England and France* (Philadelphia: University of Pennsylvania Press, 1957); Arthur M. Silverstein and Genevieve Miller, "The Royal Experiment on Immunity," in Arthur M. Silverstein, *A History of Immunology* (San Diego, Calif.: Academic Press, 1989), pp. 24–37.



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The fourth section is a review of the historiography of immunology in the twentieth century, especially in the last three decades.

## IMMUNITY AS A SCIENTIFIC OBJECT

After a long career as a chemist and a pioneer in the investigation of microbes and their relationship to fermentation, generation, and disease, Louis Pasteur announced in 1880 that he had developed a method to prevent chicken cholera. Weakened microbes would, when inoculated into chickens, confer immunity from later inoculations of virulent microbes. Within a few years, Pasteur and his colleagues (among whom Emile Roux deserves more credit than he has traditionally received) applied the principle of attenuation (weakening virulence via oxidation) to develop a vaccine for anthrax, a microbial disease of sheep and cattle. In 1886, Pasteur dramatically announced that his laboratory had developed a therapeutic rabies vaccine that had already saved the lives of two boys badly bitten by rabid dogs. The intensive fund-raising for, and rapid construction of, the Institut Pasteur in Paris, which opened in 1888, reflects Pasteur's remarkable success at forming coalitions with other important groups in French society (e.g., farmers, veterinarians, physicians, hygienists) and indicates how science would now play a key role in medicine and public health. The growing use of the word "immunity" ("immunité" in French, "immunität" in German) in the medical and scientific literature following Pasteur's discoveries indicates that a number of bacteriologists, pathologists, hygienists, and zoologists were thinking of themselves as dealing with a general, natural phenomenon.[2]

In 1888, Roux and Alexandre Yersin isolated the microbial toxin responsible for the suffocating symptoms of diphtheria, a disease that plagued children in almost every overcrowded and unsanitary European metropolis. Two years later, Emil Behring and Shibasaburo Kitasato, working in bacteriologist Robert Koch's Berlin laboratory, discovered that animals given low doses of diphtheria (or tetanus) toxin became "immune" to larger doses. The serums of these immune animals possessed a "property" that specifically neutralized the toxin. By late 1891, Behring had used antitoxic serum from such an immunized animal to save the life of a ten-year-old girl dying of diphtheria. Behring's contributions in this new field of "serotherapy" led to his being awarded the first Nobel Prize in Physiology or Medicine in 1901. Roux developed techniques to mass-produce antitoxic serums – called antiserums – in horses. Institutions such as the Institut Pasteur and the Lister Institute in London (1894) quickly began to produce and market antiserums;

[2] Patrice Debré, *Louis Pasteur* (Baltimore: Johns Hopkins University Press, 1998); Gerald L. Geison, *The Private Science of Louis Pasteur* (Princeton, N.J.: Princeton University Press, 1995); Bruno Latour, *The Pasteurization of France* [English translation of *Les microbes: Guerre et Paix* (1984)], trans. Alan Sheridan and John Law (Cambridge, Mass.: Harvard University Press, 1988).

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others were constructed specifically for that purpose, such as the Institut für Serumprüfung und Serumforschung in Berlin (1894), the commercially run Wellcome Laboratory in London (1894), and the Statens Seruminstitut in Copenhagen (1901). Although serotherapy promised to arm physicians with a powerful weapon against infectious diseases, it failed to live up to expectations except for the cases of diphtheria and tetanus poisoning.[3]

Humors in the blood, however, were not the only way to explain the phenomenon of immunity to infectious diseases. In 1883, Russian comparative zoologist Ilya Metchnikoff observed under the microscope the active gathering of mobile amoeboid cells around a thorn thrust into a transparent starfish larva; he interpreted this activity as a protective response and, after further experiments and observations, proposed the phagocytic theory of inflammation. But because the prevailing medical view regarded inflammation as a harmful event, his theory was sharply criticized by pathologists as too teleological or vitalistic. Undaunted, Metchnikoff extended his principle of phagocytic protection, claiming that immunity to infectious diseases was largely the result of the ability of phagocytes to engulf and destroy invading microbial pathogens.[4]

Finding the political atmosphere of czarist Russia inimical to his studies, Metchnikoff accepted an offer of a laboratory of his own in the newly opened Institut Pasteur, where he worked from 1888 until his death in 1916. This era, coming in the aftermath of both the Franco-Prussian War of 1870 and the bitter rivalry between Pasteur and Robert Koch over issues of bacteriology, was an intense period of immunological discoveries as the French school of cellular immunity, led by Metchnikoff, locked horns with the German school of humoral immunity. In contrast to Metchnikoff's emphasis on the cellular process of phagocytosis, humoralists believed that immunity to infectious diseases was the result of the bactericidal action of substances called antitoxins, or antibodies, found in serums. Phagocytes, in their view, merely scavenged the corpses of microbes killed by the chemical substances of the humors.[5]

Debates at the grand international medical and hygiene conferences were vigorous as each side presented the results of experiments that refuted the experiments and theories of the other. As the century closed, however, a variety of discoveries supported the humoralist position. For example, in

[3] Paul Weindling, "Roux et la Diphtherie," in *L'Institut Pasteur*, ed. Michael Morange (Paris, 1991), pp. 137–43; Paul Weindling, "From Medical Research to Clinical Practice: Serum Therapy for Diphtheria in the 1890s," in *Medical Innovations in Historical Perspective*, ed. John Pickstone (London: Macmillan, 1992), pp. 72–83; Paul Weindling, "From Isolation to Therapy: Children's Hospitals and Diphtheria in Fin de Siecle Paris, London, and Berlin," in *In the Name of the Child: Health and Welfare, 1880–1940*, ed. Roger Cooter (London: Routledge, 1992), pp. 124–45.

[4] Olga Metchnikoff, *Life of Elie Metchnikoff* (Boston: Houghton Mifflin, 1926); Alfred I. Tauber and Leon Chernyak, *Metchnikoff and the Origins of Immunology: From Metaphor to Theory* (Oxford: Oxford University Press, 1991).

[5] Tauber and Chernyak, *Metchnikoff and the Origins of Immunology*, pp. 154–74; Silverstein, *History of Immunology*, pp. 38–58.

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1895, Richard Pfeiffer of Koch's laboratory observed the degradation of cholera microbes (bacteriolysis) in cell-free antiserums. Other humoralists soon discovered that cell-free antiserums could agglutinate bacteria (1896) and precipitate both bacterial (1897) and nonbacterial (1899) substances. Further support for the humoralist position emerged from Metchnikoff's laboratory, when the young Belgian Jules Bordet observed in 1899 that red blood cells broke apart (hemolysis) under the influence of cell-free serums containing antibody and a newly discovered serum component, complement. The precise specificity of these serological reactions was exploited to develop useful diagnostic techniques, such as the Wasserman test (1906) for syphilis. Moreover, although Paul Ehrlich's pioneering efforts to quantify and standardize serum antitoxins were contested, they later became the basis for a long-term international and institutionalized effort to standardize immune serums, vaccines, and other biological reagents in the post–World War I era.[6]

By the beginning of the twentieth century, most investigators of immune phenomena were humoralists, except for those of the Institut Pasteur, where Metchnikoff responded to each humoralist discovery with his own battery of experiments and interpretations; he consolidated his position in his 1901 magnum opus *L'immunité dans les maladies infectieuses* (Immunity in Infectious Diseases). By this time, Ehrlich, guided by his empirical studies on serum standardization, had proposed a theory of antibody formation that provided the humoralists with a comprehensive framework they had previously lacked. Drawing on then current theories in organic chemistry, Ehrlich suggested that cells of the body had surface receptors – called side chains – that reacted specifically to nutritive particles as a part of normal cellular metabolism. These receptors could also react with foreign substances such as toxins or other nonnutritive "antigens." Under appropriate antigenic stimulation, cells replaced and overproduced receptors, which entered the serum as circulating antibodies.[7]

As the cellularist–humoralist debate polarized, a compromise position was sought by some investigators, such as British immunologist Sir Almroth Wright, who proposed in 1903 that antibodies became bound to pathogenic

[6] Pauline M. H. Mazumdar, "Immunity in 1890," *Journal of the History of Medicine*, 27 (1972), 312–24; Ernst Bäumler, *Paul Ehrlich: Scientist for Life*, trans. Grant Edwards (New York: Holmes and Meier, 1984); Pauline M. H. Mazumdar, *Species and Specificity: An Interpretation of the History of Immunology* (Cambridge: Cambridge University Press, 1995), pp. 107–22; Jonathan Leibman, "Paul Ehrlich as a Commercial Scientist and Research Administrator," *Medical History*, 34 (1990), 65–78; Alberto Cambrosio, Daniel Jacobi, and Peter Keating, "Ehrlich's 'Beautiful Pictures' and the Controversial Beginnings of Immunological Imagery," *Isis*, 84 (1993), 662–99; Cay Rüdiger-Prüll, "Part of a Scientific Master Plan? Paul Ehrlich and the Origins of His Receptor Concept," *Medical History*, 47 (2003), 332–56; Pauline M. H. Mazumdar, "The Antigen–Antibody Reaction and the Physics and Chemistry of Life," *Bulletin of the History of Medicine*, 48 (1974), 1–21; Pauline M. H. Mazumdar, "The Purpose of Immunity: Landsteiner's Interpretation of the Human Isoantibodies," *Journal of the History of Biology*, 8 (1975), 115–33.

[7] Elie Metchnikoff, *Immunity in Infective Diseases*, trans. F. G. Binnie (Cambridge: Cambridge University Press, 1905); Cambrosio, Jacobi, and Keating, "Ehrlich's 'Beautiful Pictures' and the Controversial Beginnings of Immunological Imagery"; Silverstein, *History of Immunology*, pp. 87–123.

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microbes, which prepared them for being dined on by the phagocytes, a process he called opsonization. The Karolinska Institute faculty found its own compromise when it divided its 1908 prize for Physiology or Medicine between Metchnikoff and Ehrlich.[8]

## THE EMERGENCE OF IMMUNOLOGY

During the first half of the twentieth century, the nature and specificity of the antibody–antigen reaction, the mechanism of antibody formation, and the physical structure of antibody molecules were a few of the central problems around which the emerging discipline of immunology gravitated. These problems were primarily tackled using chemical rather than biological approaches and techniques. Indeed, the term "immunochemistry" was coined in 1904 by Swedish physical chemist Svante Arrhenius, who, with Danish serologist Thorvald Madsen, likened the neutralization of toxin by antitoxin to that of the weak, reversible dissociations of acids and bases, in opposition to Ehrlich's chemical union. Moreover, the increasing frequency of terms such as "immunology," "immunologie," and "Immunitätsforschung" in journals and textbooks indicates not only that scientists were beginning to see immunity as a separate class of phenomena but also that they felt a need to distinguish themselves from those doing "bacteriology" and "pathology." And yet, even after specialized journals were created, such as the German *Zeitschrift für Immunitätsforschung* (1909) and the *American Journal of Immunology* (1916), many researchers continued to publish in journals of medicine, hygiene, and bacteriology.[9]

In the first years of the new century, Ehrlich engaged in a bitter debate with Viennese professor of hygiene Max von Gruber, who rejected the narrow specificity of the antitoxin–toxin (i.e., antibody–antigen) reaction as well as Ehrlich's increasing number of humoral components and their sophisticated terminology. Ehrlich had made his case for the side-chain theory even more persuasive by providing detailed drawings of the alleged receptors on the cell surface and their specific union with the antigen and complement. Less vitriolically, Bordet also criticized these rhetorical elaborations as confusing and unnecessary; instead of a strong chemical bond, he believed that the antibody–antigen interaction was weaker and less specific, like the physical adsorption of a dye to fabric.[10]

[8] Michael Worboys, "Vaccine Therapy and Laboratory Medicine in Edwardian Britain," in Pickstone, *Medical Innovations in Historical Perspective*, pp. 84–103; Alfred I. Tauber, "The Birth of Immunology. III. The Fate of the Phagocytosis Theory," *Cellular Immunology*, 139 (1992), 505–30.

[9] Mazumdar, *Species and Specificity*, pp. 202–13; Arthur M. Silverstein, "The Dynamics of Conceptual Change in Twentieth Century Immunology," *Cellular Immunology*, 132 (1991), 515–31; Silverstein, *History of Immunology*, chaps. 4 and 5.

[10] Mazumdar, *Species and Specificity*, pp. 136–51; Silverstein, *History of Immunology*, pp. 99–107; Rüdiger-Prüll, "Part of a Scientific Master Plan?"; Cambrosio, Jacobi, and Keating, "Ehrlich's 'Beautiful Pictures' and the Controversial Beginnings of Immunological Imagery."

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Another of Ehrlich's critics was Karl Landsteiner, von Gruber's former assistant, who was a physician with experience in both laboratory pathology and organic chemistry. Landsteiner had worked on blood typing, phenylketonuria, and polio. He also rejected Ehrlich's structural-chemical explanation of antibody–antigen interaction in favor of a physical-chemical explanation borrowed from colloidal chemistry. Collaborating with the colloid chemist Wolfgang Pauli, Landsteiner combined both structural and physical-chemical concepts to formulate a theory of interaction that focused on the electrochemical attractions between the surfaces of the antigen and antibody. In 1918, Landsteiner demonstrated that the electrostatic charge outline of an antigen determines the specificity of the antibody, which implied that the enormous diversity of antibody (receptor) specificities envisioned by Ehrlich was unnecessary because the same antibody could react to a variety of similar charge outlines on an antigen.[11]

Forced into early retirement after World War I, Landsteiner accepted an offer to work at the Rockefeller Institute in New York City, where he carried out significant studies of the specificity of serological reactions. Borrowing a technique pioneered before the war by Friederich Obermayer and Ernst Pick, Landsteiner painstakingly modified large protein molecules (carriers) by adding to them smaller, slightly varying chemical groups (haptens) in order to elicit specific antibodies against each hapten group. These haptens could be derived either from naturally occurring pathogenic microbes or from synthetic chemicals not found in nature. Landsteiner's results brought into focus once again the problem of antibody specificity and diversity inherent in Ehrlich's theory: Why would an organism possess preformed antibodies (receptors) that were able to react with artificial antigens found only in the chemist's laboratory?[12]

Dissatisfied with Ehrlich's theory and stimulated by Landsteiner's work on specificity, Prague serologist Ferdinand Breinl and biochemist Felix Haurowitz proposed in 1930 that an antigen serves as a template for the formation of a specific antibody that forms piecemeal on the surface of the antigen as each charged amino acid orients itself to a complementary charge on the antigen. Similar template theories were simultaneously and independently proposed by a few other researchers. An important modification was made by American chemist Linus Pauling, the foremost authority on chemical bonding, who proposed in 1940 that an already synthesized polypeptide strand coiled itself around an antigen to form an antibody of exact specificity. Because the template theory resolved many of the problems of Ehrlich's side-chain theory, it became the accepted view of most immunologists until the late 1950s.[13]

[11] Mazumdar, *Species and Specificity*, pp. 123–35, 214–36; Silverstein, *History of Immunology*, pp. 107–12.
[12] Mazumdar, *Species and Specificity*, pp. 237–53; Karl Landsteiner, *The Specificity of Serological Reactions* (Cambridge, Mass.: Harvard University Press, 1945).
[13] Silverstein, *History of Immunology*, pp. 64–71; Linus Pauling, "A Theory of the Structure and Process of Formation of Antibodies," *Journal of the American Chemical Society*, 62 (1940), 2643–57.

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As the preceding history implies, Behring's antitoxic "property" of immune serums was eventually localized as a salt-precipitable proteinaceous component of the blood. But despite Ehrlich's pictures of side-chain receptors and Pauling's coiling polypeptides, the structure of antibodies remained unresolved until the 1960s. Ultracentrifugation and gel electrophoresis, powerful techniques of physical chemistry developed in the late 1930s, revealed that serum antibodies were identified in the gamma globulin fraction of blood proteins, the so-called gamma immunoglobulins (IgGs). Antibody-producing tumor cells (myelomas) provided immunochemists with pure supplies of monoclonal IgGs, which they could selectively cleave (using chemical and enzymatic reagents) and separate (using techniques such as paper and column chromatography) in order to determine sequence and structural relationships. By the mid-1960s, largely through the efforts of two pioneering researchers – Rodney Porter and Gerald Edelman (who shared the 1972 Nobel Prize in Chemistry) – a molecule of IgG was shown to be a four-chain structure: two "light" polypeptide chains of about two hundred amino acids and two "heavy" chains about twice as long and held together in a Y shape by disulfide bonds. An antibody molecule possessed two antigen binding sites, located on each end of the Y branches, regions where amino acid and nucleic acid sequences showed a high degree of variation.[14]

Besides these conceptual and technical problems, the investigation of which helped immunology emerge as a scientific discipline, there are a number of other areas of immune research that cannot be excluded from a broader treatment of the history of immunity and immunology, such as vaccine therapy, serotherapy, serodiagnosis, anaphylaxis, autoimmunity, blood typing, and studies of immediate and delayed-type hypersensitivity reactions or allergies. Although several researchers had identified antibodies directed against self-antigens, the study of autoimmunity was hindered during the early twentieth century by adherence to Ehrlich's belief that in normal circumstances such antibodies were eliminated or controlled. By contrast, both clinical and scientific studies of hypersensitivity reactions proliferated. Drawing on clinical observations of vaccination reactions and serum sickness, in 1906 the Austrian pediatrician Clemens von Pirquet introduced the term "allergy" to denote any type of altered biological reactivity, whether giving rise to immunity or hypersensitivity. Although the meaning of the term "allergy" subsequently changed, the recognition that allergic reactions might play a role in the pathogenesis of certain diseases (such as asthma, hay fever, eczema, and food intolerance) helped to stimulate the growth of a new clinical specialty. Significantly, von Pirquet's formulation of allergy also helped to sustain links between immunology and medicine at a time when emergent preoccupations with the immunochemical dissection of antigens and

[14] Gerald M. Edelman and W. Einar Gall, "The Antibody Problem," *Annual Review of Biochemistry*, 38 (1969), 415–66. For a detailed analysis of the elucidation of antibody structure in the 1960s and 1970s, see Scott Podolsky and Alfred I. Tauber, *The Generation of Diversity: Clonal Selection Theory and the Rise of Molecular Immunology* (Cambridge, Mass.: Harvard University Press, 1998).

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antibodies were serving effectively to separate the laboratory from the clinic. As the Nobel Prize–winning immunologist Niels Jerne put it many years later, as a direct result of studies on "vaccination, allergy and serological diagnosis, immunology had a private line to medicine, which compensated for its isolation."[15]

Fringe areas of research involving physiological and pathological investigations of immune phenomena later converged after the Second World War to revitalize the science. For example, in 1945, Ray Owen reported the observation that nonidentical twin calves, which share a common circulatory system during fetal development, possess a mixture of each other's blood cells and are unable to produce antibodies against each other's differing blood types. Owen's finding helped shape Australian virologist-turned-immunologist Frank Macfarlane Burnet's theoretical views in the late 1940s about how the organism can distinguish "self" from "nonself," which for him became the central immunological problem. The practical importance of this problem was clearly seen during World War II, when physicians, faced with an increase in the number of severely burned patients, were frustrated by the phenomenon of skin graft rejection. British zoologist Peter Medawar's investigation of graft rejection in mice demonstrated that it is essentially an immune response. After the war, Owen's finding provoked Medawar and his colleagues to investigate the phenomenon experimentally in cattle and other laboratory animals, which led to their 1953 discovery of experimental immunological tolerance, whereby an animal is made to tolerate foreign skin grafts by exposing it to donor cells very early in life.[16]

## THE CONSOLIDATION OF IMMUNOLOGY

Following the war, students of transplantation, tumor biology, allergies, and autoimmune diseases began to see commonalities in their work. As Australian immunologist Gustav Nossal put it, "[S]omething special happened in the 1950s." The renewed emphasis on the importance of cells was one special linking feature that helped consolidate many disparate areas of research. Medawar's studies of graft rejection hinted that lymphocytic infiltration of the graft might play a role in the rejection process. Landsteiner and Merrill

[15] Silverstein, *History of Immunology*, pp. 214–51; Mark Jackson, ed., *The Clinical and Laboratory Origins of Allergy*, *Studies in History and Philosophy of Biological and Biomedical Sciences (Special Issue)*, 34 (2003), 383–98; Niels K. Jerne, "The Common Sense of Immunology," *Cold Spring Harbor Symposium on Quantitative Biology*, 41 (1977), 1–4, at p. 4.

[16] Silverstein, "Dynamics of Conceptual Change in Twentieth Century Immunology"; Ray Owen, "Immunogenetic Consequences of Vascular Anastomoses between Bovine Twins," *Science*, 102 (1945), 400–1; F. Macfarlane Burnet and Frank Fenner, *The Production of Antibodies*, 2nd ed. (Melbourne: Macmillan, 1949); Rupert E. Billingham, Leslie Brent, and Peter B. Medawar, "Actively Acquired Tolerance of Foreign Cells," *Nature*, 172 (1953), 603–6; Leslie Brent, *A History of Transplantation Immunology* (San Diego, Calif.: Academic Press, 1997).

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Chase had reported in the early 1940s that delayed-type hypersensitivity (such as contact sensitivity to chemicals and tuberculin) was an allergic reaction mediated by lymphocytes, not serum antibodies. Finally, immune cells became crucially important for the clonal selection theory and epitomized the general renewed emphasis by immunologists on investigating the biological basis of immune phenomena and their pathologies.[17]

In 1955, Danish immunologist Niels K. Jerne proposed the natural selection theory of antibody formation, which he believed was able to account for a number of seemingly unrelated phenomena of immunity hitherto unexplained. Jerne believed that an organism possesses a large pool of antibodies of diverse specificities. Any antigen that enters the organism will find an antibody to which it can bind reasonably well enough to form a complex that is then transported to cells capable of making more antibodies of the same specificity. Within two years, this idea had been modified by American immunologist David Talmage, and particularly by Burnet, who proposed that the antigen selects out a specific antibody bound to the surface of a lymphocyte, which is then stimulated to proliferate as a clone of cells, each capable of making antibodies of the same antigenic specificity. During the next decade, this clonal selection theory emerged as the central dogma of immunology.[18]

Burnet's modification stressed the role of cells in the immune phenomena of antigen recognition, antibody response, and immunological memory; which cells carried out these functions and how became central research problems for immunologists in the 1950s and 1960s. In 1954, Avrion Mitchison, of Medawar's group in London, showed that the passive transfer of cells, rather than serums, from an animal that had been exposed to a foreign graft to a new unexposed animal conferred in the latter an immunity to subsequent foreign grafts. This result implied that transplantation immunity was a cell-mediated phenomenon. A related phenomenon, graft versus host disease, was also shown to be cell mediated, but in this case, the lymphocytes of a foreign graft attack the tissues of the recipient. The mysterious, nondividing small lymphocyte became the focus of research for Oxford physiologist James Gowans, who demonstrated in the late 1950s and early 1960s that such lymphocytes recirculated continuously from blood to lymph, that they

[17] Gustav J. V. Nossal, *Annual Review of Immunology*, 13 (1995), 1–27, at p. 2; Karl Landsteiner and Merrill W. Chase, "Experiments on Transfer of Cutaneous Sensitivity to Simple Compounds," *Proceedings of the Society for Experimental Biology and Medicine*, 49 (1942), 688–90; Merrill W. Chase, "The Cellular Transfer of Cutaneous Hypersensitivity to Tuberculin," *Proceedings of the Society for Experimental Biology and Medicine*, 59 (1945), 134–55.

[18] Niels K. Jerne, "The Natural-Selection Theory of Antibody Formation," *Proceedings of the National Academy of Sciences USA*, 41 (1955), 849–57; David W. Talmage, "Allergy and Immunology," *Annual Review of Medicine*, 8 (1957), 239–56; F. Macfarlane Burnet, "A Modification of Jerne's Theory of Antibody Production Using the Concept of Clonal Selection," *Australian Journal of Science*, 20 (1957), 67–9; F. Macfarlane Burnet, *The Clonal Selection Theory of Acquired Immunity* (Nashville, Tenn.: Vanderbilt University Press, 1959). For a detailed account, see Podolsky and Tauber, *Generation of Diversity*.

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were long-lived, and that they could react to an antigen by proliferating and initiating immune responses such as skin graft rejection, graft versus host reaction, and immunological tolerance.[19]

The immunological significance of the thymus gland was discovered simultaneously in 1961, when three independent researchers – Robert A. Good, Byron Waksman, and Jacques F. A. P. Miller – and their groups observed impaired immune responses in animals that had been thymectomized at birth. A few years earlier, it had been observed that removing the bursa of Fabricius (a small structure of lymphatic tissue located in the avian cloaca) in chickens at hatching later impaired their ability to mount an antibody response to antigenic challenge. Further experimentation revealed that cells derived from the thymus regulated cellular immune responses, and those derived from the bursa (or, in mammals, some unknown bursa-equivalent structure) regulated antibody-mediated responses. In 1966, however, Henry Claman and his research group in Denver showed that thymus-derived cells and marrow-derived cells need to collaborate in order to produce an antibody response to certain antigens. Two years later, Miller and his colleague Graham F. Mitchell reported that marrow-derived cells are, in fact, the precursors to the cells that make antibodies (plasma cells) but that, for certain antigens, thymus-derived cells are required to "help" this antibody response. In 1969, the terms "T cell" and "B cell" were introduced, and researchers soon showed that T cells are divided into various subsets, such as "helper" T cells, cytotoxic (or "killer") T cells, and "suppressor" T cells. In the 1970s, researchers figured out how to distinguish these subsets of lymphocytes by detecting certain cell surface markers unique to each subset.[20]

The post–World War II consolidation of immunology from disparate fields is further illustrated by the recent elucidation of the molecular and genetic basis of Burnet's central immunological problem, namely "self–nonself" discrimination. Even before Medawar's pioneering research on skin graft rejection, mouse geneticists had determined that the rejection of transplanted grafts of tumorous tissues was a genetically controlled phenomenon and that tumor rejection was a response analogous to the destruction of transfused red blood cells by host serums (hemolysis) when the donor and recipient possessed incompatible blood groups. Both hemolysis and tumor rejection

[19] N. A. Mitchison, "Passive Transfer of Transplantation Immunity," *Nature*, 171 (1953), 267–8; James L. Gowans, "The Mysterious Lymphocyte," in *Immunology: The Making of a Modern Science*, ed. Richard Gallagher, Jean Gilder, G. J. V. Nossal, and Gaetano Salvatore (London: Academic Press, 1995), pp. 65–74.

[20] J. F. A. P. Miller, "The Discovery of Thymus Function," in Gallagher et al., *Immunology*, pp. 75–84; J. F. A. P. Miller, "Uncovering Thymus Function," *Perspectives in Biology and Medicine*, 39 (1996), 338–52; R. A. Good, "The Minnesota Scene: A Crucial Portal of Entry to Modern Cellular Immunology," in *The Immunologic Revolution: Facts and Witnesses*, ed. Sandor Szentivanyi and Herman Friedman (Boca Raton, Fla.: CRC Press, 1994), pp. 105–68. For a review of thymus research in the 1960s, see Craig R. Stillwell, "Thymectomy as an Experimental System in Immunology," *Journal of the History of Biology*, 27 (1994), 379–401.

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were associated with certain blood cell antigens that acted as immunological markers of genetic individuality. The field of immunogenetics was born. Using "congenic mice" – that is, highly inbred strains developed in the mid-1940s – geneticists began to describe the "major histocompatibility complex" (MHC), a set of variable alleles that determined whether or not two mice could successfully swap grafts. By the mid-1960s, a homologous complex of highly variable histocompatibility genes had been assigned to chromosome 7 in humans: the human leukocyte antigens (HLAs). Since 1964, a series of International Histocompatibility Workshops has helped to describe and systematize HLA tissue types, a collaboration that has greatly improved the success of organ transplantation.[21]

The regulatory role of the MHC in other types of immune responses became clearer in the late 1960s and early 1970s when immunologists discovered that the immune response to certain synthetic antigens is mediated by a set of highly variable genes located within the MHC. Further research showed that helper T cells will not "help" B cells make antibodies unless antigen-presenting cells, such as macrophages or B cells, share identical MHC alleles, and that "killer" T cells will not "kill" virally infected cells or tumor cells unless these cells also share identical MHC alleles. In other words, cellular interaction is "restricted" by MHC. The study of antigen processing and presentation became linked with immunogenetics, for researchers determined that T cells only "recognize" short fragments of foreign antigen complexed to specific cell surface molecules expressed by compatible – that is, "self" – MHC genes. This phenomenon of "MHC restriction," which some have interpreted as the genetic basis of "self–nonself" discrimination, was elucidated at the molecular level in the 1980s with the discovery of the T cell receptor and with the subsequent determination of its molecular structure and those of the MHC-antigen complexes with which it specifically interacts.[22]

A different line of research that shed light on cellular cooperation and immune regulation emerged out of the discovery of nonspecific, soluble protein factors, known as lymphokines or cytokines, which are secreted by cells in order to stimulate or depress the activity of other cells involved in immune responses. Although a few immunologically important factors, such as interferon, an antiviral chemical, had already been described by 1957, several significant discoveries of immunologically important factors were made about a decade later, including the factor known today as interleukin-2. By the late 1970s, the number of cytokines and types of cytokinetic activities had proliferated alarmingly, and terminology became a critical problem that required several international meetings to wrangle over. In the 1980s, researchers began to clone the cytokine genes and express them using recombinant technology.

[21] Jan Klein, *Natural History of the MHC* (New York: Wiley, 1986); Alfred I. Tauber, "The Molecularization of Immunology," in *The Philosophy and History of Molecular Biology: New Perspectives*, ed. Sahotra Sarkar (Dordrecht: Kluwer, 1996), pp. 125–69; Brent, *History of Transplantation Immunology*.

[22] Podolsky and Tauber, *Generation of Diversity*.

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Both interferon and interleukin-2 therapy became active areas of clinical research.[23]

Much immunological research during the first half of the century has been characterized as "blind serology" with little theoretical underpinning. In this respect, things changed rapidly in the postwar period. More than any other immunologist, Jerne came to play the role of its incipient theoretician, particularly when in 1973 he proposed a general theory for the function of the immune system that set the agenda for much of the research in the following decade. Drawing on the idea that antibodies can act as antigens for other antibodies, Jerne depicted the immune system as an informationally closed regulative system – a so-called idiotypic network – of reciprocally interacting antibodies, anti-antibodies, and lymphocyte receptors. The theory, which had strong antireductionist overtones, was heavily criticized in some quarters for its metaphysical character but nevertheless contributed to a growing feeling among younger immunologists that theirs was a theoretically mature discipline after all.[24]

## IMMUNITY AS AN OBJECT FOR HISTORICAL INQUIRY

According to Hegel, the owl of historical wisdom flies only at dusk. Already in 1902, however, Ludwig Hopf in Tübingen had published a small book, *Immunität und Immunisirung: Eine medizinisch-historische Studie* (Immunity and Immunisation: A Medical Historical Study), extending from the seventeenth and eighteenth centuries to the fashion of the day, the so-called enzyme theory of Rudolf Emmerich and Oscar Löw, now long since excluded from the historical canon.[25]

Hopf, however, was a lonely reflective owl. Although certain scientists and clinicians (such as von Pirquet, for example) occasionally reflected on the development of their field and particularly on their own contributions, major textbooks on bacteriology and/or immunity from the first half of the twentieth century generally paid little attention to the historical roots of their subjects. The first editions of W. W. C. Topley and G. S. Wilson's *Principles of Bacteriology and Immunity* included a diminutive historical outline of bacteriology but none at all of immunology; Jules Bordet devoted only a few pages to precursors in his 863-page *Traité de I'immunité dans les maladies infectieuses* (Treatise on Immunity in Infectious Diseases, 1939), and William Boyd's

[23] Byron H. Waksman and Joost J. Oppenheim, "The Contribution of the Cytokine Concept to Immunology," in Gallagher et al., *Immunology*, pp. 133–43.

[24] Niels K. Jerne, "Towards a Network Theory of the Immune System," *Annales d'Immunologie (Institut Pasteur)*, 125 C (1974), 373–83; Anne Marie Moulin, "The Immune System: A Key Concept for the History of Immunology," *History and Philosophy of Life Sciences*, 11 (1989), 221–36.

[25] Ludwig Hopf, *Immunität und Immunisirung: Eine medizinisch-historische Studie* (Tübingen: Franz Pietzcker, 1902).

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*Fundamentals of Immunology*, which appeared in four editions from 1943 to 1966, was completely ahistorical. Neither did Hopf have many followers among authors of histories of medical sciences. The most scholarly treatment of the subject was made by William Bulloch, who devoted the last of eleven chapters of his now classic *The History of Bacteriology* (1938) to a history of doctrines of immunity, in which Paul Ehrlich and his side-chain theory of antibody production played the central role at the expense of Metchnikoff. For over a quarter of a century, the most widely read history of early immunological discoveries was microbiologist Paul de Kruif's book *Microbe Hunters* (1926), a self-conscious, if eclectic, popularization of the "great men and their deeds" in microbiology, from Antoni van Leeuwenhoek to Paul Ehrlich.[26]

Only in the 1960s and 1970s, apparently as an accompaniment to the worldwide institutionalization of immunology, did some of the many new textbooks begin to carry short historical essays, typically providing a canonical list of major immunologists and important discoveries. John Humphrey and R. G. White provided one of the first and best introductions in their widely read book *Immunology for Students of Medicine* (1963), and later that decade Macfarlane Burnet included a "history of immunological ideas" (albeit focusing largely on precursors of the clonal selection theory) in his book *Cellular Immunology*. Two decades later, a somewhat different approach was taken by Edward S. Golub and William R. Clark, who allowed historical development itself to organize much of the subject matter in their respective textbooks.[27]

A few deliberately historical contributions were also published in this period. For example, in their book *Three Centuries of Microbiology* (1965), Hubert Lechavalier and Morris Solotorovsky spent two chapters on important experiments in cellular immunology and humoral immunology up to the 1920s. In his book *A History of Medical Bacteriology and Immunology*, W. D. Foster wrote an insightful chapter on "The Scientific Basis of Immunology," in which he resuscitated Metchnikoff from the Bullochian graveyard. He also provided something new in the historiography of immunology, namely a chapter on "The Practical Application of Immunology to Medicine." In 1974, science writer Robert Reid published the book *Microbes and Men* – essentially an updated and extended version of de Kruif's *Microbe Hunters* – as

[26] W. W. C. Topley, *Topley and Wilson's Principles of Bacteriology and Immunity*, 3rd ed., revised by G. S. Wilson and A. A. Miles (Baltimore: Williams and Wilkins, 1946); Jules Bordet, *Traité de l'immunité dans les maladies infectieuses*, 2nd ed. (Paris: Masson, 1939); William C. Boyd, *Fundamentals of Immunology* (New York: Interscience, 1943); William Bulloch, *The History of Bacteriology* (London: Oxford University Press, 1938); Paul de Kruif, *Microbe Hunters* (New York: Harcourt Brace, 1926).

[27] John Humphrey and R. G. White, *Immunology for Students of Medicine* (Oxford: Blackwell, 1963); F. Macfarlane Burnet, *Cellular Immunology* (Melbourne: Melbourne University Press, 1969); Edward S. Golub, *The Cellular Basis of the Immune Response: An Approach to Immunobiology* (Sunderland, Mass.: Sinauer Associates, 1977); William R. Clark, *The Experimental Foundations of Modern Immunology* (New York: Wiley, 1983).

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an accompaniment to the documentary series that aired on that most popular of media, television.[28]

The 125 contributions to the two-volume *festschrift* to Niels K. Jerne in 1981 on the occasion of his seventieth birthday signaled a new era in the historical reflection on immunity and especially immunology. This reflection largely began as autobiographical stories written by the major participants in the consolidation of immunology in the last half of the twentieth century. The *Annual Review of Immunology* (itself a sign of this consolidation) premiered in 1983 with the first part of a long autobiographical memoir by Elvin Kabat, whose career had stretched back to the 1930s. Subsequent volumes contained stories by John Humphrey, Merrill W. Chase, David Talmage, Michael Sela, Brigitte Askonas, Baruj Benacerraf, Avrion Mitchison, Gustav Nossal, and other well-known leaders of the "immunobiological revolution." A series of shorter memoirs and reminiscences in the journal *Perspectives in Biology and Medicine,* as well as presidential addresses to the various national immunological societies, which began to proliferate in the 1960s and 1970s, helped to develop the historical awareness of the new global discipline. A few leading immunologists also published their memoirs in book form, including Burnet's "atypical autobiography" *Changing Patterns,* Medawar's *Memoir of a Thinking Radish,* and Benacerraf's *Son of an Angel.*[29]

After an initial surge of individual memoirs came edited collections of participants' stories. The first international meeting on the history of immunology was the brainchild of accomplished immunologist Bernard Cinadar, who asked pathologist-turned-historian Pauline M. H. Mazumdar to organize it within the framework of the sixth International Congress of Immunology, convening in Toronto in 1986. This influential meeting gave prominence to the Jerne-Burnet selection theory, which, in Mazumdar's words, had "moulded the scientific thought of [the] generation" of immunologists attending the congress.[30]

On the occasion of Michael Heidelberger's hundredth birthday, in 1988, a score of "vital participants, their associates, and witnesses" were asked to give "[n]either a scientific, polemic, nor a historical analysis" but rather a personal perspective on the "immunological revolution" of the twentieth century. A few years later, the editors of another collection of essays, *Immunology: The*

[28] Hubert Lechavalier and Morris Solotorovsky, *Three Centuries of Microbiology* (New York: McGraw-Hill, 1965); W. D. Foster, *A History of Medical Bacteriology and Immunology* (London: Heinemann, 1970); Robert Reid, *Microbes and Men* (New York: Saturday Review Press, 1975). See also H. J. Parish, *A History of Immunization* (Edinburgh: E. and S. Livingstone, 1965).

[29] Charles M. Steinberg and Ivan Lefkovits, eds., *The Immune System*, 2 vols. (Basel: Karger, 1981); F. Macfarlane Burnet, *Changing Patterns: An Atypical Autobiography* (Melbourne: Heinemann, 1968); Peter Medawar, *Memoir of a Thinking Radish: An Autobiography* (Oxford: Oxford University Press, 1986); Baruj Benacerraf, *Son of an Angel* (Great Neck, N.Y.: Todd and Honeywell, 1991).

[30] Pauline M. H. Mazumdar, ed., *Immunology, 1930–1980: Essays on the History of Immunology* (Toronto: Wall and Thompson, 1989).

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*Making of a Modern Science,* admitted that they were "deliberately exclud[ing] the shackles imposed by the word 'history'," insisting by contrast that their aim was rather to recall the "exciting period of research" that helped shape modern immunology and to set this in "the personal context of place and time" – to be, in fact, "passionately biographical."[31]

Another important genre of reflection is the work of the immunologists-turned-historians. Drawing heavily on his lifelong firsthand knowledge of the primary literature, Arthur M. Silverstein wrote a series of articles in the journal *Cellular Immunology* in the 1980s; in 1989, he offered the first book-length treatise on the history of immunity and immunology. Silverstein became increasingly committed to the demands of the historical craft, and in later writings identified a Kuhnian revolution in the development of immunology – from the predominance of chemical approaches in the first half of the twentieth century to the emerging interest in cellular phenomena in the post–World War II period – but so far, few historians of immunology have followed up on the suggestion.[32]

Silverstein was tired of colleagues whose historical horizon rarely went beyond a decade and wanted to provide young immunologists with an understanding of "where immunology is today and how it got there." And yet, it is probably not coincidental that the increasing number of historical reflections followed the emergence of acquired immune deficiency syndrome (AIDS), the most spectacular chapter in the history of public health in the post–World War II period.[33]

Anne-Marie Moulin's *Le dernier langage de la médicine* (The Latest Language of Medicine), fittingly subtitled *Histoire de l'immunologie de Pasteur au SIDA* (*History of Immunology from Pasteur to AIDS*), draws less frequently on the primary immunological literature than does Silverstein, but has a strong narrative drive (a "thrilling book," said Jerne in his preface, probably because he emerged as the hero of the story). Although originally a clinician specializing in parasitology and tropical medicine (a training hardly visible in her "internalist" story, though), Moulin also has a philosophical agenda, which emerges when she goes back to Leibniz's *Monadologie* to suggest what kind

[31] Sandor Szentivanyi and Herman Friedman, eds., *The Immunologic Revolution: Facts and Witnesses* (Boca Raton, Fla.: CRC Press, 1994); Gallagher et al., *Immunology*, pp. 1–4.

[32] Silverstein, *History of Immunology*. For an example of a historian adopting Silverstein's notion of discrete, paradigmatic epochs in the history of immunology, see Ilana Löwy, "The Strength of Loose Concepts: Boundary Concepts, Federative Experimental Strategies and Disciplinary Growth: The Case of Immunology," *History of Science*, 30 (1992), 371–96.

[33] Steven B. Mizel and Peter Jaret, *In Self Defense: The Human Immune System – The New Frontier in Medicine* (San Diego, Calif.: Harcourt Brace Jovanovich, 1985); William E. Paul, ed., *Immunology: Recognition and Response, Readings from Scientific American* (New York: W. H. Freeman, 1991); Emily Martin, *Flexible Bodies: Tracking Immunity in American Culture from the Days of Polio to the Age of AIDS* (Boston: Beacon Press, 1994); William R. Clark, *At War Within: The Double Edged Sword of Immunity* (Oxford: Oxford University Press, 1995); Stephen S. Hall, *A Commotion in the Blood: Life, Death, and the Immune System* (New York: Henry Holt, 1997).

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of "metaphysical atmosphere" might have informed contemporary notions of "the immune system" that climaxed in Jerne's idiotypic network theory of 1973.[34]

In her book *Species and Specificity*, Pauline Mazumdar also interprets the development of immunology in terms of philosophical disputes. The argument among early twentieth-century immunologists was based, she asserts, on the question of whether nature is unified and continuous or composed of a plurality of definable species. Her analysis is focused on Karl Landsteiner, whose unitarian views were in strong opposition to the ideas of specificity and pluralism held by the followers of Koch and Ehrlich.

Alfred I. Tauber, in his *Metchnikoff and the Origins of Immunology: From Metaphor to Theory* (co-written with Leon Chernyak), reinterpreted the phagocytic theory as a fundamental contribution to the understanding of organismal integrity. In his book *The Immune Self: Theory or Metaphor?*, Tauber traces the origin of the central concepts of "self" and "nonself" and attempts to locate them in a broad philosophical context, including the phenomenologies of Nietzsche and Husserl; the conceptual history of the immunologically central notions of "self" and "nonself" is also neatly summarized online in the *Stanford Encyclopedia of Philosophy*. Tauber's latest book on the topic, *The Generation of Diversity*, written together with Scott Podolsky, brilliantly traces the history of the molecularization of immunology during the last three decades; it ends with a discussion of the foundational problems of today's immunology.

In her extremely useful (alas, now out-of-print) compilation of primary sources and translations *Milestones in Immunology*, microbiologist Debra Jan Bibel has also provided historical context and philosophical insight into the major discoveries these selections describe. Vicki L. Sato and Malcolm L. Gefter have compiled a selection of primary readings on cellular immunology, and a collection of seminal papers on allergy was edited by Sheldon Cohen and Max Samter.[35]

The works discussed so far – and here might also be included two proceedings volumes of meetings, held at Boston University in 1993 and Saint-Julien-en-Beaujolais (France) in 1998 – have the construction of the history of immunology and/or the study of immunity as their explicit purpose. Yet there are a number of other works that are primarily written as case studies addressing agendas set by sociology or science studies but, almost by default,

34 Anne-Marie Moulin, *Le dernier langage de la médicine: Histoire de l'immunologie de Pasteur au SIDA* (Paris: Presses Universitaires de France, 1991).

35 Mazumdar, *Species and Specificity*; Alfred I. Tauber, *The Immune Self: Theory or Metaphor?* (Cambridge: Cambridge University Press, 1994); Alfred I. Tauber, "The Biological Notion of Self and Non-Self," in *Stanford Encyclopedia of Philosophy* (http://plato.stanford.edu); Podolsky and Tauber, *Generation of Diversity*; Debra Jan Bibel, ed., *Milestones in Immunology: A Historical Exploration* (Madison, Wis.: Science Tech, 1988); Vicki Sato and Malcolm L. Gefter, eds., *Cellular Immunology: Selected Readings and Critical Commentary* (London: Addison-Wesley, 1981); Sheldon G. Cohen and Max Samter, *Excerpts from Classics in Allergy* (Carlsbad, Calif.: Symposia Foundation, 1992).

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cast light on important events in the history of immunology. The locus classicus of this genre is Ludwik Fleck's now famous book *Enstehung und Entwicklung einer wissenshaftlichen Tatsache* (1935). To illustrate his sociological thesis about the collective and impersonal creation of empirical facts, Fleck brilliantly analyzed "one of the best established medical facts," namely the relation of the Wasserman reaction to syphilis. By doing so, he produced the first sociologically informed in-depth historical analysis of a major episode in the history of immunity and immunology, an approach that has been surpassed only in recent years. Significantly, the work of Fleck, and indeed some of his Polish colleagues, has formed the central focus of some recent expansive histories of immunological knowledge and practice.[36]

In his book *Les microbes: Guerre et Paix* (1984), Bruno Latour applied his now well-known idiosyncratic mixture of Michel Serres's concept of networks and Michel Foucault's notion of micropower to an analysis of how Pasteur's study of microbes was at the center of a network of political, social, and cultural forces in late nineteenth-century France. Although the words "immunity" and "immunology" are hardly mentioned, Latour's account of the "pasteurization" of France nevertheless opens up interesting possibilities for future attempts to place the history of immunity and immunology in a combined micro- and macrocultural context. Gerald L. Geison, in his book *The Private Science of Louis Pasteur*, utilizes the French national hero's private laboratory notebooks to reveal striking discrepancies between those records and his public pronouncements. Thomas Söderqvist's biography of Jerne, *Science as Autobiography: The Troubled Life of Niels Jerne*, utilizes in-depth interviews and a vast number of private documents to show how Jerne's life experience shaped the formation of the selection theory of antibody formation and the idiotypic network theory. Because Jerne played a leading role as a theoretician and promoter of the cognitive view of the "immune system," this existential story of his lifelong search for meaning is an indirect contribution to the history of the theoretical basis of immunology from the 1960s to the 1980s.[37]

During the last ten years, approaches from post-Kuhnian science studies led to a new appreciation of biomedical scientific practices – often on a fine level of day-to-day, face-to-face details – and a new awareness of the

[36] Alberto Cambrosio, Peter Keating, and Alfred I. Tauber, eds., "Immunology as a Historical Object," *Journal of the History of Biology* (*Special Issue*), 27 (1994), 375–8; Anne-Marie Moulin and Alberto Cambrosio, eds., *Singular Selves: Historical Issues and Contemporary Debates in Immunology* (Amsterdam: Elsevier, 2000); Ludwik Fleck, *Genesis and Development of a Scientific Fact* [English translation of *Enstehung und Entwicklung einer wissenshaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollektive* (1935)], trans. Fred Bradley and Thaddeus J. Trenn (Chicago: University of Chicago Press, 1979); Ilana Löwy, "The Immunological Construction of the Self," in *Organism and the Origins of Self*, ed. Alfred I. Tauber (Dordrecht: Kluwer, 1991), pp. 43–75.

[37] Latour, *The Pasteurization of France*; Geison, *The Private Science of Louis Pasteur*; Thomas Söderqvist, *Science as Autobiography: The Troubled Life of Niels Jerne*, trans. David Mel Paul (New Haven, Conn.: Yale University Press, 2003).

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intricate networks of people, laboratory technologies, and local institutional arrangements that sustain these practices. In a series of articles in the early 1990s and summarized in her book *Between Bench and Bedside*, Ilana Löwy has made a "thick description" of experimental immunology at work in the clinic. She followed the practices of a highly publicized clinical trial in France, namely the application of interleukin-2, which has a nonspecific stimulatory effect on the immune system, as a possible agent against cancer. She focused on the transfer of immunological innovation to the clinic and observed that cooperation between laboratory scientists ("mice doctors") and clinicians ("people doctors") was not as clean and unproblematic as is often assumed.[38]

In their book *Exquisite Specificity*, Alberto Cambrosio and Peter Keating also take the reader on an ethnographic cum historical tour of the intricate practices behind a breakthrough that exploits immune phenomena, in this case the invention of monoclonal antibody technology. In Latourian fashion, Cambrosio and Keating "follow" the actors and materials that transformed a simple laboratory technique into one of the most useful tools of modern biomedicine and biotechnology.[39]

In a challenging essay charting recent directions in the history of immunology, Warwick Anderson, Myles Jackson, and Barbara Gutmann Rosenkrantz have persuasively argued that historians have worked largely within the conventional boundaries, or "invented traditions," established by immunologists themselves, and have failed to explore "histories of vague and contingent subjects such as immunity, infection, or allergy." They suggest "alternative histories of immunology, histories not of laboratories but of clinics and cultures." A small number of historians have followed Anderson's ecological vision (evident, for example, in his own work on immunity and race) and begun to explore the history of immunology in these terms. Thus, recent collections of essays have analyzed the origins of clinical allergy and autoimmunity in the early twentieth century, examined the history of specific immunological conditions such as hay fever, asthma, and rheumatoid arthritis, traced the development of novel immunological practices and technologies from a multidisciplinary perspective, and begun to fuse the approaches of medical history with a nascent environmental history. In addition, the construction of immunological knowledge has attracted the attention of anthropologists and literary scholars keen to expose the socioeconomic, political, and cultural contingencies that have shaped the field of immunology in the nineteenth and twentieth centuries.[40]

38 Adele E. Clarke and Joan H. Fujimura, eds., *The Right Tools for the Right Job: At Work in Twentieth Century Life Sciences* (Princeton, N.J.: Princeton University Press, 1992); Ilana Löwy, *Between Bench and Bedside: Science, Healing, and Interleukin-2 in a Cancer Ward* (Cambridge, Mass.: Harvard University Press, 1996).

39 Alberto Cambrosio and Peter Keating, *Exquisite Specificity: The Monoclonal Antibody Revolution* (New York: Oxford University Press, 1995).

40 Warwick Anderson, Myles Jackson, and Barbara Gutmann Rosenkrantz, "Toward an Unnatural History of Immunology," *Journal of the History of Biology*, 27 (1994), 575–94, at p. 587; Jackson,

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In conclusion, the historiography of immunology still has far to go. As for most fields of modern medical science and practice, much of the written history shows a rather narrow "internalist" perspective. There are a few recent and promising works that look more broadly and use more ambitious methods, but many important areas have yet to be explored, perhaps especially on the clinical side. There are no recent institutional histories, and biographers have yet to present their stories as microcosmic pictures of the landscape at large. The "immunological revolution" in twentieth-century science and medicine, and the cultural importance of "self" and "nonself," certainly warrant more attention from historians.[41]

"Clinical and Laboratory Origins of Allergy"; Donna Haraway, *Simians, Cyborgs, and Women: The Reinvention of Nature* (New York: Routledge), pp. 203–30; Emily Martin, "Histories of Immune Systems," *Culture, Medicine and Psychiatry*, 17 (1993), 67–76; Laura Otis, *Membranes: Metaphors of Invasion in Nineteenth-Century Literature, Science, and Politics* (Baltimore: Johns Hopkins University Press, 1999).

41 Tauber, "Biological Notion of Self and Non-Self."

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# 26

# CANCER

*Jean-Paul Gaudillière*

In January 1994, the journal *Scientific American* published a review essay on cancer that opened with a quotation from John Bailar III, a famous epidemiologist at McGill University, claiming that the "war on cancer" had not been won.[1] Bailar was referring to the anticancer campaign launched by President Richard Nixon in 1971 as a civilian alternative to the Vietnam War and as a Republican follow-up to President Lyndon Johnson's War on Poverty.[2] Bailar's argument rested on statistical data from the National Cancer Institute suggesting that U.S. cancer death rates, adjusted for the aging population, went up seven percent during the twenty-five years of a war that was waged by means of research investments – both biological and clinical.

That article reminds us that cancer remains the visible, frightening, and "scientific" disease it has been for more than a century. More than tuberculosis or syphilis, which were considered conquered after World War II, cancer was the scourge of the twentieth century.[3] From the late nineteenth century, the growing incidence of various types of tumors, as well as the limitations of existing therapies, have been at the center of Western medical discourses increasingly concerned with relatively wealthy and aging populations. Since then, experts have viewed the formation of tumors as a problem of unlimited multiplication of cells, a process that might be controlled by

[1] Tim Beardsley, "A War Not Won," *Scientific American*, 270 (January 1994), 130–8.

[2] R. A. Rettig, *Cancer Crusade: The History of the National Cancer Act of 1971* (Princeton, N.J.: Princeton University Press, 1977).

[3] I. Löwy, "The Century of the Transformed Cell," in *Science in the Twentieth Century,* ed. J. Krige and D. Pestre (Amsterdam: Harwood, 1998), pp. 461–78; D. Cantor, "Cancer," in *Companion Encyclopedia of the History of Medicine*, ed. W. F. Bynum and R. Porter (London: Routledge, 1993), pp. 537–61; P. Pinell, "Cancer," in *Medicine in the Twentieth Century*, ed. R. Cooter and J. Pickstone (Amsterdam: Harwood, 1998), pp. 671–86; D. Cantor, ed., "Cancer," *Bulletin of the History of Medicine* (*Special Issue*), 81 (Spring 2007).

The writing of this chapter would not have been possible without the works of David Cantor, Ilana Löwy, and Patrice Pinell. Their excellent analyses of the cancer historical literature provided the background for this update.



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physical or chemical means derived from a better understanding of cell growth and cell division. And if the history of cancer nicely exemplifies the uneasy relationships between the practice of science and the practice of medicine, it also sheds a powerful light on the transformation of Western medicine into a large-scale biomedical venture. The Second World War was in this respect a turning point associated with the very rapid growth of health care systems, industrial production of drugs, and new research infrastructures.

## THE CLINICAL CANCER: TUMORS, CELLS, AND DIAGNOSIS

Around the middle of the nineteenth century, medical science saw a major displacement of the scale of analysis: Pathologists began to search for *cellular* lesions as the fundamental signs of disease, underlying grosser changes in organs and bodily symptoms.[4] Increasing use of microscopes, dyes, and fixatives in the study of abnormal growths turned cancer into a cellular disease. This move, however, was not straightforward; historians of medical classifications and medical pathology[5] have stressed the importance of Theodor Schwann (1810–1882) and his cell theory, and of Rudolf Virchow (1821–1902) and other German pathologists.

Up to the 1850s, cancer – like other constitutional diseases – was perceived as linked to inflammation. Self-sustained cellular reproduction originated in a violent reaction to an external stimulus, and classifications routinely encompassed tumors along with inflammatory lesions such as cysts or tubercles. One major clinical distinction, based on the practice of autopsy, contrasted "benign" and "malignant" tumors: The former were localized and without projective growths, whereas the latter thrust themselves into surrounding tissues and were occasionally associated with comparable growing masses in other body sites.

Rudolf Virchow was among the first pathologists to promote systematic microscopic examination of cancer tissues.[6] He believed that tumor cells developed from primitive cells dispersed in the normal tissues, and his emphasis on cells rather than tissue masses led to a revision of tumor classification. For instance, in contrast to the Parisians, who emphasized the local growth of tumors, German pathologists viewed leukemias as malignant neoplastic diseases associated with an increased number of colorless white

[4] W. Bynum, *Science and the Practice of Medicine in the Nineteenth Century* (Cambridge: Cambridge University Press, 1994).

[5] L. J. Rather, *The Genesis of Cancer* (Baltimore: Johns Hopkins University Press, 1978); J. S. Olson, *The History of Cancer: An Annotated Bibliography* (New York: Greenwood Press, 1989); D. De Moulin, *A Short History of Breast Cancer* (Dordrecht: Kluwer, 1989).

[6] Rather, *Genesis of Cancer*; R. C. Maulitz, *Morbid Appearances* (Cambridge: Cambridge University Press, 1987).

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blood corpuscles produced by cell multiplication. The histological gaze was further exemplified by the introduction of "lipomas" (neoplasms derived from fat cells), chondromas (originating in cartilage), myomas (originating in muscle), osteomas (in bone tissue), or adenomas (in glandular tissue).

Virchow's plea for the cellular origins of all cancers was soon complemented by attempts at cataloging tumors according to their resemblance to, and putative derivation from, embryonic tissues. During the 1860s and 1870s, one of the most disputed questions was the existence of cancers of epithelial origin. In his masterwork *Die Krankhaften Geschwülste* (*The Disease-related Tumors*), published between 1863 and 1867, Virchow explained that all cancers derived from connective tissue. He held that inflammatory tumors, tubercles, and true tumors originated in response to stimuli acting on the reservoir of undifferentiated cells dispersed in the body's connective tissues. Virchow's favorite pieces of evidence were instances of tumors containing typical epidermal cells that had arisen in closed sites having no anatomical connections with epithelia, for instance within bone marrow, meninges, or ovaries.

But as new stains and fixing techniques became available in the second half of the nineteenth century, fine-grain analysis of embryo development expanded and the careful ordering of microscopical sections according to the stage of development allowed the recognition of zones of homogeneous development, presumptive territories, and tissue genealogies.[7] The notion that embryonic layers could easily transform into one another became less and less popular, thus undermining Virchow's theory. In the 1860s, echoing the embryologist's practice of sequential slide arrangement, Wilhelm Waldeyer presented evidence that germ-layer theory was compatible with all histological findings: Tumors of a particular type were derived from tissues of that type. Waldeyer's own study of breast cancer provided evidence that isolated clusters of epithelial cancer cells were in fact connected to overlying normal epithelium. Waldeyer's views were widely accepted up to the Great War.

What was the clinical use of such genealogical schemes? Histopathology was an academic discipline with little service function until the late nineteenth century.[8] Like autopsies, histological analysis was chiefly "after the event" – the studies took place after death or after the completion of the surgical intervention that provided the tumor tissues. For clinicians, the important symptoms of cancer remained visible growths and cachexia (wasting); histology was rare and merely for confirmation. We know little about the development of cancer histological diagnosis in the decades before 1920, but we may see it as a by-product of the "laboratory revolution in medicine," as

[7] N. Hopwood, "Giving Body to Embryos: Modeling, Mechanism and the Microtome in the Late 19th Century Anatomy," *Isis,* 90 (1999), 462–96.

[8] R. C. Maulitz, "Rudolf Virchow and the Program of Pathology," *Bulletin of the History of Medicine,* 52 (1978), 168–82; L. S. Jacyna, "The Laboratory and the Clinic: The Impact of Pathology on Surgical Diagnosis in the Glasgow Western Infirmary, 1875–1910," *Bulletin of the History of Medicine*, 62 (1988), 384–406.

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a feature typical of a time when chemical, bacteriological, and microscopical laboratories adjacent to clinical wards were used to provide complementary information and then started to perform service functions.

## THE FIRST TECHNOLOGICAL DISEASE: CANCER AND RADIOTHERAPY

The last five years of the nineteenth century were a marvelous time for medical physics. In 1895, Wilhelm Roentgen (1845–1923) produced a new sort of ray with the strange property of going right through the human body. His pictures of bones immediately triggered the imagination of dozens of physicists and doctors. In 1898, Marie Curie (1867–1934) and her husband Pierre introduced to the physics community another kind of ray. Their radium was soon compared with x-rays because of its physical properties as well as its ability to burn living tissues. The story of the alliance between physicists and cancer specialists has been widely retold, beginning with heroic portraits of Marie Curie and radium. But in contrast to the old linear histories that presented an easy transition from the physical to the biological laboratories and then to clinical wards, recent historical inquiries have described more complex moves, back and forth, between academic and industrial laboratories.

Medical historians have documented the development of medical radiology as an autonomous diagnostic specialty.[9] Clinicians were pivotal, handling both x-ray plates and patients with their medical records; although the electrical industry rapidly seized the radiology market, it did not lead the innovation process. Radium therapy offers a different picture. Historians of physics have shown that late nineteenth-century laboratories working on radium were closely linked to industry;[10] they depended on the manufacture of a whole range of tools, often by commercial ventures, and participated in the development of new products. In the case of radium, connections emerged out of the need for research material. In order to investigate radium, the Curies needed significant quantities, and it was difficult to purify. Once the procedures for its chemical preparation had been worked out, the Curies thought that it was economically sound to have an industrialist take over the process. Crafting an agreement with Armet de Lisle, a producer of chemical goods, they established a firm to produce radium for the market. This in turn stimulated two developments. First, within the Curie laboratory, interest in metrology expanded because standard measurements were critical for both the study of radioactive rays and the evaluation of the commercial material. Second, given the scale and amount of work necessary to prepare radium, a firm

9 B. Pasveer, "Depiction in Medicine as a Two-Way Affair: X-ray Pictures and Pulmonary Tuberculosis in the Early 20th Century," in *Medicine and Change: Historical and Sociological Studies of Medical Innovation*, ed. I. Löwy (Paris: INSERM–John Libbey, 1993), pp. 85–105.

10 S. Boudia, "The Curie Laboratory: Radioactivity and Metrology," *History and Technology,* 13 (1997), 249–65.

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could barely survive on orders coming from the physical laboratories alone. Medical uses could provide a much more significant market, and Armet de Lisle and the Curies worked hard to expand the clinical usefulness of radium.

Bénédicte Vincent has recounted the early days of the Institut du Radium in Paris and Marie Curie's efforts in advancing radium therapy.[11] Before the creation of a new specialized research center, comprising a physical laboratory, a biological laboratory, and a cancer ward, research was done within the industrial context. The factory laboratory included a biological and medical section, an arrangement that facilitated exchanges with hospital physicians who borrowed de Lisle's radium-containing tubes and needles for clinical tests. Early applications concentrated on skin diseases, for instance lupus, and the "burning" power of radium rays was soon applied to superficial cancer tissues. The importance of cancer as a medical target was reinforced by Jean Bergonié and Louis Tribondeau's demonstration that rapidly dividing tumor cells were more susceptible to rays than were differentiated cells.[12]

The industrial history of radioactive materials also sheds some light on the changing scale of radium therapy. In the early 1920s, one major achievement of the Curie Foundation was the invention of the "radium bomb" – an apparatus containing massive amounts of radium (i.e., several grams) – to produce a penetrating beam. Such a beam was thought to be the only way of treating deep tumors. This invention, Patrice Pinell argues, was only possible in a powerful center that could concentrate many scientific and social resources. This could happen in Paris, within the prestigious Radium Institute. But the accumulation of massive amounts of radium was a prerequisite, and this can be traced back to the discovery of large deposits of uranium in the Congo, which made the fortune of the Belgian radium industry and rapidly scaled up both supply and use. The existence of the radium bomb in turn changed local views about cancer, linking therapeutic efficiency with machines of escalating proportions. The 1920s cancer clinic thus invented a form of "big medicine" that was soon to affect other areas of medical care.[13]

But the centrality of radiology on the French cancer scene may be misleading, for radium therapy was less prominent in other countries, with the exception of Sweden. British radiology between the two world wars was mostly small scale,[14] but control was centralized. A dozen different cancer hospitals received state-owned material from the National Radium Commission, which wanted to assure that the technique would be available to patients in the whole country. But the (national) Medical Research Council was also

[11] B. Vincent, "Genesis of the Pavillon Pasteur of the Institut du Radium," *History of Technology*, 13 (1997), 293–305.

[12] P. Pinell, *Naissance d'un fléau: La lutte contre le cancer en France* (Paris: Editions Métailié, 1992).

[13] On the importance of technological platforms in the rise of biomedicine, see A. Cambrosio and P. Keating, *Biomedical Platforms* (Cambridge, Mass.: MIT Press, 2003).

[14] D. Cantor, "The MRC Support for Experimental Radiology during the Inter-war Years," in *Historical Perspective on the Role of the MRC*, ed. J. Austoker and L. Bryder (Oxford: Oxford University Press, 1989), pp. 181–204.

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involved, and some cancer centers were remarkable for their routinization, statistics, and scientific collaborations.[15] By the late 1930s, radiation dosage systems had been developed in Manchester as in Paris and Stockholm.

In the United States, rays were never used as widely as in France; surgery remained the treatment of choice.[16] But a few elite medical centers such as Memorial Hospital in New York City developed the alliance between physics and cancer studies, thus participating in the engineering culture of oncology. Building on the capabilities of a strong electrical industry, Memorial Hospital, for instance, pressed for the construction of large x-ray units – a one million volt apparatus was finally built in the late 1930s.

## CANCER AS SOCIAL DISEASE: VOLUNTARY HEALTH ORGANIZATIONS AND BIG BIOMEDICINE

How does a disease become a social scourge? Cancer, like tuberculosis, is a good example of the transformation of a peculiar form of human suffering into an object of both professional and public action. If one takes the creation of cancer societies as criteria for the "socialization" of cancer, one may say that the "dread disease" was invented around the First World War. The (British) Imperial Cancer Research Fund was started in 1902, the American Society for the Control of Cancer in 1911, the French Ligue Contre le Cancer in March 1918, and the British Empire Cancer Campaign in 1923.[17]

What factors can account for this surge of public interest? Historical epidemiology stresses the rising incidence of the disease as a result of longer life expectancy; and vital statistics in France, Britain, Germany, and the United States showed definite growth in the number of cancer cases. But the meaning of such a trend remained unclear. Were there more cancer patients because medical records changed, because diagnosis became more sophisticated, or because tumors were more numerous? In the 1920s, U.S. cancer specialists tended to favor the second hypothesis. They reviewed medical records of the late nineteenth century, stressing the great number of poorly diagnosed cases and the uncertainty of mortality data. In France, World War I changed medical statistics, and cancer emerged as a public health target in the context of the medical surveys of conscripts; military doctors quantified the incidence of the disease in the male population, raising concerns about its social impact.

[15] See E. Magnello, *A Centenary History of the Christie Hospital, Manchester* (Manchester: Christie Hospital and the Wellcome Unit for the History of Medicine, University of Manchester, 2001).

[16] J. T. Patterson, *The Dread Disease: Cancer and Modern American Culture* (Cambridge, Mass.: Harvard University Press, 1987).

[17] J. Austoker, *A History of the Imperial Cancer Fund: 1902–1986* (Oxford: Oxford University Press, 1988); D. F. Shaughnessy, "The Story of the American Cancer Society" (PhD thesis, Columbia University, 1957); Pinell, *Naissance d'un fléau.*

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Like other social diseases, cancer was a collective target, a "boundary object" for heterogeneous groups of politicians, administrators, businesspeople, female activists, and, last but not least, doctors. The alliance between physicians and lay cancer organizations emerged as technical expert roles developed strongly in expanding welfare states, and the cancer societies played a very significant role in shaping the scientific and medical meaning of the disease, especially in the United States. There, elite (university) doctors faced a badly regulated profession, a booming medical market, and a weak state; they looked for the support of lay reformers and progressives in order to fight "quacks" and other cancer "healers." The American Society for the Control of Cancer (ACSS) was established by these specialists to promote cancer education – targeting general practitioners and putative patients.

There is no good history of the early decades of the ACSS, when it seems to have played a purely professional role.[18] As a collective body for the assessment of new therapies of cancer, it organized surveys of hospital practices, inquiries into foreign experience, and reviews of the literature; and it produced reports and leaflets that demonstrated a characteristic form of medicalization, focusing on the notion that "cancer can be cured" if diagnosed early and removed when the tumor is small. But the society's early-diagnosis campaign did not just promote regular medical examinations. It created a form of body awareness, namely the search for early signs of breast cancer by means of "self-examination." European cancer societies, supported by notabilities from the upper and middle classes, often followed similar paths. Whereas the poor worker affected with *tuberculosis* was to be observed and disciplined (for instance by the visiting nurses), the more respectable *cancer* patient became a target for information and collaboration.

A distinctive and well-documented feature of the cancer-fighting societies was their enrollment of laypeople in the support of research. But the alliances differed among nations, and one may contrast the clinical construction of the cancer problem in France with the biomedical mobilization emerging in the United States. The French Ligue Contre le Cancer was a vast assembly of philanthropists, ranging from industrialists to politicians and "*dames du monde*."[19] Soon after its birth, the Ligue began to support in the country's main cities the establishment of *cancer centers* – disease-oriented research and treatment sites that would enforce the collaboration between surgeons and radiologists. These centers would take curable but poor patients, trading access to care against experimentation. Although formally a private institution, the French cancer society was in fact a quasi public health organization exemplifying the classical alliance of state and elite professionals; the money for the cancer centers was to a large extent provided by a special tax. Establishing anticancer centers became a source of prestige and influence for many local authorities and surgeons, and the number of centers boomed in the

[18] M. B. Shimkin, *Contrary to Nature* (Washington, D.C.: NIH Publications, 1979).
[19] Pinell, *Naissance d'un fléau.*

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early 1920s. The resulting managerial crisis was resolved in 1925, when Justin Godard, the Ligue's director, became health secretary. It was then argued that the anticancer centers should be sites of innovation focusing on radiotherapy, and to avoid dispersion and misuse of radium, no new centers were to be created. Thus, in a move anticipating the reorganization of the French health care system after 1945, the Ligue came to provide care through a network of high-technology hospitals. In the late 1930s, debates about the cancer policy focused on the "misfortune of the privileged": the fact that patients from the middle and upper classes could not access the anticancer centers and radiotherapy. It was for the comparatively rich that the Ligue advocated enlarged access and health care reform.

In contrast to the French society, the ACSS did not provide care. During the first phase of its existence, it advised general practitioners and local oncologists against "unproved" and proprietary treatments. Radiotherapy fell victim to this role as the society's doctors kept a critical eye on the various radium treatments advertised in the nation and viewed radiotherapy as a costly experimental procedure that could not compete with surgery. This role of the society came under attack during the New Deal, when the ACSS became the American Cancer Society and one of the most influential voluntary health organizations in the United States.

In the late 1930s, the medical directors of the ACS started a "Women's Field Army" to collect money and distribute educational material. Within the context of New Deal activism, this army rapidly grew in size, gathering more than 100,000 women by 1939 (most of them also participating in the National Federation of Women's Clubs). This provided the basis for a second reorganization. In the culture of emergency characteristic of World War II and best illustrated by the penicillin project, medical progress was to be achieved by a combination of basic sciences, industrial production, public investment, and organizational skills. During the war, a group of lay activists dominated by business managers and led by Mary Lasker (1900–1994) ousted the doctors from control of the ACS. They emphasized increased participation of the public, rational management, and large-scale funding, and they used the media for large collection campaigns like the March of Dimes' against polio. As the new ACS gathered millions of dollars for fighting cancer, it became the leading promoter of medical research in the United States.

In the late 1940s, within the context of failed attempts to develop national health insurance, the ACS increasingly viewed research and technical innovation as the clue to the cancer problem,[20] and its powerful lobbying system made it a major player in postwar biomedical policies.[21] Along with scientists, public health officials, and a few congressmen, the society's officials pleaded for continued expansion of the cancer research infrastructure. Thanks to

[20] P. Starr, *The Social Transformation of American Medicine* (New York: Basic Books, 1982).
[21] S. P. Strickland, *Politics, Science, and the Dread Disease* (Cambridge, Mass.: Harvard University Press, 1972).

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this pressure, the budget of the National Cancer Institute grew from a few hundred thousand dollars in 1945 to one billion dollars in the late 1970s during the national "war against cancer" launched by President Nixon in 1971.[22]

The managerial style of operations invented by the ACS also affected the more traditional role of cancer education. The postwar cancer society expanded its prevention campaigns into integrated screening operations aimed at every cancer-prone American. The best example of this pattern is the development of the "pap smear test."[23] First, the ACS provided most of the research funds that Dr. George Papanicolaou (1883–1962) used for his research on cytological testing for cervical cancer. Second, it set up testing consultations in many cancer centers while organizing the training of the technicians requested to perform several hundred thousand tests a year. Finally, it provided millions of leaflets and films claiming that every American woman should be regularly "pap smeared."

The postwar status of cancer was deeply shaped by the image of the "invulnerable man" fighting disease with science, a stereotype that dominated Europe as well as the United States. Some aspects of the cultural visibility of cancer, however, did not appear on the Old Continent; for instance, the link between cancer etiology and the rising concerns over the pollution of the environment during "the greening of America."[24] Studies of chemical carcinogenesis at the National Cancer Institute had started in the late 1940s, but the turning point was Rachel Carson's publication of her novel *Silent Spring* (1962), which triggered a national debate over chemical pollution. This debate in turn changed the meaning of chemical carcinogenesis by pointing to industries as threats and risk factors. Robert Proctor has shown how the fight between environmental activists, representatives of the chemical industry, epidemiologists, and cancer specialists was channeled into debates over the meaning of statistical data, animal models, and dose/response curves that underlay regulatory intervention by the U.S. Food and Drug Administration (FDA) or the newly founded Environmental Protection Agency.

## CANCER AS A BIOLOGICAL PROBLEM

The "biomedicalization" of cancer meant that clinical problems and pathological material were turned into biological research systems; there was much less flow in the opposite direction. This century-long influx of tumor cells

[22] Rettig, *Cancer Crusade.*

[23] E. Vayena, "Cancer Detectors: An International History of the Pap Test and Cervical Cancer Screening, 1928–1970" (PhD thesis, University of Minnesota, 1999).

[24] R. Proctor, *Cancer Wars: How Politics Shapes What We Know and What We Don't Know about Cancer* (New York: Basic Books, 1995).

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into biological laboratories nurtured many disciplines – including physiology, biochemistry, immunology, genetics, and molecular biology.

At the start of the century, leading cancer experts thought that the secret of the disease resided within the malignant cell, so they should join forces with other specialists in order to identify the minute differences between a normal cell and a cancerous one. Biochemistry was then an advancing discipline whose members hoped that their ability to identify the chemical reactions essential to the survival of cells would lead to the discovery of metabolic features specific to cancer cells. This idea received strong support when the German biochemist Otto Warburg (1883–1970) advanced results that seemed to demonstrate that the production of cellular energy did not follow the same pathways in cancerous and normal cells.[25] Warburg's claim that the tumor cells did not respire, but instead fermented carbohydrates and grew with little oxygen, fit observations about the low level of blood supply in tumors. During the interwar years, this theory led to hundreds of comparative investigations focusing on enzymes and other cell structures participating in energy metabolism. The quest proved disappointing, and in the late 1930s the research program's foundation was challenged. Many leading oncologists came to think that the metabolic changes were a consequence rather than a cause of the cancerous state: The metabolic differences were just side effects of the rapid proliferation rate. Meanwhile, the study of cancer cells had favored two changes in the field of biochemistry. First, it had contributed to the identification and isolation of several enzymes. Second, as chemical analogues of steroid hormones became available from the industry and considered as putative carcinogenic substances, the search for a metabolic understanding of cancer was redirected toward hormonal regulation.

Immunology, too, was deeply affected by the definition of cancer as a biological problem, as is aptly demonstrated by the history of transplantation studies.[26] In the early twentieth century, laboratory scientists studied cancer by surgically transferring tumors from one animal to another. These tumors were often rejected, and this phenomenon of "resistance" was considered to be of major medical interest; it might lead to means of increasing the resistance of patients against their disease. But the "experimentalization" of tumor transplantation led research in a very different direction. Inbred strains of mice were developed in order to check the role of genetic factors in the acceptance or rejection of grafts; their use stabilized the practice of transplantation by reducing the variability of results, forcing acceptance of the notion that the fate of tumors (and grafts more generally) depended only on the genetic constitutions of donor and host. This result made transplantation studies clinically less interesting but provided new tools for doing biology

[25] Olson, *History of Cancer.*

[26] I. Löwy, *Between Bench and Bedside: Science, Healing and Interleukine 2 in a Cancer Ward* (Cambridge, Mass.: Harvard University Press, 1996).

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and mammalian genetics in the form of inbred lines and graft-based purity testing. In the 1930s, the circulation of genetically controlled mice and tumor cell lines (maintained indefinitely in culture) contributed to restraining the variability of experimental systems and increasing the use of animal pathological models.[27]

Although laboratory studies of tumor transplantation contributed little to the treatment of cancer, they provided very valuable systems for investigating the genetic factors responsible for tissue compatibility. By the late 1940s, mouse geneticists started to select strains of mice that would differ by one "histocompatibility" gene only. These strains were raw material for genetic linkage analysis, leading to the identification of the H gene complex of the mouse. The mouse case in turn provided a reference for deciphering the human leukocyte antigen (HLA) system.

The research field whose birth was most closely articulated with cancer research was molecular biology. After World War II and the expansion of new systems for visualizing and describing macromolecules – including the electron microscope, the ultracentrifuge, and x-ray crystallography – viruses became remarkably popular research objects and contributed to the revival of the infectious theory of cancer causation.[28] Although the notion that cancer may be transmissible was never very popular among clinicians, it was developed by experimentalists. In 1911, Peyton Rous (1879–1983) succeeded in inducing leukemia in chickens by means of filtered extracts of tumor tissue that no longer contained cells or bacteria.[29] But the filterable agent of chicken sarcoma remained an oddity until the 1930s, when the isolation of a rabbit papilloma virus at the Rockefeller Institute expanded the population of invisible viral carcinogenic factors. The notion that cancer viruses were small, replicating, transmissible particles hidden in host cells, where they induced metabolic changes resulting in unlimited growth, led in two different research directions.[30] One direction was a renewed interest in studies of

[27] J.-P. Gaudillière, "Circulating Mice and Viruses: The Jackson Memorial Laboratory, the National Cancer Institute and the Genetics of Breast Cancer," in *The Practices of Human Genetics,* ed. E. Mendelsohn and M. Fortun (Dordrecht: Kluwer, 1999), pp. 89–124; K. Rader, *Making Mice: Standardizing Animals for American Biomedical Research, 1900–1955* (Princeton, N.J.: Princeton University Press, 2004).

[28] N. Rasmussen, *Picture Control: The Electron Microscope and the Transformation of Biology in America, 1940–1960* (Stanford, Calif.: Stanford University Press, 1997); A. N. Creager, *The Life of a Virus: Wendell Stanley, TMV, and Material Models in Biomedical Research* (Chicago: University of Chicago Press, 2001); J.-P. Gaudillière, *Inventer la biomédecine* (Paris: La Découverte, 2002) (English trans. New Haven, Conn.: Yale University Press, forthcoming).

[29] I. Löwy, "Variances of Meanings in Discovery Accounts: The Case of Contemporary Biology," *Historical Studies in the Physical and Biological Sciences*, 22 (1990), 87–121; H. J. Rheinberger, "From Microsomes to Ribosomes: Strategies of Representation," *Journal of the History of Biology*, 28 (1995), 49–89; T. van Helvoort, "Viren als Krebserreger: Peyton Rous, das 'infektiöse Prinzip' und die Krebsforschung," in *Stretegien der Kausalität: Konzepte der Krankheitsverursachung im 19 und 20. Jahrhundert*, ed. Christoph Gradmann and Thomas Schlich (Pfaffenweiler: Centaurus, 1999), pp. 185–226.

[30] A. N. Creager and J.-P. Gaudillière, "Experimental Arrangements and Technologies of Visualization: Cancer as a Viral Epidemic," in *Infection and Heredity: A History of Disease Transmission*, ed. J.-P. Gaudillière and I. Löwy (London: Routledge, 2001), pp. 203–41.

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Rous chicken sarcoma by means of the new molecular machines. Following this track, the Rockefeller biologist Albert Claude (1899–1991) initiated fundamental studies of cell structure, including the identification of microsomes and ribosomes.[31]

The second line of investigation involved the multiplication of oncogenic viruses in mammalian organisms. The first agent found in the mouse, a "milk factor" that induced mammary tumors in cancer-prone strains, was a virus transmitted from mothers to suckling newborns. Its discovery helped to weaken genetic theories of cancer because a "vertical epidemic" was now thought to be responsible for what had been considered a problem of hereditary predisposition. In the 1950s and 1960s, scientists isolated viruses that induced leukemias – and other forms of cancer – in many laboratory rodents. The availability of these viruses attracted the biologists interested in cell metabolism and growth, who in turn contributed to the flow of cancer virus discoveries. In the late 1960s, the conviction that such agents were hidden everywhere in mammalian cells was shared by many cancer specialists as well as by prominent medical policymakers. This visibility led to the establishment of a special research program of the National Cancer Institute to support research on animal model systems, the search for human cancer viruses, and the development of new means of intervention, namely cancer vaccines.[32]

This U.S. Virus Cancer Program did not deliver much for the cancer specialist but proved to be of critical importance for the development of molecular genetics and for the emergence of the "oncogene paradigm."[33] In the early 1970s, studies of cancer viruses had resulted in a theory that the chromosomes of normal cells permanently harbored genes of remote viral origin, which could stimulate ongoing cell division when activated by factors such as hormones, chemicals, or viral infections. In the 1980s, as genetic engineering and new biotechnology firms produced new means of manipulating, transferring, isolating, and reproducing DNA segments, this oncogene model was reshaped.[34] Genes homologous to the viral oncogenes were described in normal cells, which put an end to the search for vertically transmitted cancer viruses while providing new targets in the form of normal cellular elements controlling cell multiplication. The mutation of such genes would be an early step in the genesis of cancer. The new oncogenes have in the last two decades reached the cancer clinics, but mainly as tools for the diagnosis of uncommon tumors such as neuroblastomas. Their principal role remains in the biological research laboratory, where as a typical outcome of the

[31] Rasmussen, *Picture Control.*

[32] J.-P. Gaudillière, "The Molecularization of Cancer Etiology in the Postwar United States: Instruments, Politics, and Management," in *Molecularizing Biology and Medicine*, ed. H. Kamminga and S. de Chadarevian (Amsterdam: Harwood, 1998), pp. 139–70.

[33] M. Morange, "From the Regulatory Vision of Cancer to the Oncogene Paradigm," *Journal of the History of Biology,* 30 (1997), 1–29.

[34] J. Fujimura, *Crafting Science: A Sociohistory of the Quest for the Genetics of Cancer* (Cambridge, Mass.: Harvard University Press, 1996).

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molecularization of cancer etiology, they have proved to be very important in the control of cell differentiation and cell metabolism. They are thus of interest to specialists in hormones, protein regulation, and embryonic development as well as cancer.

## ROUTINE EXPERIMENTATION: CHEMOTHERAPY AND CLINICAL TRIALS

In the twentieth century, medicine increasingly depended on the industrial production of drugs. Up to World War II, cancer chemotherapy was a disreputable topic, despised by serious doctors who knew only two ways of handling the rogue cells: cutting and burning. How then did cancer chemotherapy come to the forefront of scientific research, industrial production, and clinical investigation?[35]

Early chemical treatments for cancer were directly related to the medical research on poisonous gases and nutrition funded during World War II by the U.S. Office for Scientific Research and Development (OSRD). After the war, researchers transferred this model to civilian clinical research, initiating screening programs for chemicals with antitumor properties.[36] Building on the example of the penicillin program, the research was goal oriented, large scale, and collaborative – linking universities, hospitals, and pharmaceutical firms. Between 1945 and 1954, two programs (the first at Sloan Kettering Institute in New York City and the second at the National Cancer Institute) investigated the effects of thousands of natural and synthetic compounds provided by the U.S. pharmaceutical companies. One problem with the laboratory modeling of antitumor properties was that the effective doses and toxic doses of anticancer chemicals were shown to be very close together, so that judging the therapeutic value was difficult. Extensive clinical experimentation seemed necessary to adapt these substances to the treatment of humans, and the growth of chemotherapy became linked to the organization of numerous and large-scale clinical trials.

In the 1950s, leading cancer specialists, the American Cancer Society, and the pharmaceutical industry put pressure on the U.S. Congress to support chemotherapeutic research. This lobbying resulted in the creation of the Cancer Chemotherapy National Service Center (CCNSC), a quasi pharmaceutical house run by the National Cancer Institute, which received increasing appropriations (5 million dollars in 1956, 28 million in 1958) to coordinate all aspects of the development of anticancer drugs. To follow the model of industrial drug research required reducing the variability of two elements: the

[35] Lowy, *Between Bench and Bedside.*

[36] R. Bud, "Strategy in American Cancer Research after World War II: A Case Study," *Social Studies of Science*, 8 (1978), 425–59.

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tumor-bearing mice employed as models of human bodies and the patients participating in the clinical trials.

Mice and tumors were controlled through the use of genetically standardized strains produced through a CCSNC production plan conducted in collaboration with the biologists and engineers from the main U.S. producer of laboratory mice, the Jackson Memorial Laboratory. The CCNSC experts fixed the protocols for breeding the mice, transplanting the tumors and testing the chemicals. They also organized the quality controls of screening laboratories and the statistical analysis of the data. This was a large enterprise, handling tens of thousands of compounds every year – from which only a few dozen reached the wards.

The control of patients was achieved through centralization and standardization of the trials. The Clinical Studies Panel of the CCSNC established a system of surveillance of the quality of the trials, focusing on laboratory analysis, randomization procedures, homogeneous protocols, and objective indicators of tumors' regression. Clinical researchers were organized into "task forces" for intensive cooperation between sites and experts. In the mid-1960s, a task force dedicated to the treatment of acute lymphatic leukemia (ALL) in children announced that they had achieved a cure of the disease. This success rested on complex clinical management complementing the administration of the drug, but it was perceived as a breakthrough. Leading oncologists, health administrators, and U.S. politicians then hoped that the extension of the same methods would lead to the cure of most malignancies. In the 1960s and 1970s, the diffusion of cancer drug therapies was achieved, not through the general use of a few compounds (as has been the case with antibiotics), but through the transformation of clinical trials using combinations of drugs in a form of palliative care. Chemotherapy thus became a system of routine experimental treatment managed by a new medical character – the medical oncologist, who specialized in the testing of putative anticancer therapies.

## CANCER NUMBERS: RISKS AND THE BIOMEDICALIZATION OF EVERYDAY LIFE

If the twentieth century was the cancer century, it was also the century of unquestioned "trust in numbers." In medicine, this trend was reflected in the rising importance of statistics as instruments of therapeutic evaluation,[37] but the use of numbers as a means of objectification was not limited to clinical management. Historians and participants agree that after World War II, studies of disease causation were deeply affected by the increasing use of probability-derived statistics and by new interest in chronic rather than

[37] H. Marks, *The Progress of Experiment: Science and Therapeutic Reform in the United States, 1900–1990* (Cambridge: Cambridge University Press, 1997).

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infectious disorders. Large-scale surveys of cancer patients were pivotal in this transformation.

The most studied debate in postwar cancer epidemiology is the controversy over the link between tobacco use and lung cancer, which developed in the 1950s and 1960s.[38] Robert Proctor's book *The Nazi War on Cancer* has shown, however, that the targeting of tobacco as a culprit for a rising incidence of lung cancer had emerged decades before in Germany;[39] early studies of lung cancer patients were initiated within the context of the Nazi public health campaign against tobacco. This campaign, which significantly reduced the consumption of cigarettes (especially after 1942, when it was enhanced by war restrictions), banned tobacco advertisements as well as smoking in public places. It also stimulated medical research on the effects of tobacco consumption; German doctors collected samples suggesting that smokers were numerous among lung cancer patients.

These studies did not make a significant impact outside Germany, perhaps because they were associated with Nazi policies. But the issue of tobacco and lung cancer surfaced again in the 1950s – in Britain and in the United States – and served as a Trojan horse for the acceptance of the statistician's role in medicine. Historians of medical statistics have stressed the influence of Austin Bradford Hill (1897–1991) and Richard Doll (1912–2005) from the (British) Medical Research Council's Statistical Unit. After a first retrospective study published in 1950, these two scientists organized what they viewed as a methodologically sounder survey: a prospective follow-up of the smoking habits and health status of British doctors. The study lasted for decades, with major reports published in the 1960s. It was contested by the tobacco industrialists and also by some geneticists, physiologists, and cancer specialists. The debate was heavily laden with methodological and statistical considerations about causation and correlation, about the assessment of multiple factors, and about control groups and tests of significance. The epidemiology of "risk factors" was born out of the tobacco and lung cancer controversy and the contemporary debates about the causes of cardio-vascular disease.

One typical innovation was the notion of "relative risk," computed from the distribution of a putative disease factor between a population of affected persons and a control group. In the 1960s, this technology was employed to list and order a wide range of highly heterogeneous factors affecting the incidence of cancer. Relative risk was also a mathematically simple notion that could help quantify, objectify, and facilitate policymaking, an aspect best exemplified by the developments of the lung cancer and tobacco controversy on the American scene. The leading force behind the large American

[38] A. Brandt, "Cigarette, Risk, and American Culture," *Daedalus*, 119 (1990), 155–76; V. Berridge, "Science and Policy: The Case of Postwar British Smoking Policy," in *Ashes to Ashes: The History of Smoking and Health*, ed. V. Lock, L. Reynolds, and E. M. Tansey (Amsterdam: Rodopi, 1998), pp. 143–57; Patterson, *Dread Disease.*

[39] R. Proctor, *The Nazi War on Cancer* (Princeton, N.J.: Princeton University Press, 1999).

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lung cancer surveys was – once again – the American Cancer Society, which provided money, medical connections, and political influence for the statisticians who demonstrated that the risk of lung cancer among heavy smokers was twenty times that of the general population. Working hand in hand with officers of the Public Health Service, the ACS managers turned the issue into a major political battle exemplifying the need to target public policy on health hazards and informing consumers about the risks associated with their behavior. Relative risks of cancer were routinely discussed in Congress, newspapers, and public education meetings. In the 1970s, the notion became so much part of the cancer culture that the ACS pamphlets on breast cancer no longer listed causal agents in order to explain the origins of the disease but included a list of factors ordered on the basis of their relative contribution to the disease as measured in terms of relative risk.

This modern culture of risk is not peculiar to medicine. Sociologists have argued that the last quarter of the twentieth century was the time when a new "risk society" took shape in the United States and Europe.[40] This risk society has been characterized by: (1) a general understanding of environmental as well as health-related problems as consequences of technological action; (2) the part played by risks and risk exposure in the constitution of personal identities and social groups; and (3) a widespread mistrust in scientific expertise and in the state institutions in charge of risk management. The historiography of repeated public controversies about cancer causes (radiation, chemical pollutants, food additives) or alternative cancer treatments (vitamin C, fat-free or fiber-rich diets) substantiates the point.[41]

The most recent signs of a "cancer risk society" originated in genomics and its application to the cancer problem. Following the launching of the U.S. genome project in 1987, there has been a revival of genetic explanations of cancer. But contemporary genes are molecular genes, which can be cloned, reproduced, or used as probes and, putatively, as drugs. The search for cancer genes has accordingly become a hot topic, focusing on molecular linkage studies of families with heavy cancer histories. Biotechnology start-up companies, as well as university laboratories, are participating in the race. When successful, this search has led to the sequencing of DNA segments whose mutation means a high risk of developing a cancer. Within the new political economy of biomedicine, these genes have been patented and commercial diagnostic services developed.[42] As diagnosis often comes without specific

[40] U. Beck, *Risk Society* (London: Sage, 1992).

[41] C. Sellers, *Hazards of the Job: From Industrial Disease to Environmental Health Science* (Chapel Hill: University of North Carolina Press, 1997); E. Richards, "The Politics of Therapeutic Evaluation: The Vitamin C and Cancer Controversy," *Social Studies of Science*, 18 (1988), 653–701; Evelleen Richards, *Vitamin C and Cancer: Medicine or Politics?* (New York: St. Martin's Press, 1991); M. R. Edelstein, *Radon's Deadly Daughters: Science, Environmental Policy, and the Politics of Risk* (Lanham, Md.: Rowman and Littlefield, 1998).

[42] M. Cassier and J.-P. Gaudillière, "Recherche, médecine et marché: La génétique du cancer du sein," *Sciences Sociales et Santé*, 18 (2000), 29–50.

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means of prevention or treatment, such developments have raised considerable criticism. In the case of breast cancer, the most eloquent opponents of the creation of a population of women labeled "at high risk" are women's health organizations, who argue for women's free choice to be tested or not while fearing the iatrogenic and discriminatory effects of increased genetic testing.[43]

## CONCLUSION: THE CANCER CELL AFTER A CENTURY?

As the twentieth century closed, gene therapy had also become the object of criticism in the columns of major scientific journals such as *Science* and *Nature*. Cautious of overrepeated promises of turning DNA into a medicine, clinicians now explained that most existing studies dealt with technical feasibility rather than therapeutic efficacy; many years of research would be necessary to ground gene therapy in a solid understanding of gene transfer. Yet most people agree that cancer science and medicine have changed very significantly in the last twenty years. Tumors transplanted from one animal to another, tissue slides, large screening programs, and repositories of medical records have been replaced by DNA probes, gene maps, molecular design, and computer databases. To put it in a nutshell, a distinctive "cancer century" may have come to its end.

As argued here, twentieth-century cancer was dominated on the one hand by the cancer cell and on the other hand by the development of physical and chemical means of intervention. The cell-centered view of cancer allowed scientists who were studying a wide range of biological problems to benefit from large resources, both material and financial, in order to develop new experimental systems and research objects; the impact on the biological sciences has thus been tremendous. But the experimentalization of cancer was not a one-way process. Clinical work, too, was profoundly transformed as radiations, chemicals, and statistical techniques were mobilized for an increasing number of cancer patients. These developments were greatly accelerated after the Second World War, when they contributed to the expansion of a vast "biomedical complex" linking state research agencies, elite medical centers, university laboratories, and pharmaceutical firms.

But today the cancer cell seems to be fading away – displaced by the DNA molecule. The origin of cancer has now become a problem of genetic information, molecular lesions are viewed as the new frontiers of drug design, and genetic risks are the targets of a new individualized and privatized form of

[43] Barron H. Lerner, *The Breast Cancer Wars: Hope, Fear and the Pursuit of a Cure in Twentieth-Century America* (New York: Oxford University Press, 2001); M. H. Casamayou, *The Politics of Breast Cancer* (Washington, D.C.: Georgetown University Press, 2001); S. Morgen, *In Our Own Hands: The Women's Health Movement in the United States* (New Brunswick, N.J.: Rutgers University Press, 2002).

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public health. And yet a historically informed observer might be tempted to think that the molecular specialists of cancer are pouring "old wines into new bottles." To be sure, any such equation of genomics with postwar chemotherapy slants the story, but it does serve to emphasize the continuities in a biomedical enterprise deeply shaped by its technological and industrial roots. From that viewpoint, the focus changes but "cancer research" rolls on.

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# 27

# THE BRAIN AND THE BEHAVIORAL SCIENCES

*Anne Harrington*

The increasing visibility and sense of intellectual opportunity associated with neuroscience in recent years have in turn stimulated a growing interest in its past. For the first time, a general reference book on the history of science has seen fit to include a review of the history of the brain and behavioral sciences as a thread to be reckoned with within the broader narrative tapestry. On the one hand, this looks like a welcome sign that a new historical subfield has "come of age." On the other hand, when one settles down to the task of composing a "state of the art" narrative, one realizes just how much these are still early days. The bulk of available secondary literature still swims in a space between nostalgic narratives of great men and moments, big "march of ideas" overviews, and an unsystematic patchwork of more theorized forays by professional historians into specific themes (e.g., phrenology, brain localization, reflex theory).[1]

The challenge of imagining a comprehensive narrative is made all the more formidable by the fact that we are dealing here with a history that resists any easy or clean containment within disciplinary confines. The paper

[1] Among exceptions or partial exceptions, Roger Smith's historiographically thoughtful *Fontana History of the Human Sciences* (London: Fontana, 1997) embeds questions about the brain–behavior relationship within a larger argument about what could constitute a history of the "human sciences," which Smith actively resists reducing to a story about biologically oriented natural sciences. Also useful is an expansive and exuberant overview of just those same sciences by an "insider" in the field: see Stanley Finger, *Origins of Neuroscience: A History of Explorations into Brain Function* (New York: Oxford University Press, 1994). There is also the historiographically more ambitious work by Edwin Clarke and Stephen Jacyna, *Nineteenth-Century Origins of Neuroscientific Concepts* (Berkeley: University of California Press, 1987). More recently, Michael Hagner in Zurich and colleagues of his at the Max Planck Institute for the History of Science in Berlin have taken a lead in this area, with a number of important solo-authored and edited collections: *Homo Cerebralis: Der Wandel vom Seelenorgan zum Gehirn* (Berlin Verlag, 1997), *Ecce Cortex: Beiträge zur Geschichte des modernen Gehirns* (Wallstein, 1999), *Mindful Practices: on the Neurosciences in the Twentieth Century* (Cambridge: Cambridge University Press, 2001).

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trail of ideas, experiments, clinical innovations, institutional networks, and high-stakes social debates not only moves across obvious sites of activity such as neurology, neurosurgery, and neurophysiology but also traverses fields as (only apparently) distinct as medicine, evolution, social theory, psychology, asylum management, genetics, philosophy, linguistics, anthropology, computer science, and theology.

We are also dealing with a history that challenges us to engage one of the largest questions that may be asked by historians: What has been the outcome of the effort by human beings over the past two centuries to apply the categories of scientific understanding to *themselves* – beings caught between a universe of social and moral realities and a universe that seems to stand outside of such realities and that they have learned to call "natural"? On all sorts of levels, our fractured understandings of ourselves meet and jostle together uneasily in this history, and any approach that fails to recognize this will in some fundamental sense miss the point.

## GHOSTS AND MACHINES: DESCARTES, KANT, AND BEYOND

Questions of where to begin a story are always contested, and we have chosen to discover a "beginning" to the history of the brain and behavioral sciences in the seventeenth century, the time when the new natural philosophers of Europe had begun to converge on a model of a universe in which everything appeared capable of being accounted for in terms of matter and motion and described using the language of mathematical geometry; everything, that is, except perhaps those same philosophers themselves – those little spots of consciousness that peered through telescopes, scribbled calculations, pondered infinity, and longed for immortality, all while living inside a body that decayed, grew sick, and could be rendered dead without a moment's warning.[2]

How was the scientist to understand the place of his own conscious mind in a world of matter and motion? Did his soul alone transcend the physical laws of the universe, interacting with the body (perhaps via a specific location, or special "seat"), while itself remaining untouched by the ravages of mortality and the prison cell of mechanical determinism? The notorious mind–body dualism of René Descartes – about which more ink has been spilled than can begin to be reviewed here – appeared to offer this promise.[3] Descartes' "ghost in the machine" (as Gilbert Ryle would much later famously mock it) began with a reflex model of physiology to account for most intelligent

[2] Steven Shapin, *The Scientific Revolution* (Chicago: University of Chicago Press, 1996).

[3] Some useful recent studies include: Marleen Rozemond, "Descartes's Case for Dualism," *Journal of the History of Philosophy*, 33 (1995), 29–64; Timothy J. Reiss, "Denying the Body? Memory and the Dilemmas of History in Descartes," *Journal of the History of Ideas*, 57 (1996), 587–608.

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functions in humans and all intelligent functions in animals[4] but then posited the existence in human beings alone of "something else" – a kind of pure thinking substance or rational soul that was able to move the body directly, at will, via the so-called animal spirits. The machine-body interacted with this soul, but the soul was the final authority in all volitional psychological events. "The will is so free in its nature that it can never be constrained," Descartes asserted.

But how creditable was this idea? In the eighteenth century, the French philosopher Voltaire would ask sardonically how it was that the great Newtonian heavens conform without exception to the commands of physical law but there remains in the universe "a little creature five feet tall, acting just as he pleases, solely according to his own caprice."[5]

Indeed, in the early twentieth century, the philosopher Alfred North Whitehead would reflect on the incoherences inherent within that original Cartesian vision of a dualistic universe and the enduring problems they had made for future efforts to think well about human minds and the living world.

> During the seventeenth century there evolved the scheme of scientific ideas which has dominated thought ever since. It involves a fundamental duality, with material on the one hand, and on the other hand mind. In between there lie the concepts of life, organism, function, instantaneous reality, interaction, order of nature, which collectively form the Achilles heel of the whole system.[6]

Some people went on the offensive against Descartes early on. In the mid-eighteenth century, the French physician and philosopher Julien Offray de la Mettrie (1709–1751) took up arms against Descartes' dualistic metaphysics and proposed to simply eliminate one half of the binary opposition. Mind, he proposed, could be simply dissolved into matter. In his soon to be notorious 1748 book *L'Homme Machine* he pushed the point: "Since all the faculties of the soul depend to such a degree on the proper organization of the brain and of the whole body that apparently they are this organization itself, the soul is clearly an enlightened machine."[7]

In this era, to call the soul a machine – enlightened or otherwise – meant that you believed all of its thoughts and behaviors were products of the same impersonal laws of matter and motion that had been shown by the great Isaac Newton to govern the stars and planets. For every Alexander Pope who

[4] Certainly reflex theory is invariably conventionally depicted as having its "origins" with Descartes. Georges Canguilhem has argued, however, that this is a retroactive construction of origins, which began after the establishment of mechanist theory around 1850. He credits the notion instead to the eighteenth-century work of Thomas Willis. See, among other works, Georges Canguilhem, *La formation du concept de réflexe au XVII^e^ et XVIII^e^ siècle* (Paris: Presses Universitaires de France, 1955).

[5] This quotation and the quotation from Descartes in the previous paragraph were cited in Daniel Robinson, *The Enlightened Machine* (New York: Columbia University Press, 1980), p. 12.

[6] Alfred North Whitehead, *Science and the Modern World* (Cambridge: Cambridge University Press, 1926), pp. 83–4.

[7] Julien Offrey de la Mettrie, *Man a Machine*, ed. and trans. G. C. Bussey (La Salle, Ill.: Open Court, 1912), p. 48.

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celebrated the clarifying intellectual power of Newton's accomplishments ("Nature and Nature's laws lay hid in night; God said, 'let Newton be,' and all was Light"), there was a Friedrich Schiller who shuddered at the deterministic prison it appeared to make of the universe ("Like the dead stroke of the pendulum, Nature – bereft of gods – slavishly serves the law of gravity"). Good or bad, in the late eighteenth century, Immanuel Kant came forward to insist that actually, in the case of living creatures – including and especially human beings – Newtonian categories of mechanistic causality fell short. To make sense of the presenting realities of life and mind, Kant said, human judgment was forced to postulate another principle of causality, which he called "natural purpose" (*Naturzwecke*). This was a form of explanation in which the working parts of an organism were to be understood in terms of the teleology or purposive functioning of the organism as a whole.[8]

For a time, this piece of the Kantian legacy would offer a touchstone to a new generation of researchers who aimed to find a "third way" between Cartesian theistic dualism on the one side and crude materialistic reductionism on the other. Figures such as Karl Ernst von Baer and Johannes Müller in Germany and Thomas Laycock in England worked within a naturalistic framework that historian Timothy Lenoir has characterized as "teleological mechanism" – a framework that had room for at least some of those unstable conceptual categories identified by Whitehead that since the seventeenth century had haunted the fault line between those two monoliths of our metaphysics, "mind" and "matter."[9]

## THE PIANO THAT PLAYS ITSELF: FROM GALL TO HELMHOLTZ

By the early nineteenth century, however, this first antireductionist science of mind, life, brain, and body would come under increasingly successful attack by a new generation of workers. The story here is complex, internally contentious, and not seamless. One strand begins at the start of the nineteenth century with the work of Franz Joseph Gall, who would become renowned (and also derided) for his system of "organology" or phrenology. This system was rooted in three fundamental principles: The brain is the organ of the mind (not an obvious proposition at the time); the brain is a composite of parts, each of which serves a distinct mental "faculty"; and the sizes of the different parts of the brain, as assessed chiefly by examining the bumps on the skull, correspond to the relative strengths of the different faculties served.[10]

[8] See Clark Zumbach, *The Transcendent Science: Kant's Conception of Biological Methodology*, Nijhoff International Philosophy Series, vol. 15 (The Hague: Nijhoff; Boston: Kluwer, 1984).

[9] Timothy Lenoir, *The Strategy of Life: Teleology and Mechanics in Nineteenth-Century German Biology* (Dordrecht: Reidel, 1982).

[10] Franz Josef Gall, *On the Functions of the Brain and Each of Its Parts*, trans. W. Lewis, 6 vols. (Boston: Marsh, Capen and Lyon, 1835). To track the further development of phrenological thinking, see

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Gall was certainly not the first to interest himself in the relationship between organic structure and different aspects of psychic activity in the brain. Before Gall, the philosopher-naturalist Charles Bonnet had gone so far as to declare that anyone who thoroughly understood the structure of the brain would be able to read all the thoughts passing through it "as in a book." Bonnet, though, working in a Cartesian mode, had imagined the brain's presumed different organs as vehicles that the immaterial soul manipulated at will, like a pianist at the keyboard. Where Gall most clearly broke from his predecessors was in his decision to eliminate this pianist, this overruling soul, and posit instead a brain composed of some thirty self-animated organs that together generated the totality of the human mind and personality. Within Gall's system, the piano was to play itself.

Originally ridiculed in the historical literature as a pseudoscience of "bump-reading," the past thirty years have witnessed a partial rehabilitation of phrenology, both as an approach to brain–behavior relations that primed the pump for enduring work to come and as an anticlerical and politically potent force that expressed itself in institutional sites ranging from the asylum to the popular lecture hall.[11]

For the purposes of this chapter, however, it will suffice to emphasize a different kind of point: that Gall's work contributed to and, even more, exemplified a spreading approach to mind–brain relations characterized by two interconnected strategic principles: (1) to break mind down to its functional building blocks is to know it, and (2) if you can ground a piece of mind in its presumed corresponding piece of brain, then you can claim it for science. This way to truth was not a necessary one (a different approach, for example, would be chosen by evolutionary biology), but it did help launch empirical programs both in the laboratory and the clinic that would prove highly productive.[12] Indeed, with the advent of new "imaging" technologies that allow one to "see" different parts of the living brain "light up" in response to tasks and stimuli, the approach is more alive than ever.

Whatever challenge Gall and his ilk offered to Christian dualistic theologies, one thing this first generation of workers rarely, if ever, seriously questioned was the Kantian insistence that living organisms need to be understood teleologically: that the characteristics of mind are not just products of *causes*

J. G. Spurzheim, *Phrenology or the Doctrine of the Mental Phenomena*, 2nd American ed. (Philadelphia: Lippincott, 1908). For a sense of what, at the time, represented the most trenchant critique of this approach to the brain, see J. P. M. Flourens, *Phrenology Examined*, trans. C. L. Meigs (Philadelphia: Hogan and Thompson, 1846).

11 For one of the first serious intellectual analyses of phrenology, see Robert Young, *Mind, Brain, and Adaptation in the Nineteenth Century* (Oxford: Clarendon Press, 1970). For some studies of phrenology from a cultural and political perspective, see Steven Shapin, "Homo phrenologicus: Anthropological Perspectives on an Historical Problem," in *Natural Order: Historical Studies of Scientific Culture*, ed. Barry Barnes and Steven Shapin (London: Sage, 1979), pp. 41–71; Roger Cooter, *The Cultural Meaning of Popular Science: Phrenology and the Organization of Consent in Nineteenth-Century Britain* (Cambridge: Cambridge University Press, 1984).

12 This is a point developed by Susan Leigh Star in her book *Regions of the Mind: Brain Research and the Quest for Scientific Certainty* (Stanford, Calif.: Stanford University Press, 1989).

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but are what they are for *reasons*. A broader shift away from this kind of approach to mind and brain began to gather force in the late 1840s (ironically, during the same time that new evolutionary ideas were reinstating concerns with functional utility elsewhere in the life sciences). We can track the shift by following the rise and growing influence of a closely knit group of "organic physicists" working in Germany – Hermann von Helmholtz, Emil du Bois-Reymond, Ernst Brücke, and Karl Ludwig. These were men who had come of age under the influence of the "teleological mechanists" and had also together resolved to rebel against their teachers – seeking instead to build a science in which all explanations of living processes would ultimately find translation into the new causal-material understandings of the physical sciences. As these men famously put it in 1847:

> [N]o other forces than the common physical-chemical ones are active within the organism. In those cases which cannot be explained by these forces, one has either to find the specific way or form of their action by means of the physical mathematical method or to assume new forces equal in dignity to the chemical-physical forces inherent in matter, reducible to the forces of attraction and repulsion.[13]

From the synthesis of organic substances such as urea in the laboratory, to the establishment of cell theory, to new mechanistic understandings of embryological development, a series of milestone events in the mid-nineteenth century acted together to invest the biophysicists' cause with considerable momentum. Of all of the apparent success stories, however, none was more historically salient for the vision than the establishment in the late 1840s of the law of conservation of energy, or the first law of thermodynamics, associated especially with physiologist-turned-physicist Helmholtz.[14] "The law in question," explained Helmholtz in an 1862 popular lecture on the topic, "asserts that the quantity of force which can be brought into action in the whole of Nature is unchangeable, and can neither be increased nor diminished." In other words, all forms of energy (mechanical, kinetic, thermal) were equivalent and could be transformed into one another. There was nothing special, nothing "extra" that was needed to understand life, including the lives and minds of human beings. As the medical physiologist Rudolf Virchow put matters in 1858: "[T]he same kind of electrical process takes place in the nerve as in the telegraph line; the living body generates its warmth through combustion just as warmth is generated in the oven; starch is transformed into sugar in the plant and animal just as it is in a factory."[15]

[13] Cited in M. Leichtman, "Gestalt Theory and the Revolt against Positivism," in *Psychology in Social Context*, ed. A. Buss (New York: Irvington, 1979), pp. 47–75, quotation from note at p. 70.

[14] Hermann von Helmholtz, *Über die Erhaltung der Kraft: Eine physikalische Abhandlung* (Berlin: George Reimar, 1847).

[15] Rudolf Virchow, "On the Mechanistic Interpretation of Life [1858]," in Rudolf Virchow, *Disease, Life, and Man: Selected Essays*, translated and with introduction by Lelland J. Rather (Stanford, Calif.: Stanford University Press, 1958), pp. 102–19, quotation at p. 115.

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## IMAGINING BUILDING BLOCKS: FROM LANGUAGE TO REFLEX

Even as the biophysicists were gaining ground from their base within the German-speaking countries, a revised vision of the brain as a collection of modular mental functions would begin to find a new life in Descartes' birthplace, France. In the 1860s, the French neuroanatomist and anthropologist Paul Broca used certain clinico-anatomic evidence to persuade his colleagues, and much of the international scientific community, that at least one of the phrenological mental faculties – the "faculty of articulate language" – in fact had a discrete "seat" in the brain, and that this seat lay in the third frontal convolution of the (as became more clear a few years later, exclusively *left*) frontal lobe of the human cortex.[16]

There is a lot that is not obvious about Broca's ability to turn the tide of international opinion in favor of a localizationist approach to brain function when opinion had been so solidly opposed to it for almost two generations preceding. On the face of things, the elements with which he had to work do not appear particularly auspicious: a small handful of patient cases, mostly of older people whose multiple ailments clouded the clinical presentation of speech loss, murky autopsy data that required considerable equivocation to make the evidence "come out right," and critics standing ready with apparently more plentiful and less ambiguous counterevidence. To bring this success story into focus, a rich "contextual" reading therefore appears necessary. The language localization efforts, for example, were undertaken during a time in France when republicanism was on the rise and the monarchy and Catholic Church were on the defensive. Thus, the French neurologist Pierre Marie, at the turn of the new century, recalled how medical students in France had quickly seized on the new doctrine of localizationism because, by its materialistic radicalness and distastefulness to the older generation, it seemed to represent scientific progress, free thought, and liberal politics. In Marie's words: "For a while, among the students, faith in localization was made part of the Republican credo."[17]

[16] Paul Broca, "Remarques sur le siège de la faculté du langage articulé, suivies d'une observation d'aphémie (perte de la parole)," *Bulletins de la Société Anatomique*, 36 (1861), 330–57; Paul Broca, "Du siège de la faculté du langage articulé," *Bulletins de la Société d'Anthropologie*, 6 (1865), 377–93; Anne Harrington, *Medicine, Mind and the Double Brain* (Princeton, N.J.: Princeton University Press, 1987).

[17] Pierre Marie, "Revision de la question de l'aphasie: L'aphasie de 1861 à 1866; essai de critique historique sur la genèse de la doctrine de Broca," *Semaine médicale* (1906), 565–71; Harrington, *Medicine, Mind and the Double Brain*, pp. 36–49. For studies in other national contexts that reinforce a similar point about broader political resonances between politics and studies of the brain and physiology, see Stephen Jacyna, "The Physiology of Mind, the Unity of Nature, and the Moral Order in Victorian Thought," *British Journal of the History of Science*, 14 (1981), 109–32; P. J. Pauly, "The Political Structure of the Brain: Cerebral Localization in Bismarckian Germany," *International Journal of Neuroscience*, 21 (1983), 145–50.

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The language localization success story also comes into clearer focus when one locates it inside a larger effort within French racial anthropology of the time to determine the biological bases of the "known" mental differences existing among the different races. One widely accepted assertion from this work held that members of the allegedly evolutionarily superior white European races also possessed a considerably more developed frontal area than the "primitive" nonwhite human races, who were supposed to have larger posterior brain regions. Broca's close colleague, the French neuroanatomist Pierre Gratiolet, had gone so far as to classify the Caucasian, Mongoloid, and Negroid races in terms of their allegedly dominant brain regions: as "frontal race," "parietal race," and "occipital race," respectively.

Given this, we can begin to see why Broca might have been so motivated to seek the seat of a faculty such as language (used to such stunning effect by fellow Europeans from Shakespeare to Voltaire to Goethe) in the frontal lobes. And we can also begin to appreciate the logic whereby the ultimate localization of articulate language in the frontal region of the left hemisphere alone (and the corresponding brain-based link made between language and right-handedness) would contribute to a broader discourse in which the brain's right hemisphere became the "savage," the "female," the "mad," and the "animal" side of the brain. We are here concerned with a "brain" that is functioning in part as a flexible symbolic resource, a concrete metaphor for the carrying out of a society's moral and political work. Parenthetically, this would be no less true in the 1970s, when the "split-brain" operations, associated with the work of people such as Roger Sperry, Joseph Bogen, and Michael Gazzaniga, reopened questions about our brain's two hemispheres and their possible different "cognitive styles" (see also the section on "Technological Imperatives").[18]

Now the plot thickens further. Back in the 1820s, anatomists Charles Bell and François Magendie together had demonstrated that the spinal cord was functionally dual, with the posterior nerves acting as a channel for (incoming) sensory information and the anterior nerves acting as a channel for (outgoing) motor responses. In this way, these men helped establish an apparent material basis in the nervous system for "reflex" action. An important project of the 1830s and 1840s focused on systematically extending the new sensory-motor reflex model of nervous functioning to ever higher levels of the nervous system. The cerebral cortex itself, however, had been exempted from this creeping colonization, honored as a more or less mysterious physiological

[18] Harrington, *Medicine, Mind and the Double Brain*, especially chaps. 2 and 3. For a fuller sense of how racial concerns played themselves out in these debates, see P. Broca, "Discussion sur la perfectibilité des races," *Bulletins de la Société d'Anthropologie*, 1 (1860), 337–42. For a useful introduction to the larger context of brain research and racializing anthropology, see Stephen Jay Gould, *The Mismeasure of Man* (Middlesex: Penguin, 1981).

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terrain that serviced "mental" functions. In localizing language, Broca accepted this view as much as anyone else.[19]

Then, in the 1870s, two German researchers, Gustav Fritsch and Eduard Hitzig, demonstrated that the cerebral cortex also plays a role in sensory-motor activity. Applying electrical currents to the brains of dogs, the two Germans were able to produce crude movements of the body, and found moreover that specific brain regions seemed responsible for specific movements.[20] Now, if the cortex possessed "motor centers," as Fritsch and Hitzig's work suggested, then it was logical to suppose, by analogy with the workings of spinal and subcortical structures, that it possessed sensory centers as well. And indeed the effort to identify these cortical motor and sensory centers in laboratory animals dominated experimental physiology in the last three decades of the nineteenth century. Parts of this work would ultimately not only advance laboratory research agendas, but help lay the foundations for the rise of neurosurgery at the end of the century.[21]

But what did this kind of localization work imply for the effort to correlate mind with matter? Could it be true, as the English neurologist David Ferrier said in 1874, that "mental operations in the last analysis must be merely the subjective side of sensory-motor substrata?"[22] In the 1870s, a young German psychiatrist named Carl Wernicke attempted an answer to this question in a way that also explicitly gestured back to the biophysicists' dream of creating an explanatory language for mind and brain that looked ultimately to the explanatory languages of the physical sciences for its orientation.

This is how it all worked. Using the anatomy of sensory-motor "projections" established in the anatomy lab by his teacher Theodor Meynert, Wernicke envisioned a cortex in which the back was specialized for processing and storing sensory data, and the front consisted of motor projections and centers. Within this schema, the form of language loss associated with "Broca's area" was reconceptualized as a "motor" deficit, while Wernicke posited a more fundamental, sensory basis for language comprehension and generation in the (posterior) temporal region of the brain. Language and rational thought were generated within this brain through hypothesized physicalist processes, whose varied forms of breakdown could be charted using paper and pencil. Sensory-motor centers were supposed to communicate

[19] For a good introduction to the intellectual issues at stake in the reflex story, see Clarke and Jacyna, *Nineteenth-Century Origins of Neuroscientific Concepts*. For an analysis of the place of reflex theory within a larger set of culturally resonant debates about control, inhibition, and regulation, see Roger Smith, *Inhibition: History and Meaning in the Sciences of Mind and Brain* (Berkeley: University of California Press, 1992).

[20] G. Fritsch and E. Hitzig *Über die elektrische Erregbarkeit des Grosshirns* [1870]. English translation in *Some Papers on the Cerebral Cortex*, trans. Gerhardt von Bonin (Springfield, Ill.: Charles C. Thomas, 1960).

[21] D. Rioch, "David Ferrier," in *Founders of Neurology*, ed. W. Haymaker and F. Schiller, 2nd ed. (Springfield, Ill.: Charles C. Thomas, 1970), pp. 195–8.

[22] David Ferrier, *The Functions of the Brain* [1876] (London: Dawsons of Pall Mall, 1966).

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with one another along "association fibers," exchanging "impressions" like so many electrical pulses along a telegraph line and combining in accordance with the established psychological "laws of association." For a new generation, this way of thinking about the brain – parsimonious, monistic, and predictive – would feel like a coming of age. It would lay the foundations for asylum-based research into a whole slew of newly conceived discrete brain disorders (the aphasias, the agnosias, the apraxias), an effort that old-timers would later nostalgically remember as a "golden era" in the history of clinical exploration of higher brain function.

## ELECTRICITY, ENERGY, AND THE NERVOUS SYSTEM FROM GALVANI TO SHERRINGTON

Nevertheless, at the beginning of the twentieth century, the Spanish neuroanatomist Santiago Ramon y Cajal recognized a fundamental shortcoming in the localization theories of his time: "However excellent, every physiological doctrine of the brain based on localizations leaves us absolutely in the dark over the detailed mechanisms of the psychological acts." Cajal's histological work identifying different types of nerve cells and the geography of their connections would, on the one hand, take the localizationist project of "mapping" the nervous system to a new level. However, he recognized that knowledge of the intricate anatomy he was untangling needed to be accompanied by an understanding of the "nature of the nervous wave, the energy transformations which it brings about or suffers at the moment when it is borne."[23]

As early as the mid-eighteenth century, confidence had been growing that the nervous force would turn out to be electrical in nature.The larger story to be told here does more than take us into the early history of what would become electrophysiology. It also opens doors for us into a series of tangled Enlightenment and Romantic era debates about the relationship between the organic and the inorganic, man and the cosmos, and brings esoteric science and popular culture into a common conversation over the efficacy and meaning of new therapeutic practices that began to circulate under the name of "animal magnetism" or mesmerism.[24]

[23] Ramon y Cajal, "Anatomical and Physiological Considerations about the Brain," in *Some Papers on the Cerebral Cortex*, ed. and trans. G. von Bonin (Springfield, Ill.: Charles C. Thomas, 1960), p. 275.

[24] For more, see Robert Darnton, *Mesmerism and the End of the Enlightenment in France* (Cambridge, Mass.: Harvard University Press, 1968); Adam Crabtree, *From Mesmer to Freud: Magnetic Sleep and the Roots of Psychological Healing* (New Haven, Conn.: Yale University Press, 1993); and portions of Alan Gauld, *A History of Hypnotism* (Cambridge: Cambridge University Press, 1992). For the later (and largely unknown) continuing history here, which locates the theme within later developments in French culture and institutionalized neurophysiology and psychiatry, see Anne Harrington, "Hysteria, Hypnosis and the Lure of the Invisible: The Rise of Neo-Mesmerism in Fin-de-Siècle French Psychiatry," in *The Anatomy of Madness: Essays in the History of Psychiatry*, vol. 3: *The Asylum and*

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For our purposes, we must leave that story aside and identify a more conventional reference point in the historical record: 1791, the year that Luigi Galvani in Italy came out in print with experiments that he believed had demonstrated that the nerves contained intrinsic electricity. In this classic work, a frog's leg was pierced and held by a brass hook through the thigh. When at rest, the foot would drop to make contact with a silver strip. On contact, a current was created, causing the leg muscles to contract and the foot to lift. This broke the current, causing the leg to drop again to the silver strip.

Galvani's interpretation of the meaning of this experiment was challenged by his Italian colleague Alessandro Volta. Volta felt that Galvani had not demonstrated the existence of an inherent animal electricity but merely revealed the possibility of creating an electric current between dissimilar metals (the brass hook and silver strip) separated by a moist medium (the frog's flesh). He could produce the same kind of phenomenon, he showed, using what he called an "artificial electric organ" – disks of different metals separated by pasteboard sheets soaked in brine, or the first wet-cell battery.[25]

Galvani's work may not have been definitive, but others – again, with the Italians taking an early lead – would make the case more definitively. Then, in the 1840s, du Bois-Reymond clinched the case with his work illustrating "negative variation" in the nerve: changes in electric potential that generated a constant current following nerve stimulation. Du Bois-Reymond's contemporary von Helmholtz then went on to measure the speed of neural electrical conduction and found it surprisingly slow – a mere eighty-five miles per hour.[26] Not only had the nervous energy been domesticated inside the conceptual categories and experimental apparatus of nineteenth-century physics; it was looking positively tame.

Meanwhile, conceptualization of the matter of the cellular architecture of the nervous system was growing through the assiduous work of histologists. Camillo Golgi in the 1870s used silver staining to visualize nerve cells at newly high levels of definition, and he felt the evidence argued for a nervous system that functioned as a continuous network (the "reticular theory"). But Cajal, working at around the same time, disagreed. He thought the microscopic evidence showed that nerve cells were not linked but rather were discrete entities that communicated with one another by some yet to be determined process (the "neuronal theory"). Cajal's view would win the day, and it would provide a foundation for relating the anatomy and physiology of the nervous

*Its Psychiatry*, ed. W. F. Bynum, R. Porter, and M. Shepherd (London: Tavistock Press, 1988), pp. 226–46.

[25] Marcello Pera, *The Ambiguous Frog: The Galvani-Volta Controversy on Animal Electricity*, trans. Jonathan Mandelbaum (Princeton, N.J.: Princeton University Press, 1992).

[26] Anson Rabinbach, *The Human Motor: Energy, Fatigue, and the Origins of Modernity* (Berkeley: University of California Press, 1992), pp. 66, 93.

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system in new, more integrated ways. Suddenly, one could begin to see how electrical messages passing through the physical architecture of the nervous system might be purposefully directed, diverted, inhibited, and augmented at different neuronal junctions, like a train having its course set and reset at various railroad switch points.[27]

The potential of neuronal theory began to be realized early in the twentieth century with the work of physiologist Charles Sherrington. Working with dogs, Sherrington aimed to map the complex pathway taken by an electrical nerve impulse as it moved from a sensory receptor on the periphery (in this case, a tactile receptor on the skin) into the spinal cord and brain, and back out over a motor pathway to produce a response (scratching). These studies led him to a way of thinking that emphasized how reflex action at one level of the nervous system could modify (stimulate or inhibit) reflex action at another level.[28] These processes were understood to result from interactions between electrical impulses and modulatory chemical signals emitted at individual nerve junctions (that Sherrington named "synapses").

During these same years, in Russia, physiologist Ivan Petrovich Pavlov (1849–1936) built on these new physiologically grounded understandings of reflex in another way, highlighting a crucial distinction between what came to be called *unconditioned* reflex actions and *conditioned* reflex actions (dogs salivating in the presence of meat powder versus dogs salivating when they hear a bell that had previously been merely *paired* with meat powder). This work helped set the stage for the emergence of behaviorist approaches in Anglo-American and Russian psychology during the early years of the twentieth century – approaches that, ironically enough, would largely eliminate considerations of brain and biology from the experimental picture in order to focus on clarifying strategies of prediction and control of behavior.[29]

Yet back in England, surveying the results of a lifetime of physiological work, Sherrington had concluded that none of these new understandings of low-level nervous functioning – to which he had so fundamentally contributed – had anything to say about high-level processes such as mind and consciousness. These, he insisted – to the dismay of at least some of his colleagues – had a soul-like reality that transcended the physical. It was evident that, even in the twentieth century, data from the clinic and laboratory alone

[27] Santiago Ramon y Cajal, *Neuron Theory or Reticular Theory? Objective Evidence of the Anatomical Unity of Nerve Cells*, trans. M. Ubeda Purkiss and Clement A. Fox (Madrid: Consejo Superior de Investigaciones Cientificas, Instituto Ramon y Cajal, 1954).

[28] Roger Smith has impressively explored the broader cultural and semantic field within which concepts of inhibition were developed and played out in physiology, psychiatry, and elsewhere and discusses Sherrington's work in that context. See Roger Smith, *Inhibition: History and Meaning in the Sciences of Mind and Brain* (Berkeley: University of California Press, 1992).

[29] Robert A. Boakes, *From Darwin to Behaviourism: Psychology and the Minds of Animals* (Cambridge: Cambridge University Press, 1984); John A. Mills, *Control: A History of Behavioral Psychology* (New York: New York University Press, 1998).

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were not going to be sufficient to resolve ongoing debates about the final nature of our humanness.[30]

## HAUNTED BY OUR PAST: THE BRAIN IN EVOLUTIONARY TIME

If studies of the brain in the late nineteenth century served as one important lightning rod for debates about our nature and fate as human beings, their importance certainly would be matched, if not bested, by the new evolutionary ideas associated with Charles Darwin. But how did the two traditions interact? Alfred Russel Wallace – cofounder with Darwin of the theory of evolution by natural selection – introduced a note of tension into the relationship early on by suggesting that, in fact, the human brain represented a dilemma for the new evolutionary theory because it was capable – even in "savages" – of far greater feats of intellectual prowess and acts of ethical refinement than would have been required for mere survival. It was therefore difficult to see how it could be a product of mere natural selection. It was as if, instead, the brain had been "prepared" in advance (perhaps by an "Overruling Intelligence") in such a way as to enable the subsequent flowering of human civilization. Charles Darwin's own comments on Wallace's actions here deserve to be recalled: I hope, he told his friend, that you have not "murdered yours and my child" too completely.[31]

More consistent with the secular, anticlerical temper of the day was the virtuoso 1874 lecture by Thomas Henry Huxley, "On the Hypothesis that Animals are Automata and Its History," which brought together reflex theory and evolutionary theory to argue for a shockingly modern metaphysics of mind–body relations. This "conscious automata" theory denied any efficacious place for consciousness or "free will" in human life. The view here was that consciousness simply accompanies us in our lives like "the steam-whistle which accompanies the work of a locomotive engine."[32]

[30] Charles Scott Sherrington, *Integrative Action of the Nervous System* [1906], 2nd ed. (New Haven, Conn.: Yale University Press, 1961); Charles Scott Sherrington, *Man on His Nature* [1940], Gifford Lectures (Cambridge: Cambridge University Press, 1951).

[31] Alfred Russel Wallace, *The Limits of Natural Selection as Applied to Man* [1870], reprinted in Alfred Russel Wallace, *Contributions to the Theory of Natural Selection* (London: Macmillan, 1875), pp. 332–72. For a sympathetic contextualizing of Wallace's story, see Loren Eiseley, *Darwin's Century: Evolution and the Men Who Discovered It* (Garden City, N.Y.: Doubleday, 1958). The quotation from Darwin was also cited from this source.

[32] Thomas Huxley, "On the Hypothesis that Animals are Automata and Its History," *Fortnightly Review*, 22 (1874), 199–245, quotation at p. 236. For a general orientation to the story of evolutionary approaches to the human mind, see Robert J. Richards, *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior* (Chicago: University of Chicago Press, 1987). On ideological relations between evolutionary theory and brain science, see Robert Young, "The Historiographic and Ideological Contexts of the 19th-Century Debate on Man's Place in Nature," in *Darwin's Metaphor: Nature's Place in Victorian Culture*, ed. Robert Young (Cambridge: Cambridge University Press, 1985), pp. 164–71, 219–47.

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Meanwhile, in some quarters, the following question began to be asked: How could one begin to orient the empirical projects of the brain sciences to do better justice to the fact that the brain, too, is not just an *object* in *space* but an evolved *process* in *time* – a four-dimensional entity? A way of thinking about this problem would ultimately be found in an image of *hierarchy*. The British neurologist John Hughlings Jackson in the 1870s had been among the first to articulate clearly the idea that different levels of the brain might serve as a kind of archaeological record of the biological history of a species, with lower and higher levels corresponding to earlier and later phases of evolutionary development.[33]

But that was not all. Jackson's temporal view of brain functioning was also predicated on the assumption that more recently evolved layers of function – in humans, associated with rational thought and moral control – were the most vulnerable ones. This meant that, in cases of shock or damage, the more refined layers broke down first, and one was then witness to a welling up of the suddenly unmasked primitive levels of brain functioning. "Dissolution" was Hughlings Jackson's term for this cascading down the nervous system to more primitive automatic and emotional states of functioning. In an era of growing social unrest, this was a model of brain functioning destined to embed itself in larger political concerns of the day. When Jackson's colleague Henry Maudsley imagined the unregulated "lower centres" of the brain to be "like the turbulent, aimless action of a democracy without a head," he was only one of many to worry that outbursts of animalistic physiology might account for everything from street riots to crimes of passion.

In psychiatric asylums, ideas like these came to serve as important resources for a renewed effort to see madness as a medical disorder with a biological underpinning, and thereby to reassert the status of asylum psychiatry as a medical science, when it had been increasingly denigrated since the late nineteenth century as a mere custodial profession. What Shorter calls the era of the "first biological psychiatry"[34] had a strong hereditarian orientation that came in a distinctively fatalistic flavor – biology was destiny, as the materialists had long insisted, but *sick* biology, "degenerate" biology, was perhaps especially so. It was not really until the 1940s that biological psychiatry would start to be identified with a slew of biological *interventions*, from shock treatment to surgery,[35] and not until the 1960s that the current identification of biological psychiatry with pharmaceutical interventions would begin to take hold.

It is true that by the second decade of the twentieth century, especially in the United States, optimistic social engineering programs would join forces

33 The best single introduction to Jackson's thought is John Hughlings Jackson, *Selected Writings of John Hughlings Jackson*, ed. J. Taylor, 2 vols. (London: Hodder and Stoughton, 1932).

34 Edward Shorter, *A History of Psychiatry: From the Era of the Asylum to the Age of Prozac* (New York: Wiley, 1997).

35 Elliot S. Valenstein, *Great and Desperate Cures: The Rise and Decline of Psychosurgery and other Radical Treatments for Mental Illness* (New York: Basic Books, 1986).

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both with behaviorist thinking in psychology and with an Americanized interpretation of psychoanalysis to make a strong counterargument for the capacity of proper socialization and education to ameliorate human vulnerabilities (the "mental hygiene" movement). Nevertheless, even within this new cultural setting, the older Darwinian-inspired image of mind as an unstable struggle between "higher" and "lower" levels would persist in covert ways. It would be incorporated, for example, into the psychoanalytic concept of "regression" and serve as the rationale for the psychoanalytic distinction between primary and secondary mental processes, expressed by Freud himself in the vivid image of the conscious, rational ego struggling to maintain some sort of check over the unconscious, passion-driven "id."[36]

Back in the more esoteric world of university laboratory research, the basic vision of a "higher" mind functioning as an inhibitory force over the "animal" below would continue to leave its imprint on emerging mid-century understandings of the brain. A high-profile laboratory program headed by John Fulton at Yale University studied hierarchical processes of inhibition and disinhibition in the brain, all conceptualized within an evolutionary framework. Building on the work of anatomist James Papez, one of the members of this laboratory team at Yale, physician and physiologist Paul MacLean, conceptualized a system of integrated subcortical brain structures that he felt acted as the "emotional" center of the brain – mediating survival-enhancing behavior, including drives to mate and care for one's young, and acting in other respects very much like a Freudian instinct-driven unconscious. MacLean ultimately called this system the "limbic system."[37]

Evolutionary thinking shaped brain science thinking in a somewhat different way with the work of Harvard psychophysiologist Walter Bradford Cannon on the role of the sympathetic-adrenal system in the arousal processes associated with the "fight or flight" emotions (especially rage and fear). Cannon saw this part of the nervous system as one half of a regulatory system (the other half would be called the "parasympathetic system") involved in maintaining a state of responsive balance or "homeostasis" in the organism as a whole. Beginning in the late 1950s, the Cannon "fight or flight" model would be pressed into service as an organizing framework for a remarkably complex tangle of science, clinical practice, and cultural moralizing about a new psychophysiological experience called "stress" – now discovered in everything

[36] Sigmund Freud, *The Ego and the Id* [1927], trans. Joan Riviere (London: Hogarth Press, 1949).

[37] See Walter B. Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage* [1919], 2nd ed. (1929); J. W. Papez, "A Proposed Mechanism of Emotion," *Archives of Neurology and Psychiatry*, 33 (1937), 725–43; Paul MacLean, "Psychosomatic Disease and the 'Visceral' Brain: Recent Developments Bearing on the Papez Theory of Emotion," *Psychosomatic Medicine*, 11 (1949), 338–53; Paul MacLean, "Man's Reptilian and Limbic Inheritance," in *A Triune Concept of the Brain and Behavior: The Hincks Memorial Lectures*, ed. T. Boag and D. Campbell (Toronto: University of Toronto Press, 1973), pp. 6–22. A useful overview of the basic issues here is also provided in John Durant, "The Science of Sentiment: The Problem of the Cerebral Localization of Emotion," in *Perspectives in Ethology*, vol. 6: *Mechanisms*, ed. P. P. G. Bateson and P. H. Klopfer (New York: Plenum Press, 1985), pp. 1–31.

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from monkeys who developed ulcers in laboratory settings, to "Type A" executives in corporate boardrooms, ripe for heart attacks.[38]

## THE SUBJECT STRIKES BACK: HYSTERIA AND HOLISM

Even as everything seemed to be going well for the expansionist ambitions of the brain sciences, there were also some growing cracks in the larger citadel. The "subject," who was to be domesticated within the current conceptual categories of brain anatomy and physiology, was in a range of ways refusing to lie down and behave the way she was supposed to.

Space permits us to do no more than gesture here in a couple of relevant directions. The first of these takes us back to the last decades of the nineteenth century, to a time when Europe's leading neurologist, Jean-Martin Charcot, had resolved to bring the conceptual categories and clinical methodologies of neurology to elucidate the physiological logic of one of his era's most baffling disorders: hysteria. At first, everything seemed to go well – even brilliantly. Order began to emerge out of chaos. Symptoms were cataloged, and physiological "laws" were described. Photographs were made of patients to provide the evidence Charcot needed to prove – as he put it – that the laws of hysteria that he had discovered were "valid for all countries, all times, all races" and "consequently universal."

But, as things unfolded, it turned out that this was a physiology whose laws, far from being "universal," were in the end so local that they basically only unfolded inside the walls of Charcot's asylum, the Salpêtrière. Using hypnosis (which Charcot had also helped rehabilitate), rivals of Charcot showed that one could reproduce all the symptoms of hysteria, and one could also change them or make them disappear. As this came out little by little, Charcot became a target of ridicule, and his disciples scattered. The entire neurological edifice of hysteria, rooted in the visible, the objective, the universal, slowly crumbled – all of its contours now chalked up to some invisible and obscure psychological process that people were beginning to call "suggestion."

In the space of confusion and humiliation that opened up here, people such as Freud came in and reinterpreted hysteria not as a disease of the "brain" but as a disease of the "mind." And out of this moment of choice one sees the rise of a new kind of Cartesian logic that would get variously institutionalized and elaborated through such twentieth-century distinctions as "neuroses" versus "psychoses," "psychiatry" versus "neurology," "talking therapies" versus "drugs," and somatic disorders that are "all in your head" versus

[38] Robert Kugelmann, *Stress: The Nature and History of Engineered Grief* (New York: Praeger, 1992); Allan Young, *The Harmony of Illusions: Inventing Post-traumatic Stress Disorder* (Princeton, N.J.: Princeton University Press, 1995); Harris Dienstfrey, *Where the Mind Meets the Body* (New York: Harper Perennial, 1991).

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somatic disorders that are "real." We are still living today with the legacy of those institutionalized metaphysical sortings. Nowhere is this more clearly seen than in our current approaches to managing what is called "the placebo effect." We are so convinced of the power and ubiquity of this phenomenon that we require all new drugs to be tested against dummy versions of themselves; at the same time, we are committed to seeing all placebo effects as "imaginary" or "unreal."[39]

At about this same time, other kinds of discontents were afoot in the neurology clinic. Particularly in the German-speaking countries, evidence was being mobilized against the diagnostically useful model of mind and brain functioning laid down by Wernicke and his generation. Much of the energy fueling the opposition drew on the anomalies and challenges raised for Wernicke's model by the problem of "recovery" – the evidence for the brain's capacity to heal itself. Increasingly, it would be said that the simple fact that brain-damaged people could get better over time, could regain lost speech and movement, was simply incompatible with the nineteenth-century "machine" model of the nervous system as a purely mechanical apparatus operating according to fixed laws of reflex and association. The fighting words were spoken: Machines did not repair themselves after suffering damage, and functions that "resided" in certain fixed regions of the brain could not reappear if those brain regions had been permanently destroyed. For this reason, and others, it had become clear that human beings were actually "more than machines" – enlightened or otherwise (pace la Mettrie) – and the brain and behavioral sciences of the future (these rebellious voices from the clinic declared) were going to have to take into account all the ways in which this was so.[40]

The 1920s began also to see laboratory-based challenges to the prevailing view of the cortex as a hard-wired structure in which highly determined nerve connections and brain areas served specific functions. The failure of the American psychophysiologist Karl Lashley to find any specific site in the rat cortex where the memory ("engram") of a learned behavior could be localized helped usher in a "new view" of the cortex dominated by principles of functional "equipotentiality" and "mass action." In the 1930s, work on amphibians by Paul Weiss further suggested that when nerve centers to limbs were cut and rearranged, orderly coordination could nevertheless be reestablished. The brain in these years (in part also for reasons that have to do

[39] A comprehensive historiographical introduction to this and other cuts through the hysteria story can be found in Mark Micale, *Approaching Hysteria: Disease and Its Interpretations* (Princeton, N.J.: Princeton University Press, 1995). On placebos, see Anne Harrington, ed., *The Placebo Effect: An Interdisciplinary Exploration* (Cambridge, Mass.: Harvard University Press, 1997).

[40] For an extended discussion of this theme, see Anne Harrington, *Reenchanted Science: Holism in German Culture from Wilhelm II to Hitler* (Princeton, N.J.: Princeton University Press, 1996); Anne Harrington, "Kurt Goldstein's Neurology of Healing and Wholeness: A Weimar Story," in *Greater than the Sum of Its Parts: Holistic Biomedicine in the Twentieth Century*, ed. George Weisz and Christopher Lawrence (Cambridge: Cambridge University Press, 1998).

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with political and cultural congeniality) appeared to be a marvelously plastic structure. Not biology but the environment – from family life to laboratory conditioning – appeared to "call the shots" in the life of a mind and brain.[41]

## TECHNOLOGICAL IMPERATIVES AND THE MAKING OF "NEUROSCIENCE"

That environmentalist perspective would only begin to change in the late 1950s when new projects in both the laboratory and the clinic began to argue for the relative *incapacity* of the brain to rewire itself after damage, and the extent to which specific functions *did* have a hard-wired "place" in the cortex. New technologies, new experimental paradigms, and renewed cultural openness to interpreting ambiguous data all probably contributed to this swing back toward a kind of biological determinism. One complex expression in the 1970s was the explosion of interest in so-called split-brain research and lateralized hemisphere functioning. California psychologist Roger Sperry and his colleagues had first studied epileptic patients in whom connections between the cerebral hemispheres had been severed for therapeutic reasons. It appeared that each severed hemisphere possessed a more or less independent sphere of consciousness – often the left brain literally did not know what the right was doing. Moreover, the two hemispheres responded to the environment and computed information differently: The left hemisphere was specialized for language and (some began to argue) for analytic, piecemeal thinking in general; the right hemisphere was specialized for visual-spatial information processing and (it was argued) "holistic" (creative, artistic) thinking in general. These studies not only stimulated new kinds of research into higher brain function; they also produced a (perhaps peculiarly American) cultural dialogue on the relative virtues of what was called "left brain" versus "right brain" thinking.[42]

Otherwise in the postwar era, technological innovation would soon drive research at least as much as theoretical preoccupation. For example, with the development of the microelectrode in the 1940s, much basic neurobiological research went to the cellular level. In the 1960s, Harvard researchers David Hubel and Torsten Wiesel used microelectrodes to record activity in single nerve cells across the cellular columns of the primary visual area of the cortex (the anatomy of which had been worked out by Johns Hopkins neuroanatomist Vernon Mountcastle). They stunned the research community

[41] Karl S. Lashley, *Brain Mechanisms and Intelligence* (Chicago: University of Chicago Press, 1929). For more on this era, see various autobiographical essays in Frederic G. Worden, Judith P. Swazey, and George Adelman, *The Neurosciences: Paths of Discovery* (Boston: Birkhäuser, 1975).

[42] For a useful overview of this literature and these events, see Sally Springer and Georg Deutsch, *Left Brain, Right Brain* (San Francisco: W. H. Freeman, 1993); Anne Harrington and G. Oepen, "'Whole brain' Politics and Brain Laterality Research," *Archives of European Neurology*, 239 (1989), 141–3.

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with their conclusions that different individual cells "saw" differently or, more precisely, had different built-in capacities to respond to visual stimuli – what they called "pattern specificity." In other words, it seemed that the specific instructions by which the brain came to know the world were written as far down as the individual cell level.[43]

Beginning in the late 1980s, the dominant molecular focus in basic neurobiological research would begin to be partly overshadowed by excitement over new neuroimaging technologies that promised insights into the contributions made by specific neural structures to more global brain functioning. In the 1940s, Seymour Kety had used nitrous oxide to track changes in cerebral blood flow, suggesting that there might be ways to watch the "living brain" in action. This work was one step in a chain of technological developments that ultimately led to the anatomical views created by computer tomography (CT) and the dramatic colored brain pictures produced by positron emission technology (PET), and more recently by functional magnetic resonance imaging (fMRI). Slowly, a new sort of celebratory rhetoric, peppered with "final frontier" imagery, spread across the disciplinary culture of brain science. In the end, the secrets of mind and brain would be resolved, not through philosophical subtleties, but through new technological devices that would allow us to go and see where no man (or woman) had gone and seen before.[44]

Today, most brain and behavior science research is still sustained by a commitment to playing for technological high stakes and a pride in its own forward-looking identity. Brain science has "the future in its bones" (to recall the famous line of C. P. Snow),[45] and it knows it. Nevertheless – more than it often likes to admit – the living flesh and blood of its practices and thinking remain fed by its discipline-divided and ethically contentious past. Despite the high hopes of multidisciplinary integration envisioned in the 1960s by Francis Schmitt's Neurosciences Research Project (NRP) – which led to the coinage of this new word "neuroscience" – all the new projects and understandings do not map seamlessly onto one another. For example, updated notions of hard-wired localization coexist with models of the nervous system as a self-updating dynamic system of "neural nets" (work associated with such names as Gerald Edelman).[46] Models of mind developed within the sanitized walls of computer science (so-called artificial intelligence) juggle uneasily against models of mind thrashed out in the less regulated worlds of primatology research and biological anthropology. Studies of the neurochemistry of the nervous system – including the discovery in the 1970s of the

[43] David H. Hubel, *Eye, Brain, and Vision* (New York: Scientific American Library, distributed by W. H. Freeman, 1988).

[44] Roger E. Kelley, ed., *Functional Neuroimaging* (Armonk, N.Y.: Futura, 1994).

[45] Charles Percy Snow, *The Two Cultures* [original title: *Two Cultures and the Scientific Revolution*], introduction by Stefan Collini (Cambridge: Cambridge University Press, 1993).

[46] Gerald M. Edelman, *Neural Darwinism: The Theory of Neuronal Group Selection* (New York: Basic Books, 1987).

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endorphins, the brain's "natural opiates" (by Solomon Snyder and Candace Pert)[47] – have led some to question the extent to which the nervous system can even be properly said to exist as an independent entity. Perhaps instead it will need to be reconceived as part of a more complex system of interconnected biochemical processes, including those that regulate endocrine and immune functions. In this last vision, the "mind" emerges, not just as a product of the brain, but in some sense of the entire human organism.

At the same time, ongoing political debates over possible brain-based determinants of sexual orientation, violence, intelligence, and supposed mental disorders (from depression to attention-deficit disorder) suggest that the moving horizon of brain and mind research will continue to be drawn into our society's changing political and cultural imperatives and preoccupations. Today, as in the past, our questions about what we think it means to be human, and all the ways we think science can help us answer that question, simply feel too urgent for us to keep them separate from – even if we want to or think we should – our human lives, part of some imagined domesticated world of disinterested inquiry alone.

[47] Solomon H. Snyder, *Brainstorming*: *The Science and Politics of Opiate Research* (Cambridge, Mass.: Harvard University Press, 1989).

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# 28

# HISTORY OF BIOTECHNOLOGY

*Robert Bud*

For almost a century, entrepreneurs, policymakers and scientists have used the word biotechnology to describe imminent revolutions based on the application of biology.[1] Yet although novel clusters of techniques, products, and promises were clearly momentous to visionaries, they repeatedly failed to achieve their foreseen potential.

The old frustration seemed to have been overcome in 1980 when the U.S. Supreme Court permitted the patenting of a transgenic bacterium that could consume oil spilled at sea. Many were enthused by the new development, and foreign governments felt that this was an American challenge they could not afford to duck. Although a few quaked before this new appropriation of science, the majority of commentators assumed that finally the subdivision and exploitation of the world of primitive living beings was about to begin.[2] The possibility of patenting new organisms made by means of modern biological techniques and, in particular, the methods of recombinant DNA that had first been developed in the early 1970s would, it seemed, open up hitherto undreamed of possibilities. Rather than relying on traditional breeding, which entailed combining genes of animals and plants within the same species, genes could now be combined from across the entire spectrum of living organisms. At this moment, when an oil crisis suggested that old energy-intensive industries had had their day, and the success of electronics had demonstrated the possibility of a new industrial revolution, every major country created its own biotechnology plan.[3]

[1] The treatment here is based on Robert Bud, *Uses of Life: A History of Biotechnology* (Cambridge: Cambridge University Press, 1994). Where no other source is given, that may be the most useful starting point.

[2] Daniel J. Kevles, "*Diamond v. Chackrabarty* and Beyond: The Political Economy of Patenting Life," in *Private Science: Biotechnology and the Rise of the Biomolecular Sciences*, ed. Arnold Thackray (Philadelphia: University of Pennsylvania Press, 1998), pp. 65–79.

[3] Margaret Sharp, *The New Biotechnology: European Governments in Search of a Strategy*, Sussex European Papers no. 15 (Brighton: Science Policy Research Unit, 1985).



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Even in recent years, the scientific and technological content and focus of biotechnology have changed significantly. When the techniques of recombinant DNA were first deployed, scientists and entrepreneurs anticipated that they could use genetically engineered organisms to make therapeutic proteins that would compensate for genetically induced deficiencies. More recently, attention has shifted to the human genome, modified genes, and genetically engineered crops, and now to cloning and the use of stem cells.

Biotechnology is characterized by an approach to biology and technology rather than by any particular methods. In 1981, the Organization for Economic Cooperation and Development (OECD) provided the following definition: "the application of scientific and engineering principles to the processing of materials by biological agents to provide goods and services."[4] Uncertainty over whether biotechnology is more like a science or a technology has confused chroniclers, particularly because the conventions for the histories of sciences and those for technologies are rather different. Accounts of the first have tended to be about knowledge and understanding, whereas the tradition in the history of technology has been to focus on practice and economic consequences. The space between fundamental science and technology, occupied by subjects such as biotechnology, has been found confusing.

Biotechnology is often held to be an "applied science," but there has been an enduring uncertainty as to what this means. Some have held that applied science is the application of pure science, whereas others hold that applied science is an activity in itself.[5] The French founder of microbiology, Louis Pasteur, famously proclaimed in a much quoted quip, "There are no applied sciences . . . there are only . . . the applications of science." By contrast, his contemporaries developing thermodynamics framed their science, so Timothy Lenoir has argued, to make it useful toward applications such as Fritz Haber's synthesis of ammonia from the gases hydrogen and nitrogen in 1909. Through much of the twentieth century, the nature of applied science was explored – without resolution – through evaluations of the engineering curriculum. To what extent should engineering itself be taught, and to what extent was it seen as the application of fundamental principles?[6]

During the 1960s, the relationship between science and technology was widely debated as the growth of science funding slowed and previous optimism that the two were intimately interconnected seemed to be misplaced.

[4] Allan T. Bull, Geoffrey Holt, and Malcolm D. Lilly, *Biotechnology: International Trends and Perspectives* (Paris: OECD, 1982).

[5] Robert Bud and Gerrylynn K. Roberts, *Science versus Practice: Chemistry in Victorian Britain* (Manchester: Manchester University Press, 1984). See also Thackray, *Private Science.*

[6] The epigram of Pasteur is cited in René Dubos, *Louis Pasteur: Freelance of Science* (New York: Scribner, 1976), pp. 67–8. For chemistry as an applied science, see Timothy Lenoir, *Instituting Science: The Cultural Production of Scientific Knowledge* (Stanford, Calif.: Stanford University Press, 1997).

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Since then, meticulous studies of particular industrial research laboratories, relations between scientists and military sponsors, and industrial networks around universities such as Wisconsin and Stanford have enriched concepts of science–technology relations. Scholars studying the uses and development of instrumentation have been able to identify more complex interactions; devices such as cell counters and gene sequencers have brought the language of automation to biochemists as mass processing of enormous numbers of samples has become possible. Moreover, techniques have traveled between apparently industrial devices and scientific instruments, so, for example, the design of the inkjet printer has made possible the fluorescent activated cell sorter. The word "technoscience" has become a popular indicator of the reduced distance between pure science, applied science, and technology.[7]

Nonetheless, the partners who have made and remade biotechnology have remained self-conscious about their allegiance to science or technology. The recent sequencing of the human genome offers a case in point. Two teams have produced similar outputs but have held quite different models of their activity. One, based at a corporation, Celera, was funded privately for the purpose of privately selling knowledge like any other product. The other, publicly funded, was based on the model of publicly accessible scientific knowledge.[8]

In the early 1980s, biotechnology seemed distinctively based on the contemporary science of molecular biology. Accordingly, it was given the name "new biotechnology" to distinguish it from anything more familiar, which was dismissed as the "old biotechnology." This distinction was made in *Commercial Biotechnology*, an important 1984 report from the United States Office of Technology Assessment.[9] Biotechnology so defined as "new" could not have a past or even a history. Even when cursory reference was made to Gregor Mendel's founding of genetics, typically the founding event of biotechnology was cited as the 1953 discovery of the double helix by Francis Crick

[7] David Noble, *America by Design: Science, Technology, and the Rise of Corporate Capitalism* (New York: Knopf, 1977). On industrial networks with research universities, see John P. Swann, *Academic Scientists and the Pharmaceutical Industry: Cooperative Research in Twentieth-Century America* (Baltimore: Johns Hopkins University Press, 1988); Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993). On technoscience, see J. V. Pickstone, *Ways of Knowing: A New History of Science, Technology and Medicine* (Manchester: Manchester University Press, 2000). On debates over whether there has been a fundamental change, see Terry Shinn, "Change or Mutation? Reflection on the Foundations of Contemporary Science," *Social Science Information*, 38 (1999), 149–76; Terry Shinn and Bernard Joerges, "The Transverse Science and Technology Culture: Dynamics and Roles of Research-Technology," *Social Science Information*, 41 (2002), 207–51; Michael Gibbons, Camille Limoges, Helga Nowotny, Simon Schwartzman, Peter Scott, and Martin Trow, *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies* (London: Sage, 1994); Peter Weingart, "From Finalization to 'Mode 2'; Old Wine in New Bottles," *Social Science Information*, 36 (1997), 591–613.

[8] John Sulston and Georgina Ferry, *The Common Thread, A Story of Science, Politics, Ethics and the Human Genome* (London: Bantam, 2002).

[9] U.S. Congress, Office of Technology Assessment, *Commercial Biotechnology, An International Analysis*, OTA-BA-218 (Washington, D.C.: U.S. Government Printing Office, 1984).

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(1916–2004) and James D. Watson (b. 1928).[10] This was treated not as a historical event but as one of the key moments of the present – part of an ongoing and essentially contemporary discussion between scientists and entrepreneurs; most practitioners were trained within one or two academic generations of the founding act.

Subsequent key dates in the cognitive development of molecular biology with significance for biotechnology included the 1961 breaking of the genetic code by Marshall Nirenberg and Heinrich Matthei, who showed how RNA (ribonucleic acid) codes for proteins, the synthesis of short stretches of DNA (deoxyribonucleic acid) by Arthur Kornberg in 1967, the 1971 development by Paul Berg of enzymes that could precisely cut DNA, and the work of Stanley Cohen and Herbert Boyer published two years later that enabled the transfer of a section of DNA from one organism to another. During the late 1970s, genes for producing human growth hormone and insulin were transferred to the bacterium *Escherichia coli*, and subsequently other proteins such as the proposed anticancer drug interferon and a blood anticlotting factor were also made in a similar way. In 1988, Harvard University obtained a patent on a whole genetically engineered mammal. Harvard professor Philip Leder had transferred a gene not just from one cell of *E. coli* to another but from a virus to a mouse to produce a novel organism.[11] The science of cloning was also linked to mammalian models when the genetically engineered sheep Dolly was produced from the cells of adult sheep in 1997.[12] The implications of the draft sequence of the human genome announced in the millennium year 2000 are still emerging. Meanwhile, the genomes of bacteria and other disease organisms are being explored.

There were also political and regulatory links between the science of molecular biology and the practice of biotechnology. The anxieties in the early 1970s about the release of dangerous organisms within or even outside laboratories were so great that they led first to a moratorium and then to controls that affected academic scientists and industry alike (even if by custom rather than regulation).[13] The moratorium was called for by the leaders of the research themselves, anxious not to repeat the heedless progressivism of their physicist forebears, who, without proper controls on the use of their work, had plunged into the development of the atomic bomb and incurred public opprobrium. Those concerned with not overly controlling the practice of

[10] Soraya de Chadaravian, *Designs for Life: Molecular Biology after World War II* (Cambridge: Cambridge University Press, 2001).

[11] Donna Haraway, *Modest_Witness@Second_Millennium.FemaleMan_Meets_OncoMouse: Feminism and Technoscience* (New York: Routledge, 1997).

[12] On Dolly and cloning, see Ian Wilmut, Keith Campbell, and Colin Tudge, *The Second Creation: The Art of Biological Control by the Scientists Who Cloned Dolly* (London: Headline, 2000); Gina Kolata, *Clone: The Road to Dolly and the Path Ahead* (London: Allen Lane, 1997).

[13] Sheldon Krimsky, *Biotechnics and Society: The Rise of Industrial Genetics* (New York: Praeger, 1991); Susan Wright, *Molecular Politics: Developing American and British Regulatory Policy for Genetic Engineering, 1972–82* (Chicago: University of Chicago Press, 1994).

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molecular biology ensured that debates about the future of the science would engage with the anticipated benefits of the industry as well as its threats.[14]

An emphasis on the means of modifying cells downplayed the engineering and chemical skills needed for their cultivation and for the extraction of their delicate products. And there are linguistic as well as philosophical reasons for the balance that has been struck in various accounts. Molecular biology and biotechnology are linked particularly in English-language sources, whereas the historical literature on the technologies of the bacterium and the yeast cell are less accessible to anglophones. Whereas molecular biology research has been reported almost entirely in English, the study of fermentation and its history were dominated in the nineteenth century and much of the twentieth century by writers of German. Even today, the most substantial account of brewing technology is in German.[15]

## THE EARLY HISTORY

Traditionally the crafts of fermentation evolved slowly, and although many have had a stake in their practice, few have had an interest in change. Nonetheless, there have been pivotal places and times in which scientific interpretation and technical skills have been reintegrated to produce quite novel techniques and approaches. At the very end of the seventeenth century, the era of the scientific revolution, new skills of thermometry and hydrometry, as well as chemical theories of fermentation, were introduced into the old craft. The Prussian court physician Georg Ernst Stahl (1659–1734), pioneered the concept of a specific fermentation technology, which he called "zymotechnology" in his book *Zymotechnia Fundamentalis* (Fundamental Zymotechnology) published in 1697.[16] This influential text can be seen as the founding document of biotechnology.

Stahl's fermentation theories, based on a rigorous separation of what he saw as the living and the inert worlds, withered early in the nineteenth century after the spectacular murder of his phlogiston theory of combustion. The familiar story of *Frankenstein*, by Mary Shelley, in which a beast that is increasingly "alive" is created from dead material, is both a product of the immediate post-Stahlian age and an enduring legacy to biotechnology.[17]

[14] Robert Bud, "Biotechnological Dancers to Different Tunes: Enthusiasts, Sceptics and Regulators," in *Resistance to New Technology*, ed. Martin Bauer (Cambridge: Cambridge University Press, 1995), pp. 293–310.

[15] Mikulas Teich, *Bier, Wissenschaft und Wirtschaft in Deutschland: 1800–1914; ein Beitrag zur deutschen Industrialisierungsgeschichte* (Vienna: Böhlau, 2000).

[16] Kristoff Glamann, "The Scientific Brewer: Founders and Successors during the Rise of the Modern Brewing Industry," in *Enterprise and History: Essays in Honour of Charles Wilson*, ed. D. C. Coleman and Peter Mathias (Cambridge: Cambridge University Press, 1984), pp. 186–98. See, for instance, M. Delbrück and A. Schrohe, eds., *Hefe, Gärung und Fäulnis* (Berlin: Paul Parey, 1904); A. Schrohe, *Aus der Vergangenheit der Gärungstechnik und verwandter Gebiete* (Berlin: Paul Parey, 1917).

[17] Jon Turney, *Frankenstein's Footsteps: Science, Genetics and Popular Culture* (New Haven, Conn.: Yale University Press, 1998).

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Nonetheless, Stahl's word zymotechnology lived on in texts and institutions. In mid-nineteenth-century Prague, Carl Balling, a full professor for thirty-three years, certainly saw himself employing chemical principles but also serving the entire brewing industry. He looked to the history of his subject for inspiration, and Stahl offered him an ancestry, identity, and historic location. Balling espoused the term "Zymotechnik," entitling the fourth volume of his classic text on brewing *Bericht über die Fortschritte der zymotechnische Wissenschaften und Gewerbe (Account of the Progress of the Zymotechnic Sciences and Arts).*

The work of Louis Pasteur reinforced the distinction between living creatures and merely chemical entities, and by the end of the nineteenth century one sees attempts to develop a newly scientific study of fermentation. The centers of brewing research were not the blasted sites of the industrial revolution such as the Ruhr or industrial Lancashire; instead they were metropolises of agroindustry such as Paris, Berlin, Copenhagen, and Chicago. The last was the center of the world's greatest agricultural market; the nearby prairies supplied the world with wheat, while Chicago's production-line meat processing was the model for Henry Ford's car assembly lines. At the same time, Denmark was the world leader in creating value-added agricultural produce, pioneering industrialized methods of pig fattening and bacon, butter, and beer production. It was here that the systems of biological science and technique were integrated and reintegrated, conceptualized and reconceptualized.

The classic text of zymotechnology in the late nineteenth century was *Microorganisms and Fermentation* by Alfred Jørgensen of Copenhagen, which first appeared in Danish in 1889 and would be translated, reedited, and reissued for over sixty years. Jørgensen was a Copenhagen consultant closely associated with the Tuborg brewery. He also ran a school that attracted students from around Europe; he claimed eight hundred by 1903. So influential was Jørgensen that his "institute of fermentology" was imitated in name and function in Chicago by a Danish expatriate, Max Henius.[18] Jørgensen's magazine *Zymotkniske Tidende* was copied by Henius's Chicago competitor, the German expatriate John Ewald Siebel, who founded the *Zymotechnic Magazine. Zeitschrift für Gährungsgewerbe* and *Food and Beverage Critic*, and later the Zymotechnic Institute. In 1906, the Chicago brewing consultants created a professional club, the Zymotechnica Association.[19]

The central matter of concern was still beer, and even at the end of the century, beer sales in Germany were as valuable as those of the steel industry.

[18] We are fortunate that while Jørgensen awaits his biographer, there are biographical treatments of both Henius and Siebel. See Max Henius Memoir Committee, *Max Henius: A Biography* (Chicago: privately printed, 1936); John P. Arnold and Frank Penman, *History of the Brewing Industry and Brewing Science in America, Prepared as a Memorial to the Pioneers of American Brewing Science Dr. John E. Siebel and Anton Schwartz* (Chicago: privately printed, 1933).

[19] Still the best introduction to early twentieth-century Chicago is Upton Sinclair, *The Jungle* (New York: Jungle Publishing, 1906). Although a novel, its impact is widely credited for the 1906 Food and Drug Act.

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In the wake of France's humiliation in the Franco-Prussian War, Pasteur had published on brewing to assert his country's superiority in an industry traditionally associated with Germany. However, it was the Dane Emil Christian Hansen who discovered that infection from wild yeasts was responsible for numerous failed brews, and the application of pure yeast brewing was developed in Berlin by Max Delbrück. Although the scientists involved were closely connected across national borders, the conceptions of what was being done were different. Whereas in Paris the new study was seen as the application of microbiology, scientists in Copenhagen and Berlin saw prospects for a newly reinvigorated zymotechnology.

The science and technology of fermentation went beyond brewing. The making of cheese and yogurt, wine and vinegar, tea and tobacco, even the removal of hair from hides in the making of leather, required the control of fermentation. At the end of the century, the first chemicals began to be made: lactic acid, citric acid, and the enzyme takaminase. A conception of "zymotechnology" associated principally with the brewing of beer began to look too limited to its principal exponents, particularly those in Denmark.[20]

## FROM ZYMOTECHNICS TO BIOTECHNICS

The broadening scope of zymotechnics was recognized in 1913 in Copenhagen, when the professorship of agricultural chemistry and fermentation physiology held by Orla Jensen, a former pupil of Jørgensen, was retitled as the chair of biotechnic chemistry. Jensen's course linked treatments of proteins, enzymes, and cells with the analysis of particular foods such as milk and margarine and with chocolate manufacture. A new discipline was emerging. Jensen would explain his approach some years later in a book that addressed the nature of applied science. For him, it was not the mere application of pure science but instead a fundamental form of knowledge growing out of practical experience. Thus, Jensen argued, botany had grown out of the search for medicinal plants or chemistry from the study of minerals. Orla Jensen may not feature in the annals of the great biochemists, but by the time he returned to Denmark from Switzerland, he had optimized the conditions for producing the holes in Emmenthal cheese.

Science and technology were also brought together under the pressure of World War I. In Berlin, yeast for animal feed was grown on an enormous scale on substrates nutrified by Fritz Haber's new synthetic ammonia. In Britain, the Byelorussian Jew and future president of Israel Chaim Weizmann, having

[20] The only English-language text on the development of Danish agriculture is Eimar Jensen, *Danish Agriculture: Its Economic Development, a Description and Economic Analysis Centering on the Free Trade Epoch, 1870–1930* (Copenhagen: Schultz, 1937).

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worked with Auguste Fernbach at the Pasteur Institute, developed his own way of using bacteria to make the solvent acetone from starch. Weizmann first wanted to find a use for Palestine's low-value agricultural produce, and later he wanted to help the Allies produce smokeless gunpowder, which depended on acetone. There is, however, no truth to the story that the British Balfour Declaration, offering Palestine as a national home for the Jews, was made out of gratitude to Weizmann.[21]

After the war, Weizmann's work was the basis for the manufacture of what had hitherto been a by-product, the alcohol butanol, now the solvent for the new cellulose paints found suitable for the newly numerous automobiles.[22] The Weizmann process also provided the inspiration for other applications of what came to be called economic microbiology.[23]

It was another wartime development that inspired the coining of the word "biotechnology." Hungary was the agricultural base of the Austro-Hungarian empire and aspired to Danish levels of efficiency. The economist Karl Ereky planned to go further and build the largest pig processing factory. Into a site fattening 50,000 swine at a time would come railroad wagons of sugar beets, and out would come fat, hides, and meat. In this forerunner of the Soviet collective farm, peasants (in any case now falling prey to the temptations of urban society) would be completely superseded by the industrialization of the biological process. Ereky went further in his ruminations over the meaning of his innovation; it presaged an industrial revolution that would follow the transformation of chemical technology. In his book *Biotechnologie*, he linked specific technical injunctions to wide-ranging philosophy. After the war, Ereky would become Hungary's minister of food.

Nonetheless, it was not through Ereky's direct action that his word seems to have been picked up. Rather, his book was reviewed by the influential Paul Lindner, head of botany at the Institut für Gärungsgewerbe in Berlin, who suggested that microorganisms could also be seen as making up a biotechnological machine as in the production of yeast and the work of Weizmann, which was widely publicized at that very time. It was with this meaning that the word "*Biotechnologie*" entered German dictionaries in the 1920s. Its links to zymotechnology were particularly clear in Chicago, where a Prohibition era consultancy in nonalcoholic fermented drinks was established under the title Bureau of Bio-technology. Shortly after, in England, a fermentation consultant set up a "bureau of bio-technology." The ongoing commercial importance of such fermentation-based activities was considerable.

[21] Jehuda Reinharz, *Chaim Weizmann, the Making of a Zionist Leader* (Oxford: Oxford University Press, 1985).

[22] H. Benninga, *A History of Lactic Acid Making: A Chapter in the History of Biotechnology* (Dordrecht: Kluwer, 1990).

[23] Keith Vernon, "Pus, Sewage, Beer and Milk: Microbiology in Britain, 1870–1940," *History of Science*, 28 (1990), 289–325.

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If biotechnology had represented merely the updating of zymotechnology, then it would have been interesting but hardly the legitimate forerunner of modern enthusiasms. Since the middle of the nineteenth century, however, there had been another interpretation of the engineering of life – eugenics, associated with the "improvement" of people, collectively if not individually. It was this tradition that, as early as 1911, would first identify the twentieth century as the biological century. These two interpretations, the eugenic and the zymotechnic, would engender modern conceptions.

Today, eugenics has a bad reputation as the ideology underpinning the murder of the weak and undesired, and of the Holocaust.[24] In the early twentieth century, however, many of its proponents believed that the weak could be made strong. Unlike those who believed that the only means of improvement was by weeding out unwanted genes, some believed that humanity could be genetically improved. After all, it was widely reasoned, man through the use of technology had already progressed beyond his biological limits.

As early as 1828, a French pupil of Jean-Baptiste Lamarck, Jean-Jacques Virey, had coined the term "*biotechnie*" to describe man's ability to make technology do the work of biology.[25] Charles Darwin's contemporary and co-father of evolution theory, Alfred Russel Wallace, saw tool building as the human route to further evolution. Several biological thinkers thought that further human improvement would stem from the proper integration of the biological and the technological. Having seen poverty, poor nutrition, ill health, and failed pregnancies, they believed that the condition of mankind could be upgraded biologically. Even those who would dismiss such claims must recognize that, indeed, in many countries through the twentieth century, such "biological" characteristics as height, disease resistance, and life expectancy have been significantly increased.

An important French intellectual tradition most frequently associated with Henri Bergson and a German tradition of social biology both saw mankind transcending its traditional limitations through technology.[26] The Austrian sociologist Rudolf Goldscheid published a volume in 1911, *Höherentwicklung und Menschenökonomie*, whose title in English means "Further development and the human economy," and his proposal that the twentieth century would be the era of biotechnics was echoed by many contemporaries.[27] The proponents of biotechnics from the period before and immediately after the

[24] The history of eugenics in Britain and the United States has been carefully described by Daniel Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (New York: Knopf, 1985).

[25] Alex Berman, "Romantic Hygeia: J. J. Virey, 1775–1846. Pharmacist and Philosopher of Nature," *Bulletin of the History of Medicine*, 39 (1965), 134–42.

[26] The tradition in France has benefited from William H. Schneider, *Quality and Quantity: The Quest for Biological Regeneration in Twentieth-Century France* (Cambridge: Cambridge University Press, 1990).

[27] Appreciations of German debates of the early twentieth century have been colored by knowledge of their tragic results, however. See Paul Weindling, *Health, Race and German Politics between National Unification and Nazism, 1870–1945* (Cambridge: Cambridge University Press, 1989).

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First World War, such as Raoul Francé and Rudolf Goldscheid, are mostly forgotten, but in England such ideas were taken up in the interwar years by Julian Huxley and his close friend Lancelot Hogben, whose works were long famous. During the interwar years, both Julian Huxley (who coauthored a popular book, *The Science of Life*, with H. G. Wells) and Hogben (author of a middle-class primer for the "Age of Plenty," *Science for the Citizen*) saw biological engineering as the next generation of engineering. Indeed, Julian Huxley's dreams of biological and social engineering called forth a reaction from his brother Aldous, author of *Brave New World*.[28] The response was made through fiction, but the biotechnological ideals themselves had not been carefully worked out in detailed discussions between scientists; rather, they were expressed in lectures and books addressed to the general public. It has indeed been a continuing feature of biotechnology that the millennial visions of scientific thinkers have been expressed in their public writings.

## BIOCHEMICAL ENGINEERING

From World War II, microbially produced antibiotics such as penicillin seemed to promise the conquest of infectious disease, biologically produced power alcohol would bring wealth to the world's rural poor, and microbially produced foods could solve the problem of world hunger. Exaggerated as these hopes proved to be, it could reasonably be argued at the end of the twentieth century that penicillin was the greatest individual product of biotechnology. Not only has it saved millions of lives, but by its example other antibiotics were discovered. Together, for a generation, they removed the fear of infectious disease from Western countries.

Penicillin was discovered in the juice produced by a naturally occurring mold. Although discovered by accident and then developed further for purely scientific reasons, the scarce and unstable chemical was transformed within three years during World War II into a powerful and widely used drug. Large networks of academic and government laboratories and pharmaceutical manufacturers in Britain and the United States were coordinated by agencies of the two governments. An unanticipated combination of genetics, biochemistry, chemistry, and chemical engineering skills had been required. When the natural mold was bombarded with high-frequency radiation, far more productive mutants were produced and subsequently all the medicine was made using the products of these man-made cells. Penicillin became cheap to produce and globally available by the 1950s, and this effort had an impact beyond the development and production of a single drug.[29] The

[28] Gary Werskey has described some of the biological visions of this circle. See Gary Werskey, *The Visible College* (London: Allen Lane, 1978). See also Turney, *Frankenstein's Footsteps*.

[29] For a survey, see Robert Bud, *Penicillin: Triumph and Tragedy* (Oxford: Oxford University Press, 2007)

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air-breathing mold was cultured in enormous continuously stirred fermenters, typically holding 50,000 liters. The skills developed in building and operating these fermenters proved useful for the industrial-scale production of many other microbiological products – including a host of new antibiotics as well as the steroids needed in the new contraceptive pill.

The new technology of cultivating and processing large quantities of microorganisms led to calls for a new scientific discipline. Biochemical engineering was one term and applied microbiology another. The Swedish biologist Carl-Goran Hedén, possibly influenced by German precedents, favored the term "*Biotechnologie*" and persuaded his friend Elmer Gaden to relabel his new journal *Biotechnology and Biochemical Engineering*. Beginning in 1962, major international conferences were held under the banner of the "Global Impact of Applied Microbiology."

The products of the new biochemical engineering could profoundly affect the lives of individuals as life-saving and health-maintaining drugs, as contraceptives, or as steroids that could eliminate pains hitherto considered inevitable. During the 1960s, the same technology was used to produce staples of modern life: fuel to provide energy and protein for food. Moreover, there was the prospect of doing this most efficiently in those tropical countries rich in biomass that were also the world's poorest. Alcohol could be manufactured by fermenting starch or sugar-rich crops such as sugar cane and corn. Brazil introduced a national program of replacing oil-based petroleum by alcohol.[30] In the United States, it seemed that oil from surplus maize would solve the problem of low farm prices aggravated by the country's boycott of the USSR in 1979. The alcohol fuels program targeted a six hundred percent increase in biofuel production and made more than a billion dollars available. The name "gasohol" came into currency.

Another new word for an old concept was "single-cell protein," which had been coined in 1966. During the First World War, the Germans had fed animals on yeast; now, by growing protein-rich bacteria and fungi on oil to produce a protein-rich food, it seemed the oil industry might eliminate problems of world hunger. In the Soviet Union, a major program of single-cell protein production was put in place.[31] In 1973, the German government, seeking a new, "greener" industrial policy, commissioned a report entitled *Biotechnologie* identifying ways in which biological processing was key to modern developments in technology.[32] Even though the report was published at the time when recombinant DNA was becoming possible, it did not refer

[30] Harry Rothman, Rod Greenshields, and Francisco Rosillo Callé, *The Alcohol Economy: Fuel Ethanol and the Brazilian Experience* (London: Pinter, 1983).

[31] David H. Sharp, *Bio-Protein Manufacture: A Critical Assessment* (Chichester: Ellis Horwood, 1989); Anthony Rimmington, with Rod Greenshields, *Technology and Transition: A Survey of Biotechnology in Russia, Ukraine and the Baltic States* (London: Pinter, 1992).

[32] Klaus Buchholz, "Die Gezielte Förderung und Entwicklung der Biotechnologie," in *Geplante Forschung*, ed. Wolfgang van den Daele, Wolfgang Krohn, and Peter Weingart (Frankfurt: Suhrkamp, 1979), pp. 64–116.

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to this new technique and instead focused on the use and combination of existing technologies to make novel products.

By the late 1970s, therefore, the renewed, self-consciously technological, zymotechnic endeavor had momentum. Major companies were investing in single-cell protein. Nations saw possibilities in new industries, and bioprocessing was becoming increasingly efficient. Single-cell protein, however, met consumer resistance, and indeed the problems of hunger in developing countries were more complex than absolute shortages of protein. The gasohol programs also proved uneconomical when oil prices dropped in the early 1980s. And the energy, enthusiasm, and vision that had been so characteristic of microbiology and biochemical engineering-based biotechnology were transferred to a new generation of companies applying new academic research in molecular biology. The eugenic and the zymotechnic would be wedded through genetics in the 1980s and 1990s – an integration already anticipated in the 1960s.

## MOLECULAR BIOLOGY

By the 1970s, molecular biology, a hitherto esoteric science, was making considerable progress, but its practice was, in general, rather distant from the world of industrial production. The phrase "genetic engineering" entered common parlance in the 1960s as a description of human genetic modification.[33] Medicine, however, was now putting a premium on the use of proteins hard to extract from people: insulin for diabetics and interferon for cancer sufferers. A few prophets, such as Joshua Lederberg and Walter Gilbert, argued that the new biological techniques of recombinant DNA might be ideal for making these expensive proteins through their expression in bacterial cells. Small companies, such as Cetus and Genentech in California and Biogen in Cambridge, Massachusetts, were established to develop the techniques. Larger companies kept a wary eye on the potential of these and other new competitors.[34]

The mechanism for the transfer of enthusiasm from engineering fermenters to engineering genes was Wall Street. At the end of the 1970s, new tax laws encouraged already adventurous U.S. investors to put money into small companies whose stock values might grow faster than their profits. New technology, particularly a technology based on American science, seemed to

[33] Gordon Wolstenholme, ed., *Man and His Future: A CIBA Volume* (London: Churchill, 1963); T. M. Sonneborn, ed., *The Control of Human Heredity and Evolution* (New York: Macmillan, 1965). At these two conferences Lederberg and Tatum launched the terms "euphenics" and "genetic engineering," respectively.

[34] Hall has described the competition between Biogen and Genentech to make the first recombinant-DNA–based insulin. See Stephen Hall, *Invisible Frontiers: The Race to Synthesise a Human Gene* (London: Sidgwick and Jackson, 1987).

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hold particular potential at a time when the Japanese were doing particularly well in such established products as steel and automobiles. The stockbroker E. F. Hutton saw the potential of the new molecular biology companies such as Biogen and Cetus; in searching around for a word that would describe their business, the well-established term "biotechnology" was chosen.[35] The use of the term attracted five hundred participants to a symposium held in September 1979 that focused on the production of interferon. In December of that year, a trademark was taken out on the use of this word in the title of a mass-produced magazine. The following year, the U.S. Supreme Court ruled for the first time that a bacterium could be patented.

The discoveries in molecular biology engendered anxiety as well as excitement. In the early 1970s, scientists, concerned about public opposition to science that could be used unethically, sought to regulate recombinant DNA research while it was still an infant and controllable technology. In 1976, the National Institutes of Health instituted regulations for government-funded experiments that enabled research to restart, at first in the United States and then elsewhere. From the late 1970s, legally binding controls on experimentation ensured that concerns within the scientific community would decline. But among the public in the United States and Europe, anxieties about the potential of biotechnology persisted. It is remarkable that whereas in the 1970s biotechnology was proposed as a green alternative to established smokestack industries, thereafter it came to be seen as particularly "unnatural." How did this happen when such care was taken in the 1970s to establish protective legislation before experimentation was carried out? Sheila Jasanoff has argued that we need to look at the separate conceptions of risk and resistance in countries such as the United States, Germany, and Britain. In each, the idea of risk itself was intimately intertwined with national traditions of assessment, expertise, and debate. Moreover, questions of biotechnology became locked into concerns about the trustworthiness of experts in general. In Germany during the 1980s, biotechnology was linked to both the memory of state-sponsored eugenics and the contemporary concerns over another suspect technology, nuclear power. In Britain, biotechnology came to be linked to numerous food scares that experts had failed to prevent. In the United States, the exploitation of stem cells has been linked to debates over abortion.

The anxieties brought forth enthusiasms. In the United States, the fear of excessive regulatory controls encouraged business and scientific leaders to express the most optimistic projections about the potential of biotechnology.

[35] For a first-class treatment of the enthusiasm of Wall Street, see Robert Teitelman, *Gene Dreams: Wall Street, Academia, and the Rise of Biotechnology* (New York: Basic Books, 1989). For the academic connections with business at the time, see Martin Kenney, *Biotechnology: The University-Industrial Complex* (New Haven, Conn.: Yale University Press, 1986). For a European view, see Luigi Orsenigo, *The Emergence of Biotechnology: Institutions and Markets in Industrial Innovation* (London: Pinter, 1989).

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Those projections then fed back into the Wall Street enthusiasm that sustained the new industry. In business, the subsequent twenty years have seen two phenomena. On the one hand, there has been an explosion of small companies, mostly in the United States but also in Europe.[36] Between 1978 and 1981, the "cumulative equity investment in all types of biotechnology companies rose from fifty million [dollars] to over eight hundred million,"[37] and despite earlier predictions, the Japanese have not become the dominant force. However, the number of firms making profits has been small. Some of the early pioneers have been taken over: Cetus and Genentech now belong largely to other organizations. Small companies still have a key role in innovation, but production and marketing have continued to be led by such large companies as Monsanto, whose genetically modified soya beans were among the first widely used genetically modified products.

Large bioscience companies have indeed been remarkably successful. In 1939, Merck, the major research-oriented pharmaceutical company, was scarcely more than two percent of the size of the major chemical company Du Pont. Half a century later, its revenues and stock market worth exceeded those of Du Pont. Combining with other firms, the great German chemical company Hoechst evolved into the pharmaceutical company Sanofi-Aventis, while its once equally diverse British competitor ICI spun off its large and profitable pharmaceutical wing. Although a few large molecules produced by recombinant DNA techniques have been successful both for research and for medicines, small molecules made by chemists following research using recombinant DNA have been the more common ultimate products. Such superdrugs of the 1980s and 1990s as Prozac (the antidepressant), Losec (combating ulcers), and Viagra (promoting sexual function) have all been made through synthetic chemistry rather than by means of molecular biology.

Nonetheless, the industrial ascendancy of the pharmaceutical companies and the recurrent attraction of the smaller biotechnology companies do point to a profound change in industry and agriculture.[38] If information technology remains more important as a determinant of overall industrial character, biotechnology seems central to our "modernity."

The Human Genome Project to sequence and map the human genome, which was born in the 1980s as a great scientific underpinning for future

[36] Sheila Jasanoff, "Product, Process or Programme: Three Cultures and the Regulation of Biotechnology," in *Resistance to New Technology*, ed. Martin Bauer (Cambridge: Cambridge University Press, 1995), pp. 311–34.

[37] Paul Rabinow, *Making PCR: A Story of Biotechnology* (Chicago: University of Chicago Press, 1996), p. 27. This book also presents a history of Cetus and an analysis of the PCR process it first developed.

[38] For the long-term history of agricultural biotechnology, see J. R. Kloppenberg, Jr., *First the Seed: The Political Economy of Plant Biotechnology, 1492–2000* (Cambridge: Cambridge University Press, 1988); Lawrence Busch et al., *Plants, Power, and Profit: Social, Economic, and Ethical Consequences of the New Biotechnologies* (Oxford: Blackwell, 1991); Daniel Charles, *Lords of the Harvest: Biotech, Big Money and the Future of Food* (Cambridge, Mass.: Perseus, 2001).

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biotechnology, may of course further promote the significance of biotechnology.[39] Despite the frequent use of the singular, there have been several human genome projects, plus other parallel projects to decode the genomes of the nematode worm, yeast, and bacteria. They demonstrate the integration of diverse traditions of applied science: on the one hand, the belief that scientific knowledge of the human genome will lead to technological benefit; on the other, a complex technological revolution in the techniques of sequencing, with a shift to an industrial ideology of speed. The Human Genome Project manifests a technological tradition that Lily Kay has seen as generic to molecular biology,[40] and appropriately, the question of patenting has been central in its recent history.[41]

Our reconstructions of the history of biotechnology remain intimately connected with our conceptions of both science and technology. This applied science is not just an application of pure science, but nor are the two quite separate; for instance, the study, cultivation, and exploitation of the bacterial cell and lately of mammalian cells have been shared. Technologies of analysis and mass production have involved both engineering and biology. Metaphors of network and linkages, ambivalence, and multiple identities have replaced the simple model of scientific cause and technical effect. The history of biotechnology can be seen as a sequence of networks through which self-consciously scientific and technological groups shared their expectations of a shining biotechnological future – as well as their specific techniques and a common concern with "bugs."

39 Robert Cook-Deegan, *The Gene Wars: Science, Politics and the Human Genome* (New York: Norton, 1994).

40 Lily E. Kay, *The Molecular Vision of Life: Caltech, the Rockefeller Foundation and the Rise of the New Biology* (Oxford: Oxford University Press, 1993).

41 F. K. Beier, R. S. Crespi, and J. Strauss, *Biotechnology and Patent Protection: An International Review* (Paris: OECD, 1985).

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# *Part IV*

# SCIENCE AND CULTURE

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# 29

## RELIGION AND SCIENCE

*James Moore*

The subject headings list of the U.S. Library of Congress is the most comprehensive such list ever assembled. A bibliographic *Michelin's Guide* would give it five stars – this world-class menu showing how Washington's *chefs de livres* serve up the field of knowledge. For a century, it has shaped the taxonomic tastes of librarians everywhere, and it still guides the providers of classified information in many fields. Some items on the menu are indeed irresistible, not least "Religion and Science." This is the library's preferred rubric for a vast number of publications, outstripping entries under "Science and Religion," "Theology and Science," and "Religion and Sciences" by a thousandfold or more. And how is "Religion and Science" carved up? The library divides it into over one hundred categories: by period and by place; through books and serials; in poetry, drama, and fiction; for readers from medics to children. The chronological breakdown is most detailed for the last two centuries, where "Religion and Science" titles are classified from 1800 to 1859, 1860 to 1899, 1900 to 1925, 1926 to 1945, and 1946 to date.

Useful as this scheme may be, like all taxonomies it assumes more than it can prove. For instance, why cut time's seamless web into segments ending in 1859 and 1925? Centuries are convenient – 1800, 1900 – and 1945 marks the end of a world war, but why pick out the years that saw publication of Charles Darwin's *On the Origin of Species* and the "monkey trial" of the Tennessee high school teacher John Scopes? To regard these events as having peculiar significance for organizing a subject as extensive as "Religion and Science" would be controversial, and in fact librarians of Congress have not always done so. A small number of older subheadings draw the line at 1857, 1858, 1879, and 1889; one range of dates ignores 1925 altogether. This suggests that with a little thought and ingenuity it would be possible to devise an entirely different periodization that takes account of the physical sciences, worldwide developments, or merely events in Europe. Why not, say, classify publications from 1789, breaking at 1814, 1848, 1871, 1914, and 1933?



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The short answer is that "Religion and Science" does not belong, as these dates do, to the nomenclature of social and political history. It is first and foremost an intellectual rubric, proper to the history of ideas, particularly ideas in the English-speaking world. Within its compass, religious ideas and scientific ideas inhabit embodied minds, but their history records the growing union of one set of ideas with the other, the separation of one set from the other, or the triumph of one over the other, accompanied in each case by the *elimination* of concrete social and political interference. Such mundane pressures are what frustrate the proper development of "Religion" and "Science." They are contaminants clogging history's cogs, debris left over from dark ages or from an era just passing away. Truth and reason (human or divine) are the agents of purification – keeping mankind on the up-and-up and history on the march – not wars, civil unrest, or meddling bureaucrats.

## A VICTORIAN RUBRIC

As an intellectual shibboleth, "Religion and Science" would have merit if G. M. Young were right that "the real, central theme of History is not what happened, but what people felt about it when it was happening;"[1] for there is no doubt that "Religion and Science" was an organizing category – an agonizing category – for many Victorians. Scores of books capitalized on their concern, retailing the odd couple in endless combinations. The boom began in the 1870s, abetted by a transatlantic best-seller, John William Draper's *History of the Conflict between Religion and Science* (1874). A radical Methodist turned chemist and pop-historian, Draper (1811–1882) lived among the Irish, moving from Merseyside to New York City. The Catholic Church was his bête-noire. From its tyrannical designs, his *Conflict* argued, mankind was destined to be liberated by science. True religion would also be enhanced, a point made even more memorably in *A History of the Warfare of Science with Theology in Christendom* (1896) by Cornell University's broad church president, Andrew Dickson White (1832–1918). In this, his life's work, White rounded on Cornell's sectarian critics, arguing that all dogmatically motivated interference with scientific inquiry invariably went against the true interests of both science and religion. It was a hands-off message – lost in a welter of footnotes. By dint of its two large tomes, *Warfare* only succeeded in "thrusting still deeper into the minds of thousands" precisely what White himself, and Draper, so deplored, "that most mistaken of all mistaken ideas: the conviction that religion and science are enemies."[2]

[1] G. M. Young, *Victorian England: Portrait of an Age*, 2nd ed. (London: Oxford University Press, 1952), p. vi.

[2] Andrew Dickson White, *A History of the Warfare of Science with Theology in Christendom*, 2 vols. (London: Macmillan, 1896), vol. 1, p. 410. Testimony to the rubric's power is in Sydney Eisen and

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In 1897, the year after White's *Warfare* was launched, the Library of Congress helped transform what Victorians "felt about" deeply into the "real, central theme of History" by incorporating "Religion and Science" into its authoritative subject headings. A pair of hypostatized abstractions made memorable by a pair of embattled propagandists became canonical for interpreting modern intellectual history. Hardly anyone inquired into the origins of "Religion and Science," the assumptions it represented, or the passions it aroused. The Victorian rubric was taken for granted as pundits and popularizers obsessed themselves with questions such as, "Are religion and science at war? Must they be? What are the causes of conflict? What are the chances of peace?" Typically, these were questions of *my* religion versus *your* science or *your* religion versus *my* science; that is, one set of convictions in their intellectual integrity as opposed to another in which retrograde ideologies were dressed up as true belief. Even academic historians did not escape such partisanship, as Frank Turner has explained.

By the third quarter of the twentieth century, when Turner joined the history department at Yale University, it had become "almost a rite of passage" for American scholars to adopt a "truth-vanquishing-error" understanding of modern intellectual life. This secular teleology was derived more or less uncritically from nineteenth-century freethinkers, such as Draper and White, who believed they were winning the "conflict between religion and science." Historians "accepted often at face value the reading and self-explanation of a relatively limited number of authors and then tended to use them as guides to their own cultural situation." With communists to the left of them, fundamentalists on the right (and lately feminists, multiculturalists, and postmodernists . . . everywhere), American liberals played up those aspects of Victorian intellectual life deemed essential to their own sense of moral worth as members of a new, embattled secular clerisy.[3] Rather than analyze the received view of "Religion and Science," they found it opportune simply to take sides and perpetuate a period piece as "the real, central theme of History."

In recent years, the poverty of this procedure has become apparent as younger historians in Britain and North America, some of them politically unsettled during the 1960s, questioned the adequacy of many liberal nostrums. Accounts of "conflict" and "warfare" now seemed one-sidedly militaristic; the terms "religion" and "science" were found "too large for profitable use" in serious debates about the past.[4] With the decline of historical master narratives – hastened by the collapse of Soviet communism – and the loss of "the big picture" in the history of science, scholars began to focus

Bernard V. Lightman, eds., *Victorian Science and Religion: A Bibliography with Emphasis on Evolution, Belief, and Unbelief, Comprised of Works Published from c. 1900–1975* (Hamden, Conn.: Archon, 1984).

[3] Frank M. Turner, *Contesting Cultural Authority: Essays in Victorian Intellectual Life* (Cambridge: Cambridge University Press, 1993), pp. 6–9.

[4] Owen Chadwick, *The Secularization of the European Mind in the Nineteenth Century* (Cambridge: Cambridge University Press, 1975), p. 175.

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on the local, the particular, and the contingent. "Actors' categories" such as "Religion and Science" became *explanandum* rather than *explanans*, social and political pressures the key rather than the contaminant to intellectual developments. The most momentous reversal came with the transformation of "Darwin studies" during the 1980s. "In no other area of Victorian intellectual history," Turner observes, "has there occurred so extensive a revision in attitudes" toward the so-called conflict of religion and science. The upheaval was made possible by historians' "bracketing the issue of the truth content of ideas and ideological movements" and focusing on "the particular social setting of scientific activity." This "contextualist" approach has been widely adopted. Fortified by a generation's work in the sociology of scientific knowledge, it has vastly enriched understanding of both science and religion during the last two centuries.[5]

As a result, 1859 no longer looms as the watershed when Darwin began mopping up a moribund opposition. The Library of Congress subject headings "Religion and Science, 1800–1859" and "Religion and Science, 1860–1900" only demonstrate the success of Victorian propaganda. Nor indeed should 1925 be taken as a decisive date, according to recent research. For decades, interpreters of the Scopes trial merely aped its protagonists, who themselves saw the proceedings as a belated outbreak of Draper's and White's old battle. After the agnostic defense attorney Clarence Darrow (1857–1938) humbled, but failed to defeat, the fundamentalist state's prosecutor, William Jennings Bryan (1860–1925), the trial nevertheless went down in American history as a "symbolic victory" for civil libertarians who "successfully stood up to a majoritarian tyranny."[6] In truth, fundamentalism was driven underground, where it continued to thrive; Bryan's death just after the trial ensured his beatification. The liberal triumphalist view has been consigned to political folklore, and with its demise, the subject headings "Religion and Science,

[5] Turner, *Contesting Cultural Authority*, pp. 17–18. Key revisionist studies (in chronological order) are: Frank Miller Turner, *Between Science and Religion: The Reaction to Scientific Naturalism in Late Victorian England* (New Haven, Conn.: Yale University Press, 1974); James Moore, *The Post-Darwinian Controversies: A Study of the Protestant Struggle to Come to Terms with Darwin in Great Britain and America, 1870–1900* (Cambridge: Cambridge University Press, 1979), pp. 1–100; Martin Rudwick, "Senses of the Natural World and Senses of God: Another Look at the Historical Relation of Science and Religion," in *The Sciences and Theology in the Twentieth Century*, ed. A. R. Peacocke (Stocksfield: Oriel Press, 1981), pp. 241–61; Robert M. Young, *Darwin's Metaphor: Nature's Place in Victorian Culture* (Cambridge: Cambridge University Press, 1985); Ronald L. Numbers, "Science and Religion," *Osiris* (2nd ser.), 1 (1985), 59–80; David C. Lindberg and Ronald L. Numbers, "Beyond War and Peace: A Reappraisal of the Encounter between Christianity and Science," *Church History*, 55 (1986), 338–54; Pietro Corsi, *Science and Religion: Baden Powell and the Anglican Debate, 1800–1860* (Cambridge: Cambridge University Press, 1988); John Hedley Brooke, *Science and Religion: Some Historical Perspectives* (Cambridge: Cambridge University Press, 1991); Ronald L. Numbers, *The Creationists: The Evolution of Scientific Creationism* (New York: Knopf, 1992); Edward J. Larson, *Summer for the Gods: The Scopes Trial and America's Continuing Debate over Science and Religion* (New York: Basic Books, 1997); John Brooke and Geoffrey Cantor, *Reconstructing Nature: The Engagement of Science and Religion* (Edinburgh: T. and T. Clark, 1998); Peter J. Bowler, *Reconciling Science and Religion: The Debate in Early-Twentieth-Century Britain* (Chicago: University of Chicago Press, 2001).

[6] Larson, *Summer for the Gods*, pp. 22–3, 234, 238, 247.

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1900–1925" and "Religion and Science, 1926–1945" also seem quaint, unwitting testimony to the power of partisan legend.

Today "Religion and Science" is being set aside as scholars revise other episodes, including the Victorian debates from which the rubric itself arose. This chapter reviews five fields of contention clustered around the transformed domain of Darwin studies.[7]

## FREETHOUGHT

Nothing better illustrates historians' flight from intellectual abstractions than their new interest in that hydra-headed monster, unbelief. Gritty, irrepressible, it constantly reinvented itself, or was reinvented, as the nineteenth century's ideological "other." Revolutionary France sponsored "materialism" and "atheism," German scholars taught "rationalism," and imperial Britain launched "secularism" and "agnosticism." From midcentury, another French export, "positivism," was hailed on both sides of the Atlantic as the latest scientific form of infidelity. The generic term "freethought" stood for all such deviant "isms," and it still captures their political thrust. Freethought was political because religion itself was. Where Christianity was established by law, resplendent in state and church, to think freely in religion was treacherous, to promulgate one's ideas, seditious. Heterodoxy and political dissent thus went hand in hand. Freethinkers were usually advanced liberals, though many bore out churchmen's worst fears by backing republican and revolutionary causes.[8]

In Britain, the nineteenth century opened with deists mocking miracles, Unitarians denying Christ's divinity, and university heretics such as William Frend (1757–1841) at Cambridge and Edinburgh's John Leslie (1766–1832) falling afoul of their clerical colleagues. Then hell let loose for decades as radicals' demands reached a crescendo. Artisan demagogues baited bishops

[7] Besides the studies in footnote 5, see these collective works (in chronological order): John Durant, ed., *Darwinism and Divinity: Essays on Evolution and Religious Belief* (Oxford: Blackwell, 1985); David C. Lindberg and Ronald L. Numbers, eds., *God and Nature: Historical Essays on the Encounter between Christianity and Science* (Berkeley: University of California Press, 1986); James Moore, ed., *History, Humanity and Evolution: Essays for John C. Greene* (Cambridge: Cambridge University Press, 1989); Bernard Lightman, ed., *Victorian Science in Context* (Chicago: University of Chicago Press, 1997); David N. Livingstone, D. G. Hart, and Mark A. Noll, eds., *Evangelicals and Science in Historical Perspective* (New York: Oxford University Press, 1999); Ronald L. Numbers and John Stenhouse, eds., *Disseminating Darwinism: The Role of Place, Race, Religion, and Gender* (Cambridge: Cambridge University Press, 1999).

[8] Edward Royle, *Victorian Infidels: The Origins of the British Secularist Movement, 1791–1866* (Manchester: Manchester University Press, 1974); Edward Royle, *Radicals, Secularists and Republicans: Popular Freethought in Britain, 1866–1915* (Manchester: Manchester University Press, 1980); Bernard Lightman, *The Origins of Agnosticism: Victorian Unbelief and the Limits of Knowledge* (Baltimore: Johns Hopkins University Press, 1987); Charles D. Cashdollar, *The Transformation of Theology, 1830–1890: Positivism and Protestant Thought in Britain and America* (Princeton, N.J.: Princeton University Press, 1989).

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and ridiculed the Bible, outraging with their effrontery. Science was pressed into service, science as knowledge open to all that made against Christianity. The radicals picked up much of it where they learned their politics, from France. The materialism of Paul d'Holbach, the mathematical determinism of Pierre Laplace, Julien de la Mettrie's "homme machine," and the transmutation theories of Jean-Baptiste Lamarck (1744–1829) and Etienne Geoffroy Saint-Hilaire (1772–1844) circulated in countless cheap recensions. Nearer to home, respectable works were cannibalized and made to teach anti-Establishmentarian lessons. Armed with these, rebels such as Richard Carlile (1790–1843) urged "men of Science" to cure society's religious mania; workingmen flocked to "Halls of Science" to learn how environmental manipulation could make a socialist world; and the blasphemous periodical *Oracle of Reason* in the 1840s made matter-to-man evolution the bulwark of political atheism.[9] Here, in a twilight world of backstreet cliques, soapbox rants, and unstamped rags, the Victorian roots of "Religion and Science" are to be found.

Science, however, was manifold, not the monolith of propagandists. The lines between artisanal and gentlemanly science, radical and conservative science, and heterodox and orthodox science were constantly redrawn. Allies turned up on opposing sides, as in debating the merits of mesmerism, phrenology, and later spiritualism, or ideological enemies might sit at the same table to defend their particular expertise.[10] Allegiances shifted and new coalitions formed, but overall a gulf was fixed between the partisans of matter and those of spirit. Some few believed that matter moves itself and produces all the phenomena of life and mind, but the great majority insisted that spirit – God, souls, angels, devils – is the force behind moving matter, endowing it with purpose, vitality, and consciousness. Intermediate positions were rare, and all agreed that morals and politics flowed from their assumptions. Among the middle and upper intelligentsia, the spiritual view was sacred to Anglicans and many Dissenters, all Tories or conservative Whigs. Matter's potency was defended by freethinkers, including Unitarians and Dissenters whose politics ranged from Whig to the radical extremes.[11] Christian men of science aligned themselves on both sides of the issue. "Religion and Science" it was not.

9 Adrian Desmond, "Artisan Resistance and Evolution in Britain, 1819–1848," *Osiris* (2nd ser.), 3 (1987), 77–110.

10 Alison Winter, *Mesmerized: Powers of Mind in Victorian Britain* (Chicago: University of Chicago Press, 1999); Roger Cooter, *The Cultural Meaning of Popular Science: Phrenology and the Organization of Consent in Nineteenth-Century Britain* (Cambridge: Cambridge University Press, 1984); Logie Barrow, *Independent Spirits: Spiritualism and English Plebeians, 1850–1910* (London: Routledge and Kegan Paul, 1986); Janet Oppenheim, *The Other World: Spiritualism and Psychical Research in England, 1850–1914* (Cambridge: Cambridge University Press, 1985).

11 L. S. Jacyna, "Immanence or Transcendence: Theories of Life and Organization in Britain, 1790–1835," *Isis*, 74 (1983), 311–29; L. S. Jacyna, "The Physiology of Mind, the Unity of Nature, and the Moral Order in Late Victorian Thought," *British Journal for the History of Science*, 14 (1981), 109–32.

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The radicals did suffer, some as causes célèbres. William Lawrence (1783–1867), a materialist and republican, saw his 1816 lectures on comparative anatomy pirated eight times by street atheists after the Tory press declared them blasphemous. John Elliotson (1791–1868), damned for denying the soul's existence, lost the chair of medicine at London University in 1838 after his patients ran amuck in mesmeric trances. His colleague Robert Grant (1793–1874), professor of comparative anatomy, survived quietly from 1827 until 1874, a perpetual pariah for his Lamarckian deism. Meanwhile, rank-and-file freethinkers kept up the attack on Tory-Anglican privilege. Radical doctors and their allies detested the Oxford- and Cambridge-educated professors who monopolized London's hospitals and royal colleges, grooming society physicians. "Old corruption" reigned here, and the dissidents, many of them fierce democrats, hated the spiritual science taught in the colleges as much as the politics. In the capital's cut-price medical schools (and to some extent in the "godless" London University), they fought back with the latest Continental research, training Dissenting general practitioners in materialist physiologies and the laws of life's unity and development. Radicals hoped that such doctrines, spread among the poor and middle classes by their trusted medical advisers, would help break the church's grip on science and state, hastening political reform.[12]

By the 1850s, with reform having spread to the Anglican universities, the secularist slogan "science is the available providence of man" expressed a truth that even freethinking Christians could affirm, not least because natural theology now pointed to a *divine* providence less persuasively than before.

## NATURAL THEOLOGY

The "religion" from which "science" had to be freed to become truly scientific was above all natural theology. Natural theology was what freethinkers fought and Darwin finally refuted. Natural theology fostered the illusion of a static purposeful world governed by God rather than law. Progress in the history of science is measured by the extent to which this illusion has been dispelled – or so it was once assumed. Natural theology was important for the impediments it created rather than the inhibitions it released. Today a more measured view prevails. No one doubts that natural theology sought to instill religious beliefs and values by appealing to evidence of divine purpose in nature or that the enterprise often obscured scientific truths that later seemed self-evident. What gives natural theology fresh significance is historians' recovery of its diverse strategies and meanings.

[12] Adrian Desmond, *The Politics of Evolution: Morphology, Medicine, and Reform in Radical London* (Chicago: University of Chicago Press, 1989).

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Natural theology was not single and static but a shifting congeries of moral pursuits. For instance, the appeal to beneficent design in nature that Basil Willey called "cosmic Toryism" was routine also among Whigs. Even the arch-radical Tom Paine (1737–1809) made political capital from design, arguing that God's inventions in nature gave men reason to invent revolutions. Conceiving nature as a preceptive moral order, whatever one's politics, was indeed the heart and soul of natural theology; it turned the world's "is" into "ought" and served on the whole, pace Paine, to reconcile human hopes with painful realities.[13] But much more was involved. Natural theology was unabashedly *apologetic*, defending the existence and attributes of the Creator against the claims of unbelief. It *edified* believers, strengthening their faith by pointing up the wonders of creation. It *mediated* among sects, serving as common ground on which to bury doctrinal conflicts. It *motivated* men to investigate the world, and it *ratified* inquiries that bore witness to God's wisdom, power, and goodness. Natural theology was also a *stumbling block* for many Christians. High Anglicans, Scots evangelicals, and pietists everywhere saw it as tainted with rationalism. Its so-called proofs, they argued, carried conviction only to those who believed and, in any case, did not produce holy character and conduct, much less faith unto salvation. These came through the church and scripture alone.[14]

Yet natural theology remained vital, more or less. The enterprise came to be identified with one aspect of it, the argument from design, thanks above all to Rev. William Paley (1743–1805). The arid archdeacon has suffered undeservedly. Contrary to legend, his book *Natural Theology* (1802) was never required reading at Cambridge University, boring students and stifling science throughout the nineteenth century. Nor was Paley himself a reactionary. Complacent in an age of revolution, tolerant of abuses in church and state, he remained a moderate Whig, latitudinarian in theology, utilitarian in morals. *Natural Theology* put the case for God's being and attributes on a purely naturalistic basis.[15] Living things, human and animal alike, are literal machines, with each lever, joint, and pulley perfectly adapted to perform its task. (Perfecting this image was Paley's own great invention.) All machines

[13] Basil Willey, *The Eighteenth Century Background* (London: Chatto and Windus, 1940), chap. 3; Jack Fruchtman, Jr., *Thomas Paine and the Religion of Nature* (Baltimore: Johns Hopkins University Press, 1993); John Hedley Brooke, "Natural Theology and the Plurality of Worlds: Observations on the Brewster-Whewell Debate," *Annals of Science*, 34 (1977), 221–86; cf. Young, *Darwin's Metaphor*, pp. 126–63.

[14] Brooke, *Science and Religion*, pp. 192–225; Frank M. Turner, "John Henry Newman and the Challenge of a Culture of Science," *The European Legacy*, 1 (1996), 1694–1704; Jonathan Topham, "Science, Natural Theology, and Evangelicalism in Early Nineteenth-Century Scotland: Thomas Chalmers and the 'Evidence' Controversy," in Livingstone, Hart, and Noll, *Evangelicals and Science in Historical Perspective*, pp. 142–74.

[15] Aileen Fyfe, "The Reception of William Paley's 'Natural Theology' in the University of Cambridge," *British Journal for the History of Science*, 30 (1997), 324–35; Mark Francis, "Naturalism and William Paley," *History of European Ideas*, 10 (1989), 203–20.

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have a designer, so nature's machines must have a Designer; God's wise and beneficent existence is as certain as Matthew Boulton's and James Watt's. To Paley, the proof was "not only popular but vulgar," a hands-on deduction from everyday life. In an age of factories and steam, it was designed to teach – and warn – restless operatives that a Master Mechanic ruled the world.[16]

*Natural Theology* was shelved in Mechanics' Institutes, lending an Anglican aura to the "useful knowledge" dispensed there for the improvement of the working classes. Cheap editions circulated among the poor. Not so the *Bridgewater Treatises on the Power, Wisdom and Goodness of God as Manifested in the Creation* (1833–6), eight works in eleven volumes costing £7, over two months' wages for a laborer. The authors, hand-picked by the Archbishop of Canterbury, the Bishop of London, and the president of the Royal Society, received £1,000 each. With authority to match its price, the set was pitched to the pious middle classes, among whom it was displayed as much as read. Yet the *Bridgewater Treatises* had a long shelf life and wide appeal. This ne plus ultra of natural theology contained up-to-date accounts of astronomy, anatomy, physiology, geology, chemistry, and other fields. Paley had argued by homely analogy for the Creator's wisdom and beneficence; these tomes testified to the wisdom and beneficence of science as pursued by gentlemanly specialists. Donors placed the *Bridgewater Treatises* in Mechanics' Institutes to ensure that workingmen got the message.[17] No sooner had the eighth title been shelved, however, than the Anglican polymath Charles Babbage (1792–1871) used his famous calculating engine – the talk of intellectual London – to append an unsettling lesson. Just as he could set up his glorified gearbox to produce discontinuities in a regular sequence of numbers, so, he argued, God could have built a "higher law" into the world's machinery by which organisms were created *naturally* rather than, as Paley and the *Bridgewater* authors assumed, miraculously. Babbage reckoned that such a law might be discovered and said so in his unsolicited *Ninth Bridgewater Treatise* (1839).

A further lesson was drawn. Machines that mimicked miracles could be used to dispense with the Creator. To avert this danger, some men of science proposed to set natural theology on a higher plane. Ideas were the best guide to God, according to Rev. William Whewell (1794–1866), whose own omniscience was legendary. A man of Christian character responds intuitively to God-implanted ideas; all the world's machinery does not tempt him to

[16] William Paley, *Natural Theology: or, Evidence of the Existence and Attributes of the Deity, Collected from the Appearances of Nature*, 5th ed. (London: printed for R. Faulder, 1803), p. 457; Neal C. Gillespie, "Divine Design and the Industrial Revolution: William Paley's Abortive Reform of Natural Theology," *Isis*, 91 (1990), 214–29.

[17] Jonathan Topham, "Science and Popular Education in the 1830s: The Role of the 'Bridgewater Treatises'," *British Journal for the History of Science*, 25 (1992), 397–430; John M. Robson, "The Fiat and Finger of God: 'The Bridgewater Treatises'," in *Victorian Faith in Crisis: Essays on Continuity and Change in Nineteenth-Century Religious Belief*, ed. Richard J. Helmstadter and Bernard Lightman (London: Macmillan, 1990), pp. 71–125.

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atheism but merely confirms his innate sense of the Lawgiver behind nature's laws.[18]

Richard Owen (1804–1892), Britain's premier paleontologist and scourge of the francophile medical fringe, was such a man. As the *Bridgewater Treatises* appeared in the 1830s, he sought to strengthen the design argument by interpreting organisms as more than just machines. Nature's machines sometimes failed, their structures proved maladaptive. But where adaptation was less than perfect, Owen found similarities of form, as in the skeletal structure of the mole's trowel, the bat's wing, and the human forelimb. Lamarck and Geoffroy took this as material evidence of evolutionary descent, but for all his love of old bones, Owen hated matter. He explained the similarities as variants of an underlying Idea. Gone was God the Master Mechanic, miraculously crafting species, each for its environment. In Owen's natural theology, God was the August Architect who devised an eternal blueprint and then guided its realization through time, adjusting it step-by-step at the birth of species one from another through ordinary reproduction. This blueprint, or "archetype," so clear in Owen's mind, was proof of Mind's dominion over matter, a proof more durable than that of Paley's mere machines.[19]

But Owen's natural theology itself was ill-adapted to an age of material progress. When Robert Chambers (1802–1871), in his anonymous potboiler *Vestiges of the Natural History of Creation* (1844), ascribed the birth of new species to a Babbage-like higher law and then, with a bit of untutored handwaving, declared that matter could indeed account for all life's phenomena, mankind included, outraged Anglicans pressed Owen to explain why his own views did not make *him* an evolutionist.[20]

## EARTH HISTORY

The "curious providentialism" of naturalists such as Owen cast up "embarrassing obstacles" to the progress of the earth and life sciences. So argued Charles Gillispie in his Cold War classic *Genesis and Geology* (1951). The problem, as Gillispie saw it, was "one of religion (in a crude sense) *in* science rather

[18] John Hedley Brooke, "Scientific Thought and Its Meaning for Religion: The Impact of French Science on British Natural Theology, 1827–1859," *Revue de synthèse* (4th ser.), 110 (1989), 33–59; Richard Yeo, *Defining Science: William Whewell, Natural Knowledge Public Debate in Early Victorian Britain* (Cambridge: Cambridge University Press, 1993).

[19] Desmond, *Politics of Evolution*, chaps. 6–8; Adrian Desmond, *Archetypes and Ancestors: Palaeontology in Victorian London, 1850–1875* (London: Blond and Briggs, 1982); Nicolaas Rupke, *Richard Owen, Victorian Naturalist* (New Haven, Conn.: Yale University Press, 1994); Dov Ospovat, "Perfect Adaptation and Teleological Explanation: Approaches to the Problem of the History of Life in the Mid-nineteenth Century," *Studies in the History of Biology*, 2 (1978), 33–56.

[20] Evelleen Richards, "A Question of Property Rights: Richard Owen's Evolutionism Reassessed," *British Journal for the History of Science*, 20 (1987), 129–71; James A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of "Vestiges of the Natural History of Creation"* (Chicago: University of Chicago Press, 2000).

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than one of religion *versus* science."[21] Religion did not attack science from without so much as undermine it from within. The subversion had to stop for science to progress, and rooting it out fell above all to the father of modern geology, Charles Lyell, and his intellectual heir-apparent, Charles Darwin.

Gillispie's groundbreaking social history was a boon for scholars, but its positivist dynamic harked back a century. The title *Genesis and Geology* was itself a piece of Victoriana, one to which White's *Warfare* gave a characteristic twist in a chapter entitled "From Genesis to Geology." The alliteration is as misleading as it is memorable. At no time since the early 1800s was the Book of Genesis normative or (with rare exceptions) even relevant for the theory or practice of the earth sciences among accredited geologists. Amateurs there were aplenty, men of marginal attainment, dabblers and dilettantes, collectors and speculators, who took Genesis as an inspired shortcut to truths about the earth. And laypersons continued to be upset by the disharmony between these truths and the increasingly confident claims of geological specialists. But among those uttering such claims, the cultured men who first made the earth sciences a profession, none did more than genuflect toward Genesis in his research. Their religious stance would be better characterized as "Genesis *or* geology," or better, "*gentlemen* and geology." Any adept could make his name studying beetles, shellfish, or mushrooms, but eminence in the nineteenth-century earth sciences was reserved for men of rank and substance – squires, clergymen, lawyers, military officers, and only later full-time academic specialists. Even the Geological Survey of Great Britain, the largest professional scientific organization maintained by Victorian governments, was dominated by wealthy landowners and littered with their protégés.[22] Piety united these patricians, as befitted their lofty status. If Genesis did not dictate their science, religious conviction nevertheless sustained it.

Stratigraphy, their chief task, called for a strict empiricism that was itself enshrined in natural theology as the one true method of studying God's works. In all technical debates, the squires fought constantly to occupy this "Baconian" high ground.[23] Dynamic geology, concerned with causes in earth history, offered more scope for speculation, yet here, too, the gentlemen

[21] Charles Coulston Gillispie, "Preface to the Original Edition," in Charles Coulston Gillispie, *Genesis and Geology: A Study in the Relations of Scientific Thought, Natural Theology, and Social Opinion in Great Britain, 1790–1850*, new ed. (Cambridge, Mass.: Harvard University Press, 1996), p. xxix, emphasis added.

[22] Roy Porter, *The Making of Geology: Earth Science in Britain, 1660–1815* (Cambridge: Cambridge University Press, 1977); Nicolaas Rupke, *The Great Chain of History: William Buckland and the English School of Geology, 1814–1849* (Oxford: Clarendon Press, 1983); Nicolaas Rupke, "Foreword," in Gillispie, *Genesis and Geology*, pp. v–xix.

[23] Martin J. S. Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists* (Chicago: University of Chicago Press, 1985); James A. Secord, *Controversy in Victorian Geology: The Cambrian-Silurian Dispute* (Princeton, N.J.: Princeton University Press, 1986); John Hedley Brooke, "The Natural Theology of the Geologists: Some Theological Strata," in *Images of the Earth: Essays in the History of the Environmental Sciences*, ed. L. J. Jordanova and Roy S. Porter (Chalfont St. Giles: British Society for the History of Science, 1979), pp. 39–64.

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were agreed in principle: Ancient causes ordained by God had been natural – "actual" – rather than supernatural; evidence alone could decide whether the effects were sudden and "catastrophic" or gradual and "uniform." In any case, no one doubted that the earth's crust had been shaped over millions of years. Even in the tense field of paleontology, the geological gents joined hands. With one key exception, they saw the fossil record as a progression of life forms culminating in *Homo sapiens*, a progression not unlike that of (but neither derived from) the creation narratives of Genesis. Such unanimity belies the traditional view that the nineteenth-century earth sciences were torn by religious controversy. The notion that geologists were "split into opposed parties of uniformitarians and catastrophists, of progressives and reactionaries, of enlightened scientists and bigoted obscurantists" is a "historical myth."[24]

Professionals did fall out with amateurs who insisted on their superior competence as interpreters of earth history from the Book of Genesis. These "scriptural geologists" were a ragtag tribe – retired merchants, medics with time to kill, clergymen-naturalists, linguists, and antiquaries – men with a vested interest in expounding the meaning of books, rather than rocks, in public places. The rock specialists, for their part, responded wearily, with wonted piety. Far from impugning God's Word, they sought to show how slightly or edifyingly their discoveries affected its interpretation. For some, "harmonizing" Genesis and geology became an avocation. The clerical core of London's Geological Society, William Buckland (1784–1856), Adam Sedgwick (1785–1873), and William Conybeare (1787–1857), did not condone detailed schemes of reconciliation, but each of them was at pains to assure laypersons that although the Bible did not teach scientific truths, it would never be found to contradict well-established facts. In North America, the same conviction was espoused throughout the century by a succession of elite evangelical geologists, Benjamin Silliman (1779–1864), Edward Hitchcock (1793–1864), Arnold Guyot (1807–1884), James Dwight Dana (1813–1895), and John William Dawson (1820–1899). Each of them published one or more popular books – Dawson over a dozen – to show how Genesis could accommodate the revelations of earth history.[25]

Charles Lyell (1797–1875) would have none of it. He wrote *Principles of Geology* (1830–3) precisely to free the science from scripture. So plausibly did he tar his colleagues with a biblicist brush that historians long thought he stood alone for science against religion. The truth is nearly the reverse. An

[24] Rudwick, *Great Devonian Controversy*, p. 46. See Martin J. S. Rudwick, *The Meaning of Fossils: Episodes in the History of Palaeontology* (London: Macdonald, 1972); R. Hooykaas, *The Principle of Uniformity in Geology, Biology, and Theology*, new ed. (Leiden: E. J. Brill, 1963).

[25] James Moore, "Geologists and Interpreters of Genesis in the Nineteenth Century," in Lindberg and Numbers, *God and Nature*, pp. 322–50; Rodney L. Stiling, "Scriptural Geology in America," in Livingstone, Hart, and Noll, *Evangelicals and Science in Historical Perspective*, pp. 177–92; Charles F. O'Brien, *Sir William Dawson: A Life in Science and Religion* (Philadelphia: American Philosophical Society, 1971).

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Anglican-turned-Unitarian, Lyell regarded nature with the usual reverence, upholding Paleyan design, divine providence, and even the possibility of creation by some higher law. It was his scientific beliefs that made him idiosyncratic. In *Principles*, he defined the study of geological dynamics, conventionally enough, to exclude miraculous events such as Noah's Flood, but he also stipulated that only causes of the same type *and intensity* as those now acting may be invoked. Geologists with evidence of extrabiblical catastrophes objected to this a priori ban. Nor were Noah and his ilk all that Lyell ruled out. To undermine Lamarckian transmutation, he went so far as to deny life a history. In *Principles*, the earth's crust subsists in a directionless steady state; species are inserted, preadapted, into environments fluctuating around an eternal mean. The fossil record shows no progression – or would not if it were complete – no escalator rising up to man. And so Lyell arrived at the belief from which, in fact, his whole science began: Mankind is sui generis, not evolved from soulless beasts.

Others based this belief on Genesis. Lyell defended it by expunging all trace of Genesis from his geology, even a progressive creation. In this he stood all but alone, isolated by a deep faith in personal immortality and a lofty distaste for miscegenation (as seen in his tolerance of slavery). Elevated spiritual creatures like himself, ex Oxford and Lincoln's Inn, were the be-all and end-all of life on earth.[26] Even Lyell's most celebrated disciple could not dissuade him.

## DARWIN

At the crossroads of freethought, natural theology, and Lyellian earth history stood Charles Darwin (1809–1882), world traveler and wealthy Whig, whose science is supposed to have undermined religion by solving that age-old "mystery of mysteries," how living species originate.[27] Baptized an Anglican and brought up a Unitarian, Darwin was sent by his freethinking father first to read medicine at Edinburgh University, where he became a protégé of the young Lamarckian Robert Grant, and afterward, by default, to Cambridge

[26] Martin J. S. Rudwick, "The Strategy of Lyell's 'Principles of Geology'," *Isis*, 61 (1970), 4–33; Michael Bartholomew, "The Singularity of Lyell," *History of Science*, 17 (1979), 276–93; Michael Bartholomew, "The Non-progress of Non-progression: Two Responses to Lyell's Doctrine," *British Journal for the History of Science*, 9 (1976), 166–74; Michael Bartholomew, "Lyell and Evolution: An Account of Lyell's Response to the Prospect of an Evolutionary Ancestry for Man," *British Journal for the History of Science*, 6 (1973), 261–303.

[27] Adrian Desmond and James Moore, *Darwin* (London: Michael Joseph, 1991); Janet Browne, *Charles Darwin*, 2 vols. (London: Cape, 1995–2002); Peter J. Bowler, *Charles Darwin: The Man and His Influence* (Oxford: Blackwell, 1990). For literature, see Adrian Desmond, Janet Browne, and James Moore, "Darwin, Charles Robert (1809–1882)," *Oxford Dictionary of National Biography*, 60 vols. (Oxford: Oxford University Press, 2004), vol. 15, pp. 177–202.

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to prepare for holy orders. Here he pored over Paley and fell in with reverend professors who steered him aboard HMS *Beagle* with exhortations to read Lyell. Having taught himself *Principles* on the voyage, Darwin returned a convert, joined the Geological Society under Lyell's wing, and began to make his name in the science, whereupon, in a rush of audacity, in the privacy of pocket notebooks, he committed intellectual treason. At the Victorian crossroads, he struck out in a direction all his own, an evolutionist incognito, hell-bent on explaining the whole living creation – species, mind, and society – by natural law. The church was left behind.

But this was no atheist agenda, however much Darwin's faith eventually faltered. From his freethinking and Unitarian heritage, he knew that matter held the potency to fulfill God's purposes. From Paley and the *Bridgewater Treatises*, he understood that these purposes included the exquisite adaptations of organism to environment. From Lyell, he learned how environments changed gradually through countless ages according to divinely established laws. Where master and disciple parted ways was over man. Darwin had encountered savages – native Fuegians in South America – whereas Lyell had not. And for one inured to members of his own species living like beasts, transmutation held no terrors, so Lyell's antiprogressionism did not hinder Darwin's clandestine research. He pushed ahead defiantly, searching for the law by which organisms became adapted, knowing it must explain the origin of the human species like all the rest. It was to be a higher law like Babbage's rather than Owen's ethereal archetype (which Darwin took as evidence of real descent), but equally a law without the vagary of *Vestiges*, though the book was backed by religious liberals who one day would be his allies: Baden Powell (1796–1860), W. B. Carpenter (1813–1885), and Francis Newman (1805–1897).[28]

Darwin got his law from reading a notorious work of social theology, *An Essay on the Principle of Population* (6th ed., 1826), by the darling of Whig reformers, Rev. Thomas Malthus (1766–1834). "Parson Malthus" taught that economic scarcity was natural, a struggle for the means of subsistence inevitable. Human populations tend to grow at a geometrical rate, whereas their food supply can at best be increased arithmetically. This "principle" was ordained by God as inducement for men to till the soil, restrain their lusts, and prepare themselves for a blessed hereafter.[29] Reading Malthus in 1838, Darwin realized that among animals and plants, unable to control their lusts, the struggle for existence must be many times more intense. Its "final cause" or purpose, he decided, was to sift out the individuals with some advantage and thus *selectively* adapt populations to changing environments. He called

[28] Corsi, *Science and Religion*, chaps. 16–17.

[29] Patricia James, *Population Malthus: His Life and Times* (London: Routledge and Kegan Paul, 1979); Mervyn Nicholson, "The Eleventh Commandment: Sex and Spirit in Wollstonecraft and Malthus," *Journal of the History of Ideas*, 51 (1990), 401–21; A. M. C. Waterman, *Revolution, Economics and Religion: Christian Political Economy, 1798–1833* (Cambridge: Cambridge University Press, 1991).

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the process "natural selection" (by analogy with the artificial selection practiced by breeders) and spent the rest of his life seeing how much it could explain. Such a law seemed to Darwin to render creation "far grander" than in Paley's and Owen's science, where God figured merely as "a man, rather cleverer than us."[30] But at the same time, natural selection made God remote.

*On the Origin of Species* (1859) was the last great work in the history of science for which theology was an active ingredient. The word "evolution" did not appear in the text (except once in the final edition), but Darwin used "creation" and its cognates over one hundred times. Opposite the title stood a quotation from Francis Bacon (1561–1626) about studying God's works as well as his Word, and one from Whewell on "general laws" as God's way of governing. On the last page, Darwin rhapsodized about the "grandeur" in his view of life, with nature's "most beautiful and most wonderful" diversity arising from "powers . . . originally breathed into a few forms or into one." Although this played to the audience, the tone and the terminology – even the biblical "breathed" – were sincere. For start to finish, the *Origin of Species* was a pious work: "one long argument" against miraculous creation, but equally a reformer's case for creation by natural law. That it dodged human evolution and the origin of life must be set against its personification of "Nature" as selector, and in later editions, inclusion of remarks by Bishop Joseph Butler (1692–1752) and "a celebrated author and divine," Rev. Charles Kingsley (1819–1875).[31] These features evince something of Darwin's own conflicted religious character.

While writing the *Origin of Species*, Darwin's faith in a "personal God" remained firm, and he never considered himself an atheist. What seemed incredible to him was Christian theism. A perpetual, designing Providence, present in all events, rendered "my deity, 'Natural Selection,' superfluous." And a God who punished men eternally for their unbelief was Himself, Darwin insisted, immoral. The deaths of his father in 1848 and ten-year-old daughter in 1851 embittered his loss of faith. Even so, he marveled at life's adaptations – the *Origin of Species* did not disprove design, only Paley's mechanical proofs – and wondered whether he should speak out on religion. But he differed poignantly with his wife on the subject and only confessed his beliefs in private. "Freedom of thought," he believed, was "best promoted by

[30] Francis Darwin, ed., *The Life and Letters of Charles Darwin, Including an Autobiographical Chapter*, 3 vols. (London: John Murray, 1887), vol. 3, p. 62. On Darwin and Malthus, see Dov Ospovat, *The Development of Darwin's Theory: Natural History, Natural Theology, and Natural Selection, 1838–1859* (Cambridge: Cambridge University Press, 1981); John Hedley Brooke, "The Relations between Darwin's Science and His Religion," in Durant, *Darwinism and Divinity*, pp. 40–75; David Kohn, "The Aesthetic Construction of Darwin's Theory," in *The Elusive Synthesis: Aesthetics and Science*, ed. A. I. Tauber (Dordrecht: Reidel, 1996), pp. 13–48.

[31] Morse Peckham, ed., *The Origin of Species by Charles Darwin: A Variorum Text* (Philadelphia: University of Pennsylvania Press, 1959), pp. 40 (*1.1:b*), 719 (4), 748 (*183.3:b*); David Kohn, "Darwin's Ambiguity: The Secularization of Biological Meaning," *British Journal for the History of Science*, 22 (1989), 215–39.

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the gradual illumination of men's minds, which follow[s] from the advance of science," not by confrontation. Having never "published a word directly against religion or the clergy," he died as he had lived, a respectable agnostic, and was buried in Westminster Abbey.[32] The church reclaimed its own.

## THE CONFLICT

The *Origin of Species* did not cause a "Darwinian revolution," destroying natural theology and propelling religion and science into unholy conflict. Darwin's imposing argument, backed by an impeccable reputation, merely pointed up and sharpened preexisting tensions.

Freethinkers everywhere welcomed the *Origin of Species* – the theology notwithstanding – as a potent addition to their liberal armory. Most read it through philosophical spectacles; Clémence Royer, the first French translator, infuriated Darwin by repackaging it as an anticlerical tract. Churchmen of the *Bridgewater* school, Genesis-and-geology harmonizers, and all scriptural geologists obliged by treating the *Origin of Species* as a bombshell, fatal to the design argument, morals, and God's Word – the theology again notwithstanding. But to a growing number of educated persons, the *Origin of Species* was simply honest science tackling the age-old mystery of species. They snapped it up by the thousands, read it critically, and within a decade were converting to creation by law. That law was seldom natural selection. Even godly men who understood and endorsed the theory, such as the Harvard University botanist Asa Gray (1810–1888), a moderate Congregationalist, could not fully reconcile themselves to Darwin's understanding of it. And Gray stood on the same ground as the non-Darwinian Richard Owen, a devout Anglican, and his followers, the Catholic anatomist St. George Mivart (1827–1900) and the Scots Presbyterian Duke of Argyll (1823–1900). To them all, life's law was a divine edict, not a bloody, blind, and stumbling struggle for existence.[33]

[32] F. Darwin, *Life and Letters of Charles Darwin*, vol. 2, p. 373, vol. 3, p. 236; Desmond and Moore, *Darwin*, pp. 635–6, 645; James Moore, "Darwin of Down: The Evolutionist as Squarson-Naturalist," in David Kohn, *The Darwinian Heritage* (Princeton, N.J.: Princeton University Press, 1985), pp. 435–81; James Moore, "Freethought, Secularism, Agnosticism: The Case of Charles Darwin," in *Religion in Victorian Britain*, vol. 1: *Traditions*, ed. Gerald Parsons (Manchester: Manchester University Press, 1988), pp. 274–310; James Moore, "Of Love and Death: Why Darwin 'Gave up Christianity'," in Moore, *History, Humanity and Evolution*, pp. 195–229; James Moore, *The Darwin Legend* (Grand Rapids, Mich.: Baker, 1994).

[33] Alvar Ellegård, *Darwin and the General Reader: The Reception of Darwin's Theory of Evolution in the British Periodical Press, 1859–1872* (Göteborg: Elanders Bocktryckeri Aktiebolag, 1958); Moore, *Post-Darwinian Controversies*, chaps. 9–12; Frederick Gregory, "The Impact of Darwinian Evolution on Protestant Theology in the Nineteenth Century," in Lindberg and Numbers, *God and Nature*, pp. 369–90; Jon H. Roberts, *Darwinism and the Divine in America: Protestant Intellectuals and Organic Evolution, 1859–1900* (Madison: University of Wisconsin Press, 1988); Gregory P. Elder, *Chronic Vigour: Darwin, Anglicans, Catholics, and the Development of a Doctrine of Providential Evolution* (Lanham, Md.: University Press of America, 1996).

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Fewer could accept the clear implication of the *Origin of Species*, drawn out by Darwin in *The Descent of Man* (1871), that humans had evolved by means of the same struggle. Again, objectors had authority on their side, not just Lyell in his *Geological Evidences of the Antiquity of Man* (1863) but Alfred Russel Wallace (1823–1913), the cofounder of natural selection. Wallace's view of human origins was as characteristically plebian as Lyell's was patrician. Both naturalists saw *Homo sapiens* standing above evolution, but Wallace, self-taught and socialist-influenced, believed in the equal elevation of all. Having abandoned the church as a youth, he took up mesmerism and discovered that ordinary men like himself could induce trances. Later, living among primitive tribes, Wallace realized that their intellectual capacities were no different from his own. In the 1860s, while attending séances, he decided that human minds had originated in the spirit world. Men of science scoffed at his credulity and churchmen thought him a heretic, but a conflict of religion and science this was not.[34]

That a conflict took place is undeniable, and "Religion and Science" is how Victorians characterized it. But the issues were more complex than they knew. The conflict was not just about doctrines or ideals – a "crisis of faith" – nor did real people marshal neatly beneath the banners of Religion on the one hand and Science on the other. Men of science, even non-Christians, professed themselves religious. Religious laymen, Christian or not, took pride in being scientific. Alliances were forged and fractured; conversions and defections, private pacts and public rifts, affected all sides. What set people at odds were a range of issues, practical as well as theoretical, empirical as well as metaphysical, social and political as well as ideological. At stake, finally, was the world's industrial order.[35] *Whose* science, *whose* religion, would best promote the progress that, all believed, mankind was enjoying? *Whose* science, *whose* religion, should be credited for the spectacular progress to date? Such questions wracked the intelligentsia of most European nations, their colonies, and the populous parts of North America during the late nineteenth century. The most resounding and ultimately decisive answers came from those who at the time began to call themselves "scientists."[36] And scientists at this time first legitimized their rising status with a dramatic new creation myth, "the conflict of Religion and Science."

[34] W. F. Bynum, "Charles Lyell's 'Antiquity of Man' and Its Critics," *Journal of the History of Biology*, 17 (1984), 153–87; Turner, *Between Science and Religion*, chap. 4; Oppenheim, *Other World*, chap. 7; Peter Raby, *Alfred Russel Wallace: A Life* (London: Chatto and Windus, 2001); Michael Shermer, *In Darwin's Shadow: The Life and Science of Alfred Russel Wallace; A Biographical Study on the Psychology of History* (New York: Oxford University Press, 2002).

[35] Moore, *Post-Darwinian Controversies*, chap. 4; James Moore, "Crisis without Revolution: The Ideological Watershed in Victorian England," *Revue de synthèse* (4th ser.), 107 (1986), 53–78; James Moore, "Theodicy and Society: The Crisis of the Intelligentsia," in Helmstadter and Lightman, *Victorian Faith in Crisis*, pp. 153–86.

[36] James Moore and Adrian Desmond, "Transgressing Boundaries," *Journal of Victorian Culture*, 3 (1998), 150–2. For public debates in the English-speaking world outside Britain, see Numbers and Stenhouse, *Disseminating Darwinism.*

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Debates erupted with peculiar force in the first industrial nation, a Britain at the apex of its imperial religiosity. Freethought became respectable in the 1860s; unwashed anticlericalism came clean, calling itself "agnostic." The key protagonists belonged to a guerrilla group of nine, known to themselves – with a casual disregard for Roman numerals – as the X Club. All but one of the members were under the age of forty when they first met; all but one had been elected Fellows of the Royal Society. All but one of the nation's top scientific offices fell to them within two decades, and over this period the X men dominated successively the British Association, the Royal Institution, and the Royal Society. In a land shielded from revolution and shrouded in evangelicalism, this was an intellectual palace coup. Wielding the sword of natural law, and with Darwin's private blessing, the insurgents drove out the old Paley-mongers and parson-naturalists – all who yoked science with God or Mammon – replacing them with single-minded professionals, a scientific brain trust for Britain's emerging industrial culture.[37]

Many saw this as science ousting religion, or Darwin versus the church, and the X Club made political capital of such imagery. But like Darwin himself, the Young Turks also presented themselves as reformers zealous for the truth. Martin Luther's Reformation was their model, not the sack of Rome. Best known of the X men were Thomas Henry Huxley (1825–1895), John Tyndall (1820–1893), and Herbert Spencer (1820–1903), an agnostic puritan, a pantheistic Orangeman, and a metaphysical Methodist, respectively.[38] Their potboilers and public speeches created an earthquake zone that sent intellectual tremors around the world. The Jena zoologist Ernst Haeckel (1834–1919) was jarred into action. His crusading *Darwinismus* backed German unification, an anti-Catholic *Kulturkampf*, and philosophical "monism" as linking what he called "religion and science." The unholy trinity themselves – Huxley, Tyndall, and Spencer – each toured the United States between 1872 and 1882, galvanizing scientists and religious liberals as well as home-grown freethinkers, themselves ripe for renewal in the unholy aftermath of the Civil War.[39] Draper's *Conflict* and White's *Warfare* followed in their train, typical

[37] Turner, *Between Science and Religion*, chap. 1; Turner, *Contesting Cultural Authority*, chaps. 6–7; Ruth Barton, "'Huxley, Lubbock, and Half a Dozen Others': Professionals and Gentlemen in the Formation of the X Club, 1851–1864," *Isis*, 89 (1998), 410–44, with references to literature; Josef L. Altholz, "A Tale of Two Controversies: Darwinism in the Debate over 'Essays and Reviews'," *Church History*, 63 (1994), 50–9; James Moore, "Deconstructing Darwinism: The Politics of Evolution in the 1860s," *Journal of the History of Biology*, 24 (1991), 353–408.

[38] Adrian Desmond, *Huxley: From Devil's Disciple to Evolution's High Priest* (London: Penguin, 1998); David Wiltshire, *The Social and Political Thought of Herbert Spencer* (Oxford: Oxford University Press, 1978); Ruth Barton, "John Tyndall, Pantheist," *Osiris* (2nd ser.), 3 (1987), 111–34; J. Vernon Jensen, "Return to the Wilberforce-Huxley Debate," *British Journal for the History of Science*, 21 (1988), 161–79, with references to literature.

[39] Paul Weindling, "Theories of the Cell State in Imperial Germany," in *Biology, Medicine and Society, 1840–1940*, ed. Charles Webster (Cambridge: Cambridge University Press, 1981), pp. 99–155; Paul Weindling, "Ernst Haeckel, Darwinismus and the Secularization of Nature," in Moore, *History, Humanity and Evolution*, pp. 311–27; Alfred Kelly, *The Descent of Darwin: The Popularization of Darwinism in Germany, 1860–1914* (Chapel Hill: University of North Carolina Press, 1981); Roberts,

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productions of an age when New World hubris took on Old World hauteur in the cause of Science with a capital "S."

## BEYOND "RELIGION AND SCIENCE"

In those halcyon days when scientists freed themselves from religious establishments, only skeptics dared prophesy a time when scientists might form establishments from which people would seek to be freed. Progress was palpable, the benefits of unfettered research self-evident. Science made up for lost religious hopes by promising endless secular abundance. The skeptics – Victorian antivivisectionists and antivaccinationists, Edwardian critics of racist anthropology and eugenics – seemed selfishly ungrateful. They lacked a progressive conscience; they failed to see how science allied with the state could ameliorate mankind. Knowledge institutionalized to this end did not threaten human values; only those who persisted in attacking science in the name of religion or some other irrationality did.[40]

After World War I, a new threat to science came from self-styled "fundamentalists," ordinary Americans angry that their most cherished beliefs were being undermined with their own tax dollars. These lowbrow skeptics resented an educational establishment that made beasts of men, taught human inequality, put eugenics in school textbooks, and portrayed life as a godless bloody struggle. Wasn't this the doctrine of German generals and the Bolsheviks, as well as doddery old Darwin? Didn't natural selection, at the time, have vastly more critics than friends? William Jennings Bryan, a progressive Democrat and pacifist who resigned as secretary of state in protest when the United States entered World War I, made the politics of evolution his personal crusade, and from 1922 until his death the "Great Commoner" carried fundamentalist America by a landslide. His last hurrah at the Scopes trial was, to evolutionists, the last gasp of a mindless fanaticism. Clarence Darrow, a progressive Democrat and Darwinian determinist who believed in science like Bryan believed in Christ, dubbed his opponent "the idol of all morondom."[41]

Liberal believers in science as the embodiment of tolerance, disinterestedness, and democratic values got their comeuppance in the depressed 1930s.

*Darwinism and the Divine in America*; James Moore, "Herbert Spencer's Henchmen: The Evolution of Protestant Liberals in Late Nineteenth-Century America," in Durant, *Darwinism and Divinity*, pp. 76–100; James Turner, *Without God, without Creed: The Origins of Unbelief in America* (Baltimore: Johns Hopkins University Press, 1985).

[40] Roy MacLeod, "The 'Bankruptcy of Science' Debate: The Creed of Science and Its Critics, 1885–1900," *Science, Technology, and Human Values*, 7 (1982), 2–15.

[41] George Marsden, *Fundamentalism and American Culture: The Shaping of Twentieth Century Evangelicalism, 1870–1925* (New York: Oxford University Press, 1980); Ferenc Morton Szasz, *The Divided Mind of Protestant America, 1880–1930* (Tuscaloosa: University of Alabama Press, 1982), chaps. 9–11; Larson, *Summer for the Gods*.

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Even before the slump set in, critics of laissez-faire capitalism were scorning an establishment gripped by Darwinian dogma. "To suggest social action for the public good to the City of London," John Maynard Keynes (1883–1946) fumed, "is like discussing the *Origin of Species* with a Bishop sixty years ago."[42] Soon enough it became clear that science would swing behind less liberal ideologies. A large section of the German scientific community garnered state support by sponsoring Darwinian policies of ethnic extermination. In the Soviet Union, industrialized, militarized, and committed to world domination on the basis of Marxist materialism, science and the state were one. Western liberals reacted in horror, declaring totalitarianism a greater threat to science than fundamentalism. Yet during World War II, and particularly with the mobilization of research to meet the postwar Soviet challenge, science in the West was harnessed to state objectives, tied to state funding, and subjected to state regulation as never before. By the height of the cold war, the "cultural situation for science," East and West, had become "the mirror image of that which once pertained to religion. A science-directed culture ... to a considerable extent replaced a church-directed culture. Scientific establishments ... achieved the privileged cultural positions once held by religious establishments." Little wonder that when neo-fundamentalists in the 1960s and 1970s developed a strategy for neutralizing the teaching of evolution in U.S. public schools, they called for more science – "Creation Science" – to be introduced.[43] Religion, to be credible, now had to present itself as scientific, just as science once had to demonstrate its religiosity.

At the end of the twentieth century, historians of "Religion and Science" subjects studied the past as self-conscious creatures of their time. "What happened" (to revert to G. M. Young's maxim) was not only acknowledged to include "what people felt about it when it was happening"; it was also assumed to be refracted through the experience of historians themselves – what happened to *them* in the late twentieth century and how *they* felt about it when it was happening. And as their experience was, in most respects, the opposite of the Victorians', the "real, central theme of History" ceased to be the antiphonal march of "Religion and Science." A more dissonant, less triumphant note was struck, one resounding to the clash of cultures and belief systems. Besides the critique of liberalism and the collapse of political faiths such as Soviet communism, it was the failure of secularization that shaped historians' outlook a century after Draper and White: the erosion of

[42] John Maynard Keynes, *The End of Laissez-Faire* (London: Leonard and Virginia Woolf at the Hogarth Press, 1926), p. 38.

[43] Frank M. Turner, "Science and Religious Freedom," in *Freedom and Religion in the Nineteenth Century*, ed. Richard Helmstadter (Stanford, Calif.: Stanford University Press, 1997), p. 85. See also Christopher P. Toumey, *God's Own Scientists: Creationists in a Secular World* (New Brunswick, N.J.: Rutgers University Press, 1994); James Moore, "The Creationist Cosmos of Protestant Fundamentalism," in *Fundamentalisms and Society: Reclaiming the Sciences, the Family, and Education*, ed. Martin E. Marty and R. Scott Appleby (Chicago: University of Chicago Press, 1993), pp. 42–72; Numbers, *Creationists.*

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belief in Science with a capital "S" accompanied by the rise of competing small "s" sciences, the growth of fundamentalisms, and the emergence of elite-sponsored scientific-religious worldviews.[44] The fact that, in this fragmenting context, many historians felt compelled to adjust or abandon their own, often deeply held commitments adds poignancy to their efforts to revise the old certainties about "Religion and Science."

Today historians aim to situate religion and science on cultural common ground and so recover the religiosity of science, the scientificity of religion, and the integrity of metaphysics occupying that large terra incognita "between science and religion" as traditionally conceived.[45] Indeed, real terrain is crucial to this task, as David Livingstone has argued in proposals to put space, place, and geography at the center of science–religion discussions. Adrian Desmond has shown how far these discussions have been skewed by Huxley's "holy war" against theology, a metaphorical war with a real-world punch that "the General" himself helped deliver by drilling army recruits in Darwinism, cultivating armaments manufacturers, and backing Britain's industrial "warfare" overseas.[46] Huxley's victims and other casualties of historiographic strife are also receiving their due, notably Richard Owen among scientists and die-hard divines such as John Henry Newman (1801–1890) and Charles Hodge (1797–1878), whose reflections on science now occasionally seem astute. Equally, subprofessional persons poised "between science and religion" – syncretists and idealists, adherents of mesmerism, spiritualism, and metaphysical systems from Hegelianism to Theosophy – are being reevaluated. They include not only scientific outsiders such as Wallace, but intellectual women – Mary Baker Eddy, Frances Power Cobbe, and Annie Besant – long excluded by both scientific and religious "clerical" establishments. Not least among the many drawbacks of the "Religion and Science" rubric is its tendency to perpetuate this exclusion in the field of history.[47]

44 Brooke and Cantor, *Reconstructing Nature*, chap. 2; James Gilbert, *Redeeming Culture: American Religion in an Age of Science* (Chicago: University of Chicago Press, 1997).

45 Turner, *Between Science and Religion*; James Moore, "Speaking of 'Science and Religion' – Then and Now," *History of Science*, 30 (1992), 311–23; David B. Wilson, "On the Importance of Eliminating 'Science and Religion' from the History of Science and Religion: The Cases of Oliver Lodge, J. H. Jeans and A. S. Eddington," in *Facets of Faith and Science*, vol. 1: *Historiography and Modes of Interaction*, ed. Jitse M. van der Meer (Lanham, Md.: Pascal Centre/University Press of America, 1996), pp. 27–47.

46 David N. Livingstone, "Science and Religion: Foreword to the Historical Geography of an Encounter," *Journal of Historical Geography*, 20 (1994), 367–83; Desmond, *Huxley*, pp. 632–6.

47 David N. Livingstone, "Situating Evangelical Responses to Evolution," in Livingstone, Hart, and Noll, *Evangelicals and Science in Historical Perspective*, pp. 193–219; Charles Hodge, *What Is Darwinism? and Other Writings on Science and Religion*, ed. Mark A. Noll and David N. Livingstone (Grand Rapids, Mich.: Baker, 1994); Alex Owen, *The Darkened Room: Women, Power, and Spiritualism in Late Victorian England* (Philadelphia: University of Pennsylvania Press, 1990); Lori Williamson, *Power and Protest: Frances Power Cobbe and Victorian Society* (London: Rivers Oram Press, 1998); David F. Noble, *A World without Women: The Christian Clerical Culture of Western Science* (New York: Knopf, 1992); Maureen McNeil, "Clerical Legacies and Secular Snares: Patriarchal Science and Patriarchal Science Studies," *The European Legacy*, 1 (1996), 1728–39.

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Perhaps the most telling recent development noted by historians is the vaunted convergence of religion and science in some new vision of reality whose scientific authority will command full religious and moral assent.[48] Although physics is the usual stalking horse for this emergent Religious Science, biology appears to serve just as well. Neo-creationists may be hard at work proving "intelligent design" from life's complex structures, but Darwinian atheists have been marveling at unintelligent design for years. Their creed is essentially theological, according to Stephen Jay Gould, who dubs these latter-day adaptationists "apostles of ultra-Darwinism" and "Darwinian fundamentalists."[49] Indeed, many ultra-Darwinian works clearly belong to that large historic literature that attempts to "find in science indications and proofs concerning ultimate questions of meaning and value." From Julian Huxley's *Religion without Revelation* (1927) to Richard Dawkins's *Blind Watchmaker* (1986), from the popularizations of G. G. Simpson, Garrett Hardin, C. H. Waddington, E. O. Wilson, and Daniel Dennett to the potboilers of evolutionary psychologists, a global intelligentsia now increasingly takes its science from "the Bridgewater Treatises of the twentieth century."[50] And with scientists promising to "play God" through molecular manipulation, redesigning nature and redefining human life, the new century may well see a renascence of natural theology in which a new Darwin will arise to prick its grand pretensions. "Religion and Science," long since abandoned by historians, would then manifestly have had its day.

48 Eileen Barker, "Science as Theology – The Theological Functioning of Western Science," in Peacocke, *Sciences and Theology in the Twentieth Century*, pp. 262–80; Richard C. Rothschild, *The Emerging Religion of Science* (New York: Praeger, 1989); Mary Midgley, *Science as Salvation: A Modern Myth and Its Meaning* (London: Routledge, 1992); David F. Noble, *The Religion of Technology: The Divinity of Man and the Spirit of Invention* (New York: Knopf, 1997).

49 Stephen Jay Gould, "On Transmuting Boyle's Law to Darwin's Revolution," in *Evolution: Science, Society and the Universe*, ed. A. C. Fabian (Cambridge: Cambridge University Press, 1998), pp. 24–5; Stephen Jay Gould, "Darwinian Fundamentalism," *New York Review of Books*, June 12, 1997, pp. 34ff. See Mary Midgley, *Evolution as a Religion: Strange Hopes and Stranger Fears* (London: Methuen, 1985); Howard L. Kaye, *The Social Meaning of Modern Biology: From Social Darwinism to Sociobiology* (New Haven, Conn.: Yale University Press, 1986); John R. Durant, "Evolution, Ideology and World View: Darwinian Religion in the Twentieth Century," in Moore, *History, Humanity and Evolution*, pp. 355–73; R. C. Lewontin, *Biology as Ideology: The Doctrine of DNA* (New York: Harper, 1992); John C. Avise, *The Genetic Gods: Evolution and Belief in Human Affairs* (Cambridge, Mass.: Harvard University Press, 1998); Thomas Dixon, "Scientific Atheism as a Faith Tradition," *Studies in History and Philosophy of Biological and Biomedical Sciences*, 33 (2002), 337–59.

50 John C. Greene, *Science, Ideology, and World View: Essays in the History of Evolutionary Ideas* (Berkeley: University of California Press, 1981), pp. 162–3.

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# 30

# BIOLOGY AND HUMAN NATURE

*Peter J. Bowler*

In traditional Christian thought, the soul and the body were distinct from one another: If behavior was affected by animal impulses, this merely indicated that the soul did not have sufficient control over its fleshly garment. Descartes' insistence that the mind existed on a separate plane from that of the body – the latter being conceived of essentially as a machine – continued the dualistic interpretation. In such a model, psychology and the social sciences would constitute a body of knowledge with no link to biology. The workings of the mind could be investigated by introspection without reference to the body. The dualistic perspective came under fire in the eighteenth century, as materialist philosophers such as Julien Offray de la Mettrie argued that the mind *was* affected by the body. They implied that the mind should be treated as nothing more than a by-product of the physical processes going on in the brain. For the materialists, human nature was essentially biological. The conflict between dualism and materialism was renewed in the nineteenth century as developments in biology began to offer a range of techniques for investigating human behavior. There was not, however, a complete triumph of the materialistic approach. Efforts to preserve the mind as a distinct level of activity have continued, partly in defense of the concept of the soul but increasingly as a means of creating a professional niche for psychology and the social sciences.

Every attempt to create a biologically founded account of human nature has been marked by controversy. The suggestion that aspects of our behavior are determined by biological processes has been seen as an assault on human dignity and moral responsibility. If the mind is merely a reflection of physical changes taking place in the brain, then perhaps it is the neurophysiologist, not the philosopher or the psychologist, to whom we should turn for advice on moral and social issues. And if the brain is a product of natural evolution, then a study of the evolutionary process should tell us why we are programmed to behave as we do. The late twentieth century witnessed a renewed assault on human nature by the biological sciences. The implications are as controversial



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as ever, and the history of earlier efforts to impose biology onto the human sciences may offer valuable insights and warnings.

Historians have focused a great deal of attention onto the crucial steps in biology's advance into this once-forbidden territory. These include attempts to show that human nature is dictated by the structure of the brain, by inherited limitations of intelligence or behavior patterns, or by the nature of the evolutionary process. Many of these debates are perceived as having both philosophical and ideological dimensions. The philosopher may argue that the brain is the organ of the mind, but it is the ideologue who uses that assertion to justify social actions such as the attempt to limit the reproductive capacities of persons alleged to have limited mental powers or dangerous instincts. Over the last several decades, historians have shown an increasing willingness to interpret many of the debates in ideological terms. The old assumption that science offers objective knowledge has broken down in many areas, but in none more evidently than this, where the human implications of scientific knowledge are so immediate. We have become increasingly sure that what passed for scientific knowledge at various points in the past was influenced (I do not say determined) by the social values of the time. As one influential voice in this movement has asserted, "Darwinism *is* social."[1] It is not a question of Darwinism being applied to society but of social images being built into the very fabric of science itself. The rise and fall of phrenology, an early theory of cerebral localization, was used as a case study by a pioneering member of the "Edinburgh school" – the most persistent advocates of the claim that scientific knowledge is socially constructed.[2] Scientists often resist this claim in defense of their objectivity, but if historians can show that earlier efforts to apply biology to the study of human nature were influenced by social values, the lessons should be studied by those engaged in modern efforts to continue the program.

Certain topics have attracted particular attention from historians, although some remain relatively unworked. Extensive literature exists on the cerebral localization of mental functions, on "social Darwinism," on theories alleging biological differences between the races, and on other forms of genetic determinism associated with "eugenics" (Francis Galton's term for a selective breeding program for the human species). Paleoanthropology, the science of human origins, has remained largely untouched by historians of science. But none of these areas are as distinct as they sometimes appear. They all depend on the belief that the brain controls behavior (see Harrington,

[1] Robert M. Young, "Darwinism *Is* Social," in *The Darwinian Heritage*, ed. David Kohn (Princeton, N.J.: Princeton University Press, 1985), pp. 609–38. See also Young's collected papers, *Darwin's Metaphor: Nature's Place in Victorian Culture* (Cambridge: Cambridge University Press, 1985).

[2] Steven Shapin, "Homo Phrenologicus: Anthropological Perspectives on a Historical Problem," in *Natural Order: Historical Studies of Scientific Culture*, ed. Barry Barnes and Steven Shapin (Beverly Hills, Calif.: Sage, 1979), pp. 41–79. See also Michael Mulkay, *Science and the Sociology of Knowledge* (London: Allen and Unwin, 1979); Stephen Yearley, *Science, Technology and Social Change* (London: Unwin Hyman, 1988).

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Chapter 27, this volume), although this may be forgotten when attention shifts to the evolutionary origins of particular behavior patterns. Alleged mental differences between races are only a special case of the more general claim that a person's character is controlled by heredity and cannot be modified by learning. Determinism itself often rests on assumptions about the role played by evolution in shaping the characters transmitted by heredity. The debate over the relative powers of "nature" and "nurture" in determining behavior raises a wide range of issues in the relationship between the biological and the social sciences. Modern theories such as sociobiology may thus combine influences from sources with separate origins in the development of biology. Recent studies stressing the role of gender in shaping scientists' assumptions about human nature also cut across the conventional boundaries.

## MIND AND BRAIN

The eighteenth-century materialists came into conflict with the prevailing view of the working of the mind, which focused on the "association of ideas" as the source of learning and habits. Much early nineteenth-century psychology was based on the assumption that the mind built up associations between sense impressions and memories, without reference to any physical processes in the brain. A major challenge to this dualistic psychology was mounted by the advocates of phrenology, led by Franz Josef Gall (1758–1828) and Johann Gaspar Spurzheim (1776–1832). Based on studies of cerebral anatomy and observed behavior, they postulated a series of distinct mental functions, each located in a particular area of the brain. Individual behavior was, in effect, determined by the structure of the brain – which was assumed to be detectable from the external form of the skull.[3] Phrenology achieved wide popularity in the 1820s and 1830s, although it was bitterly criticized by both philosophers and anatomists. A particularly intense debate took place in Edinburgh, where the champion of phrenology George Combe (1788–1858) linked it to a reformist social policy based on the claim that people could better control their lives if they knew their mental strengths and weaknesses.[4] Combe's *Constitution of Man* (1828) was one of the early nineteenth century's best-selling books.

Conventional accounts of phrenology dismiss it as a pseudoscience: The anatomists were quite right to point out that the fine structure of the brain

[3] For an account of Gall's work, see Robert M. Young, *Mind, Brain and Adaptation in the Nineteenth Century* (Oxford: Clarendon Press, 1970), chap. 1. A general survey that covers many of the topics mentioned in this chapter is Roger Smith, *The Fontana/Norton History of the Human Sciences* (London: Fontana; New York: Norton, 1997).

[4] Geoffrey Cantor, "The Edinburgh Phrenological Debate, 1803–1828," *Annals of Science*, 32 (1975), 195–218; Shapin, "Homo Phrenologicus"; Steven Shapin, "Phrenological Knowledge and the Social Structure of Early Nineteenth-Century Edinburgh," *Annals of Science*, 32 (1975), 219–43.

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is not reflected in the shape of the skull. Modern historians point out that so easy a dismissal in the light of hindsight fails to take account of the fact that phrenology's more fundamental claims were not so far-fetched. The development of cerebral localization later in the nineteenth century confirmed that some mental functions can be shown to take place in certain regions of the brain, so that damage to that region affects the corresponding function. To dismiss phrenology as nonsense is to repeat uncritically the views of its opponents at the time. In these circumstances, a more sophisticated analysis is required that asks: Who decides what is to be counted as scientific knowledge? The accounts by Steven Shapin and Roger Cooter show that phrenology was welcomed by those who stood to gain from the reformist social philosophy linked to it by Combe and others.[5] It was rejected by an academic establishment that sought to retain the traditional view of the human mind. Phrenology influenced many leading thinkers, including some who contributed to the later developments in cerebral anatomy. Its marginalization from academic science (so successful that the later vindications were not admitted as such) tells us more about the social processes that determine the attitudes of the scientific community than it does about the objective testing of theories.

Mid-nineteenth-century developments in neurophysiology confirmed that some mental functions do seem to depend on the proper functioning of a particular part of the brain. In 1861, Paul Broca (1825–1880) identified an area that, if damaged by a lesion, resulted in the loss of the ability to speak. In the 1870s, David Ferrier (1843–1928) and others began detailed work on cerebral localization. Significantly, Ferrier had been influenced by developments in philosophical psychology. The leading associationist psychologist Alexander Bain (1818–1903) included a chapter on neurophysiology in his book *The Senses and the Intellect* (1855), indicating his expectation that work on the activity of the nervous system would soon extend to the brain, providing insights into the physical foundations of psychological processes. In the same year, the philosopher Herbert Spencer's book *Principles of Psychology* adopted an evolutionary view of mental capacities. For Spencer, the individual mind was preshaped by the experiences of its ancestors: Learned habits became instinctive behavior patterns transmitted by heredity. Spencer's psychology depended on the Lamarckian theory of the inheritance of acquired characteristics – but the assumption that learned habits could be transmitted in turn depended on the belief that habits are determined by structures built up in the brain (and hence capable of being transmitted by biological inheritance).[6]

[5] Shapin, (note 4); Roger Cooter, *The Cultural Meaning of Popular Science: Phrenology and the Organization of Consent in Nineteenth-Century Britain* (Cambridge: Cambridge University Press, 1984). See also John Van Wyhe, *Phrenology and the Origins of Victorian Scientific Naturalism* (Aldershot: Ashgate, 2004).

[6] On these developments, see Young, *Mind, Brain and Adaptation in the Nineteenth Century*; Raymond E. Fancher, *Pioneers of Psychology* (New York: Norton, 1979), chap. 2; Robert J. Richards, *Darwin*

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Ferrier's work was extended by Sir Charles Sherrington (1857–1952), whose book *Integrative Action of the Nervous System* (1906) provided an overview of the coordinating action of the nervous system. But Sherrington was a dualist who avoided discussion of mental states. His work thus kept neurophysiology distinct from psychology and may have held back the latter's development as a science in Britain.[7] A far greater impact was made by exponents of scientific naturalism such as Thomas Henry Huxley (1825–1895) and John Tyndall (1820–1893), who argued that mental activity was merely a by-product of the physical activity of the brain. Although accepting that the mental world could not be reduced to the physical, they nevertheless insisted that the mind could not exert a controlling influence on the physical world. Twentieth-century developments in cerebral localization that confirmed the real but very complex nature of the relationship between mind and brain have gone largely unrecorded by historians.

Far more attention has focused on less direct legacies of phrenology. Evolutionists naturally welcomed the implication that as animals acquired bigger brains, their mental powers were enhanced. This link was made explicit in *Vestiges of the Natural History of Creation*, published anonymously in 1844 by the popular writer Robert Chambers (1802–1871).[8] Physical anthropologists, determined to show that nonwhite races were less intelligent than whites, began to use craniometry (the measurement of cranial capacity) as a means of arguing their case. Samuel George Morton (1799–1851) used a volumetric technique applied to empty skulls that, although flawed by modern standards, gave him the evidence he needed. Broca, too, applied craniometry to physical anthropology.[9] By the time Charles Darwin (1809–1882) popularized the theory of evolution, it could be taken almost for granted that the "lower" races were relics of earlier stages in humankind's progress, their primitive character confirmed by their smaller brains and less highly developed intellectual powers. This kind of physical anthropology has been largely purged from science, although its legacy continues to haunt popular debates.

A leading exponent of measurement applied to living human skulls was Francis Galton (1822–1911), the founder of the eugenics movement. Galton measured skulls as part of an effort to distinguish racial types – but he also introduced the systematic measurement of mental powers by testing large

*and the Emergence of Evolutionary Theories of Mind and Behavior* (Chicago: University of Chicago Press, 1987).

7 See Roger Smith, *Inhibition: History and Meaning in the Sciences of Mind and Brain* (London: Free Association Books, 1992), chap. 5.

8 See James A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation* (Chicago: University of Chicago Press, 2000). See also Secord's introduction to Robert Chambers, *Vestiges of the Natural History of Creation and Other Evolutionary Writings* (Chicago: University of Chicago Press, 1994).

9 Stephen Jay Gould, *The Mismeasure of Man* (New York: Norton, 1981). See also William Stanton, *The Leopard's Spots: Scientific Attitudes toward Race in America, 1815–1859* (Chicago: Phoenix Books, 1960); John S. Haller, *Outcasts from Evolution: Scientific Attitudes of Racial Inferiority, 1859–1900* (Urbana: University of Illinois Press, 1975); Nancy Stepan, *The Idea of Race in Science: Great Britain, 1800–1960* (London: Macmillan, 1982).

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numbers of subjects. Along with the physiological model used by Wilhelm Wundt (1832–1920), Galton's techniques helped to establish psychology as an experimental science.[10] Early twentieth-century applications of intelligence testing, which were also held to confirm the inferior mental powers of nonwhite races, were founded on similar techniques of mass testing.[11]

## EVOLUTION, PSYCHOLOGY, AND THE SOCIAL SCIENCES

During the early years of the debate over Darwinism, T. H. Huxley debated with his great rival Richard Owen (1804–1892) on the significance of the anatomical similarities between humans and apes. Huxley's *Man's Place in Nature* (1863) is popularly supposed to have established the closeness of the relationship, especially in the structure of the brain. But far more than anatomical relationships were at stake. Darwin avoided discussion of human origins in his *On the Origin of Species* because he realized how controversial the topic would be. Did the relative increase in the size of the human brain explain the emergence of the human mind, with the rational and moral powers that were once thought to distinguish us from the brutes? An evolutionary perspective on the mind had been developed and published by Herbert Spencer even before Darwin's writings. By the time Darwin issued his book *The Descent of Man* in 1871, he could draw on a number of studies that had begun to explore the implications of evolutionism for the emergence of the human mind and the development of society. The late nineteenth century saw a flowering of interest in evolutionary models within the human sciences. Some of these models stressed the role of the struggle for existence as the motor of progress and have been widely labeled as "social Darwinism." Historians have debated the nature and influence of social Darwinism and have also disagreed over its dependence on Darwin's biological theory. Some evolutionary models certainly contained elements that were not derived directly from Darwinism.

Darwin himself adopted a materialist view of the mind from the start of his evolutionary research. He was particularly interested in the origin of instincts, treating these as behavior patterns that had become imprinted on the brain by the process of evolution. Spencer adopted the Lamarckian view that learned habits could be transformed into hereditary instincts by the inheritance of acquired characters. But Darwin realized that natural selection could also modify instincts as long as there was some variation within the behavior pattern. In *The Descent of Man*, he explained the origin of social instincts via both Lamarckism and the process of group selection (competition in which

[10] Kurt Danziger, *Constructing the Subject: Historical Origins of Psychological Research* (Cambridge: Cambridge University Press, 1990).
[11] See Gould, *Mismeasure of Man.*

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the groups with the strongest social instincts survive). For Darwin, it was human efforts to rationalize the instincts governing our social interactions that were the basis of all ethical systems.[12]

Darwin accepted that, in the long run, evolution had steadily increased the level of animal intelligence – although he knew that many branches of the tree of life did not progress toward higher levels of development. He offered a specific theory to explain why humans had developed a level of intelligence so much higher than that of our closest relatives, the great apes. The majority of evolutionary psychologists, however, had little interest in the possibility that there might have been a crucial turning point in human evolution. They sketched in a putative scale of mental development running through the animal kingdom to humankind and then assumed that evolution would almost inevitably have advanced steadily up the scale. This approach can be seen in the work of George John Romanes (1848–1894), who became, in effect, Darwin's heir apparent in the area of mental evolution. In the United States, evolutionary models of the mind were proposed by James Mark Baldwin (1861–1934) and G. Stanley Hall (1844–1924).

Darwin and Romanes exaggerated the mental powers of animals to minimize the gulf that evolution had to bridge to the human mind. The "canon" proposed by Conway Lloyd Morgan (1852–1936) in his book *Introduction to Comparative Psychology* (1895) is supposed to have warned psychologists against this tendency to anthropomorphize animal behavior, although Morgan himself was an evolutionist. He later developed his theory of "emergent evolution," in which mind and spirit were new categories that emerged unpredictably at certain stages of evolution.[13] This theory challenged the view that mind was merely an epiphenomenon or by-product of the activities of the material universe. Once it had emerged, mind had an active role to play in the universe. In this respect, Lloyd Morgan tried to sustain the view held by the late nineteenth-century "psycho-Lamarckians," who had insisted that the inheritance of acquired characteristics had a moral advantage over Darwinian natural selection because it allowed consciously chosen habits to direct the evolution of a species. Baldwin introduced the concept of "organic selection" in 1896 in an attempt to show that natural selection, too, could be directed along channels predetermined by habit.[14]

An important element within late nineteenth-century developmental theories was the concept of recapitulation: the belief that the evolutionary history of the species is recapitulated in the development of the individual

[12] For a detailed account of Darwin, Spencer, and other evolutionists' accounts of the origin of the mind, see Richards, *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior*.

[13] Conway Lloyd Morgan, *Emergent Evolution* (London: Williams and Norgate, 1923). See also David Blitz, *Emergent Evolution: Qualitative Novelty and the Levels of Reality* (Dordrecht: Kluwer, 1992).

[14] On the background to Lamarckism and the Baldwin effect, see Richards, *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior*, especially chaps. 6, 8, and 10; Peter J. Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900* (Baltimore: Johns Hopkins University Press, 1983), chaps. 4 and 6.

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organism. In biology, this was promoted by Ernst Haeckel (1834–1919), who coined the phrase "ontogeny recapitulates phylogeny," and by American neo-Lamarckians such as Edward Drinker Cope (1840–1897). The recapitulation theory offered a model of evolution in which progress toward the goal of increasing maturity seemed inevitable. Evolutionary psychologists were convinced that the development of the individual human mind passed through the phases of mental evolution that had marked the evolution of the animal kingdom. Romanes explicitly identified the mental capacity of the child at certain ages with various levels of animal mentality. This model encouraged the belief that "savage" races, assumed to be relics of the earlier stages in the advance from the apes, had minds equivalent to those of white children. Cesare Lombroso (1835–1909) proposed a system of "criminal anthropology" in which criminals had minds that were relics of earlier stages in human evolution.[15]

Although recapitulationism waned in the early twentieth century, it played a role in the creation of several important theories that have been seen as characteristically modern. Frank Sulloway and others have noted that the psychology of Sigmund Freud (1856–1939), for all its disturbing implications, was firmly grounded on an evolutionary model in which the mind consists of layers corresponding to levels of animal mentality. Freud's revolutionary insight was that the integration of these levels in the individual's development was a process fraught with danger. In this respect, at least, the confident progressionism of nineteenth-century evolutionism was challenged. Significantly, given the long association between recapitulation theory and Lamarckism, Freud remained loyal to the latter theory throughout his career. The same combination of biological ideas can be seen in the work of another eminent psychologist, Jean Piaget.[16]

All of these models of mental evolution were based on the assumption that development consisted in the ascent of a scale of increasing maturity. The same model emerged independently in late nineteenth-century anthropology. Although histories of anthropology once assumed that this evolutionary perspective was stimulated by the Darwinian revolution, modern studies tend to see the two developments as parallel manifestations of the same cultural values. Evolutionary anthropologists such as Edward B. Tylor (1832–1917) and Lewis H. Morgan (1818–1881) assumed that modern "savages" were relics of the stage of cultural development through which the ancestors of

[15] The evolutionary psychologists' use of the recapitulation theory is described in Richards, *Darwin and the Emergence of Evolutionary Theories of Mind and Behavior*, especially chap. 8, and John R. Morss, *The Biologizing of Childhood: Developmental Psychology and the Darwinian Myth* (Hove: Erlbaum, 1990). More generally on the influence of the theory – including Lombroso – see Stephen Jay Gould, *Ontogeny and Phylogeny* (Cambridge, Mass.: Harvard University Press, 1977), chap. 5.

[16] See Frank Sulloway, *Freud: Biologist of the Mind* (London: Burnett Books, 1979); Lucille B. Ritvo, *Darwin's Influence on Freud* (New Haven, Conn.: Yale University Press, 1990); Richard Webster, *Why Freud Was Wrong: Sin, Science and Psychoanalysis* (London: HarperCollins, 1995). On Piaget's recapitulationism, see Morss, *Biologizing of Childhood*, chap. 4.

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the white race had passed in prehistoric times. Their inspiration lay in the new discoveries by archaeologists that, from the 1860s onward, confirmed the vast antiquity of the human race and created the notion of a primitive "stone age."[17] All living cultures were assigned a position on a scale of development culminating in modern industrial civilization. Cultural differences were explained not by divergent evolution but as differences in the level of development along a single scale. At first, the anthropologists resisted the claim that the more "primitive" peoples were *mentally* inferior to the whites, but as the century progressed it became increasingly difficult for them to separate mental development from cultural development.[18]

Herbert Spencer's philosophy of evolution firmly linked mental development with cultural and social development. Spencer's psychology had stressed that there was no universal "human nature" – the human mind was shaped by its social environment, and the more stimulating the environment, the greater the level of individual mental development. Conversely, the greater the level of individual intelligence, the faster society would progress, creating a feedback loop between mental and social evolution. In this model, it was inevitable that those races that preserved a "primitive" level of technology (assumed to mark a primitive level of social structure) must also be stuck at a lower stage of mental evolution. Savages were biological as well as cultural relics of the past, preserving apelike – and childlike – levels of mentality.

But what was the driving force of mental and social evolution? In Darwin's theory of natural selection, change results from the elimination of the unfit in a struggle for existence, leaving the fittest individuals to survive and breed. There were certainly many "social Darwinists" who proclaimed that struggle was the motor of progress. But to suppose that Darwin's theory was transferred from biology to society is – as far as some historians are concerned – to put the cart before the horse. We know that Darwin himself was directly influenced by Thomas Robert Malthus's (1766–1834) principle of population expansion, a classic product of free-enterprise economic thinking. This leads historians such as Robert M. Young to argue that ideological values were built into the heart of scientific evolutionism.[19] It is hardly suprising, then, that Darwin's theory was used to legitimize the ideology on which it was built by arguing that society should be based on "natural" principles.

Much has been written on the vogue of "social Darwinism" in the late nineteenth century, with Spencer presented as the leading advocate of the claim

[17] See Donald Grayson, *The Establishment of Human Antiquity* (New York: Academic Press, 1983); A. Bowdoin Van Riper, *Men among the Mammoths: Victorian Science and the Discovery of Human Prehistory* (Chicago: University of Chicago Press, 1993).

[18] On developments in nineteenth-century anthropology, see J. W. Burrow, *Evolution in Society: A Study in Victorian Social Theory* (Cambridge: Cambridge University Press, 1966); George W. Stocking, Jr., *Victorian Anthropology* (New York: Free Press, 1987); Peter J. Bowler, *The Invention of Progress: The Victorians and the Past* (Oxford: Blackwell, 1989).

[19] See Young, "Darwinism *Is* Social"; Young, *Darwin's Metaphor*.

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that the free-enterprise system generated progress through struggle. Successful capitalists certainly justified the system by appealing to the metaphor of the survival of the fittest. The conventional view – supported by at least one recent study – is that this claim was inspired by Darwinism. Some historians have urged caution, however, noting that the term "social Darwinism" was introduced by writers opposed to the view that struggle should play a role in human affairs. Their use of this term has highlighted Darwinism's involvement, and there is no doubt that the selection theory was part of this ideology. But natural selection was by no means the only biological mechanism exploited in this way. Other theories, especially Lamarckism, were caught up in the enthusiasm for progress by struggle. The term "social Darwinism" may be a convenient label for this whole movement, but it can be misleading if it is thought to imply that what modern biologists single out as Darwin's most important insight was the central theme of late nineteenth-century social thought.[20]

Lamarckism has gained a reputation as a theory that could more easily be used by the opponents of ruthless social policies. Lamarckians such as Lester Frank Ward (1841–1913) believed that their theory offered a humane route to social progress: If children were taught appropriate social behavior, the resulting behavior patterns would eventually become inherited instincts. The human race itself would thus become more socialized. But Lamarckism also played a role in promoting the recapitulation theory, with its strong emphasis on the inferiority of "primitive" mentalities. Spencer himself was a Lamarckian and had developed both a social and a biological evolutionism on this basis even before Darwin published his theory. Spencer took up the idea of natural selection after reading Darwin – he coined the emotive phrase "survival of the fittest" – but he never relinquished his support for Lamarckism as the primary mechanism of biological evolution. His enthusiasm for free enterprise and the role of struggle was at least in part driven by his belief that a competitive society provided the best stimulus to individual self-improvement – and the hope that such improvements could be transmitted to future generations. Much of the support for social Darwinism came from writers who could not clearly distinguish between the Spencerian model of self-improvement and the Darwinian model of selection. Later in the nineteenth century, the focus of attention switched from individual competition to national and racial rivalries.

[20] The classic expression of the view that Darwinism played a dominant role is Richard Hofstadter's *Social Darwinism in American Thought*, revised ed. (Boston: Beacon Press, 1955). More recently, see Mike Hawkins, *Social Darwinism in European and American Thought, 1860–1945: Nature as Model and Nature as Threat* (Cambridge: Cambridge University Press, 1997). See also Greta Jones, *Social Darwinism in English Thought* (London: Harvester, 1980). For a critique of Hofstadter, see Robert C. Bannister, *Social Darwinism: Science and Myth in Anglo-American Social Thought* (Philadelphia: Temple University Press, 1979). For a revisionist account of Spencer's social thought, see Mark Francis, *Herbert Spencer and the Invention of Modern Life* (Stocksfield, U.K.: Acumen, 2007).

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HUMAN ORIGINS AND SOCIAL VALUES

Evolution theory threw particular emphasis onto the question of how the human race itself had emerged, and theories relating to this topic were especially susceptible to influence by social values. Historians have focused on the ways in which scientific theories of human origins were used to dismiss nonwhite races as inferior, and more recent scholarship has also highlighted the role played by gender in the underlying assumptions of what was for a long time a male-dominated territory.

In the early 1860s, geologists realized that stone-age humans dated back at least to the last ice age, giving credence to the view that modern peoples with low levels of technology were relics of this distant past. As yet, however, there were few human fossils, only the stone tools. The first Neanderthal fossil, discovered in 1857, was at first highly controversial. Even Huxley denied its significance for the study of human origins, despite the heavy brow ridges that gave the skull an apelike appearance. By the 1890s, however, more Neanderthal specimens had been found, and the conviction grew that here was an early race of humans that had preserved some apelike characters – the Neanderthals were the "missing link." This view was reinforced by the discovery of "Java man" (*Pithecanthropus erectus*, now *Homo erectus*) in 1891–2, in which the brain size was intermediate between the ape and the modern human. The debates over the notorious Piltdown discoveries of 1912 – later shown to be fraudulent – were a classic product of the new science of paleoanthropology. Modern accounts of fossil hominids often begin with an outline of the early debates, although this literature has developed largely in isolation from historical analysis of evolutionism. The Piltdown affair has become the focus of a minor literary industry dedicated to uncovering the true culprit. Few historians of science have ventured into this territory, and the extent to which the interpretation of fossils was shaped by prevailing evolutionary theories has thus gone largely unrecorded.[21]

The reluctance of historians to tackle this material is surprising, given that the paleoanthropological debates reveal clear evidence of ideological influences. The theories of mental and social development discussed earlier were based on a developmental model that stressed the steady ascent of a progressive scale. By the 1890s, the paleoanthropologists had constructed a similar model in which *Pithecanthropus* and the Neanderthals were steps in the

[21] Exceptions are Peter J. Bowler, *Theories of Human Evolution: A Century of Debate, 1844–1944* (Baltimore: Johns Hopkins University Press, 1986); Bert Theunissen, *Eugene Dubois and the Ape-Man from Java: The History of the First "Missing Link" and Its Discoverer* (Dordrecht: Kluwer, 1989). For popular accounts of the fossil discoveries, see John Reader, *Missing Links: The Hunt for Earliest Man* (London: Collins, 1981); Roger Lewin, *Bones of Contention: Controversies in the Search for Human Origins* (New York: Simon and Schuster, 1988). A later but controversial contribution to the Piltdown debate is Frank Spencer, *Piltdown: A Scientific Forgery* (London: Natural History Museum; Oxford: Oxford University Press, 1990).

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ascent from the apes to the modern white races (nonwhites being relegated to lower rungs just above the Neanderthals). The emphasis was on the presumed continuity of the process by which the brain and the level of intelligence had enlarged. In *The Descent of Man*, Darwin had partially challenged this model by arguing that the adoption of an upright posture was a key breakthrough separating the line of human evolution from that of the apes. Those apes that ventured out onto the open plains had stood upright and had begun to use their hands; their use of tools had then stimulated the development of greater intelligence. By constructing an adaptive scenario based on the transition from tree-dwelling to living on the open plains, Darwin made the expansion of the human brain seem much less the inevitable product of a progressive trend. His insight went largely unnoticed in the age of developmental evolutionism, however, and as late as 1912 the theory proposed by Grafton Elliot Smith (1871–1937) still treated bipedalism as a consequence of increasing intelligence. Humans were the inevitable product of the main trend in primate evolution (brain expansion), not the unlikely consequence of a unique combination of circumstances.[22]

The initial lack of interest in the discovery of the *Australopithecus* fossil by Raymond Dart (1893–1988) in 1924 can be attributed to its incompatibility with the belief that brain expansion was the driving force of human evolution. The creature had evidently walked at least partially upright, yet had a brain no larger than an ape's. Recognition that bipedalism was indeed an ancient characteristic of the hominid line did not come until the discovery of more *Australopithecus* specimens in the late 1930s. Significantly, this was also the period in which the theory of natural selection became dominant in evolutionary biology, casting doubts on the earlier faith in the inevitability of progress. For modern paleoanthropologists, brain expansion cannot be taken for granted; indeed, explaining why it has happened at all is the greatest problem.

Although the developmental model survived into the twentieth century, it underwent a transformation in the period around World War I. Instead of being treated as rungs in the ladder of ascent from the apes, the Neanderthals and other fossil hominids were now dismissed as extinct side branches of human evolution. This was not, as has sometimes been alleged, a rejection of the evolutionary model – contemporary paleontologists were convinced that the evolution of most groups proceeded through the parallel advance of many species in the same direction. The developmental model had become more sophisticated, allowing for differential rates of advance. The Neanderthals were not our ancestors; they were an independent line of hominid evolution that had not advanced so far up the scale of mental development.

[22] Charles Darwin, *The Descent of Man and Selection in Relation to Sex*, 2 vols. (London: John Murray, 1871), vol. 1, pp. 138–45; Grafton Elliot Smith, *The Evolution of Man: Essays* (London: Humphrey Milford, 1924), p. 40. See Bowler, *Theories of Human Evolution*, chap. 7.

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The "expulsion of the Neanderthals" from the ancestry of modern humans matched early biogeographers' fascination with the possibility that waves of superior types could spread out from a center of rapid evolution, driving earlier, less advanced species to extinction. The resonance of this model with the rhetoric of imperialism is easy to demonstrate.[23] The question of Neanderthal extinction has again become controversial in modern paleoanthropology, thanks to genetic evidence suggesting the comparatively recent origin of all modern humans in Africa.

Biology was also called in to define the characters that proclaimed the white race's superiority. We have already noted that from the mid-nineteenth century onward, physical anthropologists had attempted to rank the living races into a hierarchy based on average brain size, with the whites at the top. Although at first resisted by cultural anthropologists, attempts to define each race in terms of distinctive biological and mental characters became steadily more popular. The anatomist Robert Knox (1791–1862) insisted that each race had distinct mental characters, and by the 1860s both Paris and London had anthropological societies devoted to the study of racial differences. Archaeologists also stressed the successive appearances of different racial types in Europe.[24]

Many post-Darwinian evolutionists were happy to use the evidence for differences in brain capacity to confirm that the nonwhite races were relics of the evolutionary past. The growing emphasis on parallel evolution allowed a different interpretation to be put on the same alleged phenomenon: The "lower" races were not primitive relics of the whites' past but parallel lines of evolution that had not advanced so far up the scale. This interpretation supported the widely held view that the races were different biological species. The extinction of the Neanderthals by an invasion of truly modern humans was an early example of a process that had gone on throughout prehistory – and would be repeated in the modern world wherever a higher race invaded the territory of a lower one.

These developments throw light on the involvement of biological theories in Europeans' evaluation of other races. Historians have explored the ways in which science was used in an attempt to provide legitimacy for the assumption that nonwhite races were mentally inferior. That science was used in this way is beyond question; the real issue confronting historians is the extent to which these concerns shaped the development of science itself. The sociological perspective supposes that scientific knowledge reflects the

[23] See Michael Hammond, "The Expulsion of the Neanderthals from Human Ancestry: Marcellin Boule and the Social Context of Scientific Research," *Social Studies of Science*, 12 (1982), 1–36; Bowler, *Invention of Progress*, chap. 4. On biogeography and the metaphors of imperialism, see Peter J. Bowler, *Life's Splendid Drama: Evolutionary Biology and the Reconstruction of Life's Ancestry, 1860–1940* (Chicago: University of Chicago Press, 1996), chap. 9.

[24] On science and the race question, see the works by Gould, Stanton, Haller, and Stepan cited in footnote 10 and Bowler, *Theories of Human Evolution*.

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ideological interests of those who produce it. Theories were constructed in a way that would maximize their ability to lend support to prejudices such as the assumption of white racial superiority. The wave of enthusiasm for theories of racial differentiation coincided with the age of imperialism, and this ideology almost certainly shaped the thinking of those scientists who dismissed other races as inferior. Historians have become wary, however, of adopting a determinist approach in which a particular ideology necessarily generates a particular scientific theory. Many different theories were adapted to the same social purpose, and this leaves the historian looking for other reasons why the scientists involved chose their particular theories. Most of the different evolutionary theories proposed in the late nineteenth and early twentieth centuries contributed to race science, Darwinian and non-Darwinian alike.

The evolutionary models based on the alleged inferiority of nonwhite races remained popular through the early twentieth century. They have never been eliminated completely from science, but they suffered a massive loss of influence in the middle decades of the century, in part because of the excesses of the Nazi regime in Germany. Scientific factors were also at work, however: The rise of the genetical theory of natural selection undermined the theories of parallel evolution that had been used to proclaim the distinct character of races and at the same time emphasized the genetic affinity between all modern humans. Even so, many scientists at first resisted the trend, and historians will continue to debate the extent to which science contributed to, or was driven by, social attitudes.[25]

## BIOLOGY AND GENDER

One aspect of the debates over paleoanthropology leads us toward a new theme: the insistence by modern feminist scholars that science has tended to present a masculine view of nature. Late twentieth-century studies of primate behavior, often undertaken to throw light on human origins, were marked by the unusual prominence of female researchers. Some, including Dian Fossey and Jane Goodall, achieved international reputations. Their impact highlighted the fact that here, as apparently nowhere else in science, women had been able to play a major role. Scholars such as Donna Haraway began to ask whether the input from women influenced the way the data were interpreted, suggesting that there was clear evidence of a tension between a predominantly male-oriented vision of the world and an (all too rarely presented) female alternative. This feminist historiography raised the question "Is science sexist?" at several levels. Most obviously, it asked why women were normally excluded or discouraged from participating in scientific research.

[25] See Elazar Barkan, *The Retreat of Scientific Racism: Changing Concepts of Race in Britain and the United States between the World Wars* (Cambridge: Cambridge University Press, 1992).

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Most scientists might have admitted the exclusion but would have insisted (a) that this was a problem equivalent to that faced in other activities where women had been excluded for social reasons and (b) that it had no effect on the way the science was actually done. The feminists have argued that the exclusion of women reflects a deeply masculine bias in the way science approaches nature. Women are not only denied access; they are put off by what they see as a way of looking at the world that almost always seems to favor the emergence of theories that reflect masculine values and that necessarily alienate women from the field. In this model, we could have a very different science (i.e., a very different view of the world) if women were able to contribute their very different perspective to the activity.

In her study of primatology, Donna Haraway has shown how Sherwood Washburn's image of "man the hunter" shaped paleoanthropological theories to promote male images of what defines humanity.[26] By focusing on a male activity as the principal stimulus in the evolution of humankind, female values are sidelined in evolution and, by implication, in what it means to be human. Feminist paleoanthropologists later sought to undermine this gender bias by challenging the plausibility of the claim that our distant ancestors were big-game hunters and by stressing the possible significance of female activities such as food gathering in stimulating the development of bipedalism and increased social activity.

Feminist historians have also highlighted the problem of science's masculine bias by pointing to the difficulties faced by the few women who manage to force themselves to the top rank of researchers. Primatologists such as Fossey and Goodall were able to sidestep the normal "glass ceiling" because they worked in an area where intense public interest allowed them to gain influence without passing through the normal channels of technical publication. But in most other areas of science, women have found it hard to reach the top because their work has been hindered or even marginalized by their male colleagues. Rosalind Franklin, whose x-ray diffraction pictures gave a vital clue to the double-helix structure of DNA, was frequently obstructed by her colleagues and was later dismissed as an irrelevance in James Watson's self-centered account of the discovery.[27] In the case of the geneticist Barbara McClintock, it has been argued that a female perspective led to the uncovering of a phenomenon (genetic transposition, where genes can move between chromosomes) that had been ignored by male geneticists because it did not fit into their hard-line determinist view of how the gene controls the development of the organism.[28]

[26] Donna Haraway, *Primate Visions: Gender, Race and Nature in the World of Modern Science* (London: Routledge, 1990).

[27] Brenda Maddox, *Rosalind Franklin* (London: HarperCollins, 2002); James D. Watson, *The Double Helix* (New York: Atheneum, 1968).

[28] Evelyn Fox Keller, *A Feeling for the Organism: The Life and Times of Barbara McClintock* (San Francisco: W. H. Freeman, 1983).

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This case points us toward the more controversial possibility that scientific knowledge itself may be influenced by the values of those who construct it. Feminist scholars argue that some theoretical perspectives, of which genetic determinism is one, reflect the male fascination with models of control rather than harmonious interaction. Darwin's theory of natural selection is seen as reflecting the masculine ideal of competition at the expense of cooperation. These are controversial claims, but there seems little doubt that throughout the history of science theories have been presented in ways that reinforce masculine values. Whether the theories actually embody those values or have merely been distorted to give an appearance of upholding them is the crucial point of issue.

Feminist scholars have certainly identified a wide range of areas within the biological sciences where theories have been used to depict women as inferior to men.[29] In many respects, these applications parallel those (discussed earlier) where biology was called in to defend the claim that the white race was mentally and morally superior to the rest of humanity. Medical writers treated the female body as a pathological modification of the "normal" male type, and the term *hysteria* became common to describe the mental imbalances supposedly caused by the effects of the female reproductive organs on the brain.[30] Anatomists, including T. H. Huxley, held that the female brain was less complex than that of the male, and Huxley refused to support efforts to open medical education to women. A whole generation of evolutionists shared the belief that women were endowed with a mentality that adapted them to raising children but made them unable to cope with life outside the family. Darwin's theory of sexual selection can be seen as one way in which Victorian gender roles were imposed on nature, but it was certainly not the only one. Herbert Spencer argued that the energy of the female body was diverted from the brain to the reproductive system, so that any attempt to educate women to take part in the harsh world of work and the professions would diminish their ability to bear children and threaten the future of the human race.[31]

[29] See Brian Easlea, *Science and Sexual Oppression: Patriarchy's Confrontation with Women and Nature* (London: Weidenfeld and Nicolson, 1981); Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven, Conn.: Yale University Press, 1985); Ludmilla Jordanova, *Sexual Visions: Images of Gender in Science and Medicine* (Hemel Hempstead: Wheatsheaf, 1989); Cynthia Eagle Russet, *Sexual Science: The Victorian Construction of Womanhood* (Cambridge, Mass.: Harvard University Press, 1989).

[30] J. M. Masson, *A Dark Science: Women, Sexuality and Psychiatry in the Nineteenth Century* (New York: Farrar, Straus and Giroux, 1986); Elaine Showalter, *The Female Malady: Women, Madness and English Culture, 1830–1980* (New York: Pantheon, 1986).

[31] On Huxley and Darwin, see Evelleen Richards, "Huxley Finds Man, Loses Woman: The 'Woman Question' and the Control of Victorian Anthropology," in *History, Humanity and Evolution: Essays for John C. Greene*, ed. J. R. Moore (Cambridge: Cambridge University Press, 1989), pp. 253–84; Evelleen Richards, "Darwin and the Descent of Woman," in *The Wider Domain of Evolutionary Thought*, ed. D. R. Oldroyd and Ian Langham (Dordrecht: Reidel, 1983), pp. 57–111. More generally, see Lorna Duffin, "Prisoners of Progress: Women and Evolution," in *The Nineteenth-Century Woman: Her Cultural and Physical World*, ed. Sara Delamont and Lorna Duffin (London: Croom Helm, 1978), pp. 57–91.

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It would be easy to dismiss these ideas as distortions of science brought about by Victorian male prejudice, but feminist historians see them as manifestations of a deeper problem in which scientific thought, and even the scientific method itself, are pervaded by male values. The whole idea that nature must be probed by experiment is seen as embodying the masculine view of domination, as does the application of science to control the natural world. We have already noted claims that both genetic determinism and the theory of natural selection manifest the same masculine way of thinking. On this basis, the Victorian efforts to use science as a means of defining women's subordinate place in society are not aberrations – they are merely the surface manifestations of a much deeper problem that can only be overcome when women become strong enough within the scientific community to generate a more interactionist view of both the scientific method and natural processes. Critics may consider efforts to dismiss successful theories such as natural selection as the products of ideological misrepresentation. Are we, as Brian Easlea seems to imply, supposed to revive the discredited Lamarckian theory because we distrust the wider implications of Darwinism?

## HEREDITY AND GENETIC DETERMINISM

The conviction of many nineteenth-century thinkers that a person's level of ability was predetermined by sex or by racial origin represents the first wave of support for biological or hereditary determinism. Liberal thinkers at first protested against this claim, arguing that background and education played the major role in shaping personality and ability. This difference of opinion fed into the celebrated and apparently never-ending debate over the relative significance of nature and nurture in the determination of character. But a major change took place in the structure of this debate in the late nineteenth century. It was increasingly argued that, even within a single race, there were individual differences that were predetermined by each person's ancestry. Levels of ability, and perhaps even temperament, were transmitted by inheritance from parent to offspring, and individuals born with a "poor" heritage were doomed to inferiority whatever education they received. This development within social opinion coincided with a massive focusing of biologists' attention on the topic of heredity, leading historians once again to ask about the role played by ideology in shaping scientific knowledge.

This new wave of hereditarianism was pioneered by Darwin's cousin, Francis Galton. On a trip to Africa, Galton became convinced of the inferiority of the black races. He then began to argue that the hereditarian principle was applicable even within the white race: Intelligent people had intelligent children, and by implication stupid people had stupid children. Galton's book *Hereditary Genius* (1869) laid the foundations for what became a political campaign to avert the dangers that might flow from ignoring this alleged

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biological fact. Galton argued that in a modern society the "unfit" were no longer removed by natural selection but survived and bred rapidly, thereby increasing the level of poor heredity in the population. Galton coined the term "eugenics" to denote a program designed to improve the character of the race by restricting the breeding of the unfit and encouraging the fit to have more children.[32]

By the early twentieth century, Galton found himself the leader of a powerful pressure group. Eugenics flourished in many countries, buoyed by fear of racial degeneration and a wave of support for the idea that science offered the route to a carefully managed society. This rise to prominence coincided with the emergence of heredity as a major focus of biologists' attention. Galton's disciple Karl Pearson (1857–1936) developed statistical techniques for evaluating the effect of selection on hereditary characters within a population, and the "rediscovery" of Mendel's laws came in 1900. Historians have linked the developments in science and social opinion, and the most extreme interpretations have argued that the structure of theories of heredity was determined by the uses to which they were put in supporting eugenics. As with the race question, it is relatively easy to argue the case for social pressures focusing scientists' attention onto particular issues but less easy to prove that the theories themselves reflect particular social values. The fact that rival theories were offered to legitimate the same social attitudes undermines the determinist interpretation, leaving room for the possibility that scientific questions shaped the details of thinking within a generally hereditarian framework.

Pearson supported Darwinian natural selection, and Darwinism has thus been seen as a model for eugenics: Natural selection is replaced by artificial selection in the human population. Pearson laid the foundations of many modern statistical techniques, and his strong support for eugenics has led Donald Mackenzie to argue that those techniques were designed to highlight the effects of heredity in human society. A recent study of Pearson's statistics suggests, however, that many of his techniques were motivated by biological problems; when he turned to human heredity, he introduced different methods of analysis.[33] The link to Darwinism must also be treated with care:

[32] On Galton, see for instance Ruth Schwartz Cowan, "Nature and Nurture: The Interplay of Biology and Politics in the Work of Francis Galton," *Studies in the History of Biology*, 1 (1977), 133–208; N. W. Gilham, *A Life of Sir Francis Galton: From African Exploration to the Birth of Eugenics* (Oxford: Oxford University Press, 2002). The literature on eugenics is enormous. Classic studies include Mark H. Haller, *Eugenics: Hereditarian Attitudes in American Thought* (New Brunswick, N.J.: Rutgers University Press, 1963); D. K. Pickens, *Eugenics and the Progressives* (Nashville, Tenn.: Vanderbilt University Press, 1968); G. R. Searle, *Eugenics and Politics in Britain, 1900–1914* (Leiden: Noordhoff, 1976); Daniel Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (New York: Knopf, 1985). On the controversial issue of German eugenics, see Richard Weikart, *From Darwin to Hitler: Evolutionary Ethics and Racism in Germany* (New York: Palgrave Macmillan, 2004).

[33] Donald Mackenzie, *Statistics in Britain, 1865–1930: The Social Construction of Scientific Knowledge* (Edinburgh: Edinburgh University Press, 1982); Eileen Magnello, "Karl Pearson's Methodological

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Galton stressed the negative effects of removing selection pressure but did not believe that natural selection was the source of new characters in evolution.

The most characteristic product of the new wave of interest in heredity was, of course, Mendelian genetics. Although Gregor Mendel's (1822–1884) laws of heredity had been published in 1865, they were largely ignored until rediscovered in 1900 by Hugo De Vries (1848–1935) and Carl Correns (1864–1933). Soon Mendelism offered a powerful rival to Galton and Pearson's nonparticulate model of heredity. In the United States especially, genetics was linked to the eugenics program via oversimplified assumptions about the genetic basis of human characteristics. Charles Benedict Davenport (1866–1944) argued that feeblemindedness, for instance, was a single Mendelian character that could easily be selected out of the population by sterilizing the carriers of the gene. Yet the leading British geneticist, William Bateson (1861–1926), did not support eugenics, while Pearson – Bateson's great scientific rival – repudiated Mendelian genetics because he thought it an oversimplified theory that might undermine the credibility of eugenics. Bateson and most early geneticists rejected the Darwinian selection theory. The exact way in which enthusiasm for hereditarian thinking expressed itself in science thus depended on the circumstances of the scientists involved. One of the pioneers of population genetics, Ronald Aylmer Fisher (1890–1962), was influenced by eugenics, although his work helped to show how difficult it would be to eliminate harmful genes from the human population. Similar work was done by J. B. S. Haldane (1892–1964), a socialist who was suspicious of the eugenics movement's efforts to limit the variability of the human population.

Support for eugenics diminished in the 1940s along with the distaste felt by many for the repressive policies of Nazi Germany. A new wave of liberalism in the social sciences generated support for the view that people can be improved by better conditions. In the 1970s, the debate over nature and nurture broke out again over the claims made by Edward O. Wilson (b. 1929) for sociobiology. Ethologists (students of animal behavior) had long maintained that many aspects of behavior are controlled by instincts that have been created by evolution. Wilson pioneered techniques for explaining many aspects of social behavior, especially in insects, in terms of instincts created by natural selection. When he suggested that human behavior, too, might be determined in this way, liberals reacted with outrage and claimed that a new wave of social Darwinism had begun.[34] More recently, many neuroscientists have begun to support the view that genetic inheritance plays a role in determining the structure of the brain, and hence both intellectual

Innovations: The Drapers' Biometrical Laboratory and the Galton Eugenics Laboratory," *History of Science*, 37 (1999), 79–106, 123–50.

34 The literature on sociobiology is also immense. For a survey of the original literature, see Arthur O. Caplan, *The Sociobiology Debate* (New York: Harper and Row, 1978). For a later analysis, see Ullica Segerstrale, *Defenders of the Truth: The Battle for Science in the Sociobiology Debate* (Oxford: Oxford University Press, 2000).

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ability and instinctive behavior. There have been renewed claims that different racial groups have different average levels of intellectual ability. The Human Genome Project has encouraged the belief that there is a genetic "fix" available for every physical and emotional disorder.

These modern controversies form the backdrop for historical research. We cannot escape the knowledge that science's involvement in such debates raises problems about the nature and objectivity of science itself. When we address the past, we are uncovering the origins of concepts and attitudes that still shape our rival visions of human nature. History is used as a means of labeling modern theories to highlight their alleged social implications, as in the identification of sociobiology with social Darwinism. Such appeals to the past show that history is still relevant today but also reveal the dangers that await any historian trying to delve into such controversial issues. We have a duty to warn about the misuse of history, including simpleminded claims that particular ideologies must necessarily be identified with particular scientific theories. But the historian has access to a wealth of information that can confirm the day-to-day involvement of past scientists with the social issues of their time. A socially informed analysis of history offers a valuable way of warning us all of the extent to which science may still be influenced by the same factors.

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# 31

# EXPERIMENTATION AND ETHICS

*Susan E. Lederer*

"Experiments," observed French physiologist Claude Bernard in *An Introduction to the Study of Experimental Medicine* (1865), "may be performed on man, but within what limits?" In the nineteenth and twentieth centuries, answers to Bernard's rhetorical question have differed as physicians, scientists, and soldiers have sought to define the appropriate conduct of human experimentation. Whereas Bernard argued that "The principle of medical and surgical morality consists in never performing on man an experiment which might be harmful to him to any extent, even though the result might be highly advantageous to science," German and Japanese physicians in the Second World War performed experiments on concentration camp inmates and prisoners that were calculated to maim and kill their subjects.[1] Although the limits of ethical experimentation have wide, and in some cases grotesque, variations, physicians and scientists have never been free to experiment at will and without regard for the welfare of research subjects – animal and human. In Nazi Germany, in a hideous reversal of the usual norms regarding human experimentation, Nazi doctors were able to use concentration camp inmates as experimental subjects without restraint, but they were restricted by law in their use of laboratory animals. Part of this chapter explores the ways in which the practice of human experimentation has been constrained in the last two centuries and the groups – physicians, legislators, activists, and members of the lay public – who have participated in defining and implementing limits on human subject research.

Although Bernard explicitly discussed the limits of appropriate human experimentation, it was not his primary concern. Bernard sought to provide a rationale for the use of animals in physiological research. As the successor of physiologist François Magendie, Bernard experienced firsthand criticism of animal experimentation. His own wife and daughters denounced his use

[1] Claude Bernard, *An Introduction to the Study of Experimental Medicine*, trans. Henry Copley Green (New York: Dover, 1957), p. 101.



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of laboratory animals.[2] One of the aims in his 1865 *Introduction* was to establish the utility and morality of animal experimentation or vivisection as instrumental and integral to the science of life. For Bernard and for many others, animal experimentation and human experimentation remained intimately linked. As he reminded his readers.

> Experiments must be made either on man or on animals. Now I think that physicians already make too many dangerous experiments on man, before carefully studying them on animals. I do not admit that it is moral to try more or less dangerous or active remedies on patients in hospitals, without first experimenting with them on dogs; for I shall prove, further on, that results obtained on animals may all be conclusive for man when we know how to experiment properly. (Bernard, p. 102)

Although the fields of animal and human experimentation in the late twentieth century were often viewed and discussed as separate domains, the two were closely entwined for most of the nineteenth and twentieth centuries. This chapter will follow that dual concern – with the animal subjects of research, with the links between human and animal experimentation, and with the issues that were believed to be specific to humans.

## BEFORE CLAUDE BERNARD

Before the 1860s, few physicians conducted systematic human experimentation. The practice of testing drugs and procedures on human beings excited only occasional interest or comment, especially when an experiment involved injuries to subjects. In 1774, when English farmer Benjamin Jesty attempted to vaccinate his wife and sons against smallpox using material taken from the udders of cows infected with cowpox, his neighbors labeled him an "inhuman brute" when his wife nearly lost her arm after developing severe inflammation in the vaccinated area.[3] Nearly two decades later, the successful demonstration by English physician Edward Jenner that inoculation with material taken from cowpox lesions could produce immunity from the more deadly smallpox produced little comment. In May 1796, Jenner vaccinated a healthy eight-year-old boy with fluid taken from a dairy maid infected with cowpox. James Phipps developed a mild reaction to the vaccine; six weeks later, Jenner challenged the protection conferred by the cowpox vaccine by inoculating the boy with pus taken from a patient with smallpox. Although Phipps's failure to develop smallpox encouraged Jenner to pursue his "experiments with redoubled ardor," his initial attempts to publish his

[2] Joseph Schiller, "Claude Bernard and Vivisection," *Journal of the History of Medicine*, 22 (1967), 246–60.
[3] Nicolau Barquet and Pere Domingo, "Smallpox: The Triumph over the Most Terrible of the Ministers of Death," *Annals of Internal Medicine*, 127 (1997), 635–42.

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results were rebuffed by the Royal Society. Jenner sought to accumulate additional proofs by boldly vaccinating five more healthy children with cowpox, challenging the immunity in three of the children with inoculations of smallpox. Although opposition to Jennerian vaccination continued, few, if any, of these critics expressed concern about his human demonstrations. In the United States, early efforts at Jennerian vaccination similarly involved tests of the effectiveness of the vaccine; after vaccinating seven of his children with the new smallpox vaccine, physician Benjamin Waterhouse exposed three of the children to people sick with smallpox.[4]

Not all early human experiments involved infectious disease. The discovery of anatomic peculiarities in some human beings encouraged notable human experiments in the early nineteenth century. In the 1820s and 1830s, U.S. Army surgeon William Beaumont took advantage of a gunshot wound to the abdomen of a French-Canadian trapper to conduct systematic studies of human digestion. Beaumont performed a series of investigations on Alexis St. Martin, who developed a gastric fistula when the wound to his abdomen failed to close. Beaumont's experiments, including the removal of digestive juices and partially digested foods from the man's stomach, proved uncomfortable for St. Martin. In order to obtain greater compliance from his research subject, Beaumont entered into a contractual arrangement with St. Martin. In exchange for room, board, and annual compensation of $150 U.S., St. Martin promised "to assist and promote by all means in his power such philosophical or medical experiments as William [Beaumont] shall direct or cause to be made on or in the stomach of him." As historian Ronald Numbers has argued, Beaumont's experiments on the person of St. Martin hardly disturbed his contemporaries, who encouraged his taking advantage of an "experiment in nature" to promote medical knowledge.[5]

Beaumont was compelled to adopt the novel method of contract to secure the compliance, if not the cooperation, of his anatomically unusual research subject. Such methods were not necessary for physicians in the American South who used slaves of African descent as research subjects.[6] Such eminent American physicians as J. Marion Sims, "father of American gynecology," made arrangements with slaveowners to gain access to slave bodies for experimentation. In 1845, seeking to develop a surgical procedure to correct vesico-vaginal fistula, a tear in the vaginal wall often resulting from injuries during childbirth, Sims attempted a series of operations on slave women. He recalled how his first patient, known only as Lucy, suffered as he strove to

[4] Susan E. Lederer and Michael A. Grodin, "Historical Overview: Pediatric Experimentation," in *Children as Research Subjects: Science, Ethics, and Law*, ed. Michael A. Grodin and Leonard H. Glantz (New York: Oxford University Press, 1994), pp. 3–25.

[5] Ronald L. Numbers, "William Beaumont and the Ethics of Human Experimentation," *Journal of the History of Biology*, 12 (1979), 113–35.

[6] Todd L. Savitt, "The Use of Blacks for Medical Experimentation and Demonstration in the Old South," *Journal of Southern History*, 48 (1982), 331–48.

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perfect his technique: "Lucy's agony was extreme. She was much prostrated and I thought she was going to die."[7] Lucy and the other female slaves underwent as many as thirty surgical procedures, all without the benefit of ether or chloroform. After 1849, when he had developed a satisfactory technique, Sims performed the surgery on white women.

Like slaves, the sick poor who entered nineteenth-century hospitals sometimes found themselves the objects of medical use for both research and teaching. The testing of novel and untested therapies on hospital patients was part of an implicit societal bargain whereby the poor received medical care, often from eminent practitioners, in exchange for allowing themselves to be used as material in the education of medical students and as subjects in experiments. French novelist and social reformer Eugene Sue portrayed the large Parisian hospital of the early nineteenth century as a sinister place, where the bodies – living and dead – of the sick and dying poor were sacrificed on the altar of scientific knowledge. In his popular novel *Les Mystères de Paris* (1843), Sue's murderous doctor, Dr. Griffon, informs his students, "I have a great deal of science because I study, because I experimentalise, because I risk and practice a great deal on my subjects."[8] In the second half of the nineteenth century, reports that hospital patients were being used as subjects of experiments became part of the growing agitation against the use of animals in experimental medicine.

## ANIMALS AND THE VICTORIANS

Opposition to animal experimentation grew out of the larger animal-protection movement, one of many humane reforms of the nineteenth century. In the 1860s, the Royal Society for the Prevention of Cruelty to Animals (founded in London in 1824) publicly criticized student experiments on horses and mules at the French veterinary school in Alfort and called on the French government to end the practice. On the Continent, physiologist Moritz Schiff, a former student of Claude Bernard, excited similar protests in the English community in Florence over his use of animals in vivisectional experiments. Anglo-Irish journalist Frances Power Cobbe led the protest against Schiff and then returned to England where she galvanized the campaign against animal experimentation that convulsed British medicine and the life sciences during the last three decades of the nineteenth century. Dissatisfied with the failure of the more mainstream RSPCA to condemn animal experimentation, Cobbe organized a separate society to pursue her goal of abolishing the vivisection of animals. Her reputation as a journalist and her

[7] J. Marion Sims, *The Story of My Life* (New York: Da Capo Press, 1968), p. 238.
[8] Quoted in John Harley Warner, *Against the Spirit of System: The French Impulse in Nineteenth-Century American Medicine* (Princeton, N.J.: Princeton University Press, 1998), p. 261.

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ability to attract such influential supporters as poet Alfred Lord Tennyson, the Archbishop of York, and Lord Shaftesbury, made the Victoria Street Society the recognized leader of the organized antivivisection movement.[9]

Growing public interest in the vivisection question prompted a Royal Commission in 1875 charged with assessing "the practice of subjecting live animals to experiments for scientific purposes." The following year, the British Parliament enacted the Cruelty to Animals Act, which governed the conduct of animal experimentation in Britain for 110 years. The act required any scientist wishing to conduct experiments on animals to apply for a license from the home secretary. Only those experiments performed for the purpose of advancing knowledge were permitted; public demonstrations and the use of animals by students of medicine and physiology to practice procedures in order to develop manual dexterity were expressly forbidden. The act further required licensed experimenters to report details of their experiments, including numbers and species of animals used, to the Home Office. Since the late nineteenth century, opinions about the adverse effects of this legislation on experimental medicine in Britain have varied. American defenders of medical research, for example, made much of Joseph Lister's statement in 1898 that the licensing requirements would have prevented his own early work on inflammation and antisepsis.[10] Historian Richard French makes a convincing case that implementation of the act initially hindered some experimenters in the years 1876–82. After 1882, however, when the home secretary effectively transferred decision making on licensing applications to the Association for the Advancement of Medicine by Research, a research advocacy group formed by leading British medical practitioners and researchers in 1882, experimental medicine experienced spectacular growth, a pattern well documented by the reports required by the home secretary.[11]

The English antivivisection movement profoundly influenced the development of similar agitation on behalf of laboratory animals in Western Europe and the United States. In Germany and Switzerland, translations of English writings on cruelty in animal experimentation encouraged a crusade against vivisection. A convert to the cause of antivivisection, composer Richard Wagner (1813–1883), provided financial support for German organizations opposed to the "scientific torture" of animals.[12] Consciously modeling the behavior of the English antivivisection movement, German organizations sought to achieve legislative restrictions on the practice of animal

[9] James Turner, *Reckoning with the Beast: Animals, Pain, and Humanity in the Victorian Mind* (Baltimore: Johns Hopkins University Press, 1980).

[10] William Williams Keen, *Animal Experimentation and Medical Progress* (Boston: Houghton Mifflin, 1914), pp. 19, 28, 225–7.

[11] Richard D. French, *Antivivisection and Medical Science in Victorian Society* (Princeton, N.J.: Princeton University Press, 1975).

[12] Ulrich Tröhler and Andreas-Holger Maehle, "Anti-vivisection in Nineteenth-Century Germany and Switzerland: Motives and Methods," in *Vivisection in Historical Perspective*, ed. Nicolaas A. Rupke (London: Croom Helm, 1987), pp. 149–87.

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experimentation at universities and hospitals but proved unable to do so in light of waning public support for the cause. In the 1890s, the German antivivisection movement assumed an anti-Semitic aspect. German antivivisection periodicals such as *Thier- und Menschenfreund* not only supported the abolition of kosher butchering but criticized the "Judaification of doctors" and the "penetration of cynicism into medicine." These criticisms of experimental medicine continued in the 1920s and 1930s, culminating in 1933 in the adoption of laws under the Nazi regime prohibiting "vivisection of animals of whatever species in all parts of the Prussian territory" and warning that "persons who engage in vivisection of animals of any kind will be deported to a concentration camp."[13]

In the United States, the antivivisection movement began slowly in the 1880s and continued to build in the years before the First World War. American antivivisectionists, like their counterparts in Germany, looked to the example of the English antivivisection movement. The first American organizations dedicated to the abolition of animal experimentation appeared in the 1880s, stimulated in part by the personal influence of Frances Power Cobbe. As in Britain, the animal protection and antivivisection societies attracted large numbers of female members, who defenders of medical research labeled overly sentimental and ill-informed about medical science.[14]

The American antivivisection encompassed a spectrum of views about animal experimentation. In addition to ardent abolitionists, who steadfastly denied any benefit from vivisection, self-styled moderates criticized medical researchers for performing cruel experiments on animals without anesthesia or regard for animal suffering, but nonetheless granted that some useful knowledge could be obtained from humanely conducted animal experiments. Although this moderate position proved unpopular with abolitionists who distrusted any compromise with vivisectionists, the defenders of unrestricted animal experimentation found it convenient to conflate the reform position with the more extreme abolitionist rejection of any utility for animal experimentation. Until recently, historians have tended to adopt uncritically this rhetorically useful strategy for dismissing any criticism of animal experimentation as a radical rejection of progress in medical science and to overlook differences between moderates and extremists.[15]

The swelling interest in the protection of laboratory animals sparked a growing interest on the part of physicians and medical researchers in protecting medical research from legislative interference. Fearful that American

[13] Robert N. Proctor, *Racial Hygiene: Medicine under the Nazis* (Cambridge, Mass.: Harvard University Press, 1988), p. 227. See Arnold Arluke and Boria Sax, "Understanding Nazi Animal Protection and the Holocaust," *Anthrozoös*, 5 (1992), 6–31.

[14] Susan E. Lederer, "Moral Sensibility and Medical Science: Gender, Animal Experimentation, and the Doctor–Patient Relationship," in *The Empathic Practitioner: Empathy, Gender and Medicine*, ed. Ellen Singer More and Maureen A. Milligan (New Brunswick, N.J.: Rutgers University Press, 1994), pp. 59–73.

[15] Susan E. Lederer, "The Controversy over Animal Experimentation in America, 1880–1914," in Rupke, *Vivisection in Historical Perspective*, pp. 236–58.

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legislatures would follow the British Parliament in adopting restrictions on the use of animals, leaders of organized medicine in the United States formed committees to combat the threat. One of the leaders of the American defense of medical research was Harvard physiologist Walter Bradford Cannon, who devoted several decades of his career to the cause. In 1908, Cannon developed a set of guidelines for the humane conduct of animal experimentation, which he circulated in his capacity as chair of the American Medical Association's Committee on the Protection of Medical Research to the deans of U.S. medical schools. These rules included provisions for holding dogs and cats for twenty-four hours in order to allow owners of lost or stolen pets to reclaim their animals.[16]

Sensitive to antivivisectionist reliance on medical and scientific journals for examples of cruelty in animal experimentation, Cannon encouraged journal editors to pay special attention to words and expressions that could be misunderstood by critics and the lay public. Calling attention to the fact that an investigator's failure to mention the use of anesthetics in conducting research on animals did not mean that an anesthetic was not administered, Cannon asked that such details be routinely included in biomedical publications. At the *Journal of Experimental Medicine,* the leading American biomedical research journal in the first half of the twentieth century, editor Francis Peyton Rous and his staff closely monitored submitted manuscripts with an eye to averting accusations of abuse or charges of insensitivity in the use of human and animal subjects. In addition to substituting less emotionally laden words such as fasting for starving or intoxicant for poison, Rous restricted photographs of animal experiments to parts or limbs of animals and refused to publish altogether "unsightly" photographs of animal subjects.[17]

This careful scrutiny in publication illustrates the extent to which leading biomedical researchers in the middle decades of the twentieth century continued to fear antivivisectionist interference in medical research. Despite the marginalization of the antivivisection movement in both England and the United States, the movement's focus in the 1920s and 1930s on rescuing dogs from the research laboratory compelled vigilance on the part of experimenters. The heightened interest in dogs and fears about pets stolen for sale to research facilities created difficulties about getting sufficient numbers of animals for experimental work. In Britain, the success of a canine distemper vaccine, developed through experiments on dogs, played a significant role in undermining the legislative campaigns in the 1920s to abolish experiments involving these animals.[18] The discovery of insulin in the 1920s and the

[16] Saul Benison, A. Clifford Barger, and Elin L. Wolfe, *Walter Bradford Cannon: The Life and Times of a Young Scientist* (Cambridge, Mass.: Belknap Press, 1987).

[17] Susan E. Lederer, "Political Animals: The Shaping of Biomedical Research Literature in Twentieth-Century America," *Isis*, 83 (1992), 61–79.

[18] E. M. Tansey, "Protection Against Dog Distemper and Dogs Protection Bills: The Medical Research Council and Anti-vivisectionist Protest, 1911–1933," *Medical History*, 38 (1994), 1–26.

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dramatic photographs of diabetic children saved from certain death helped investigators avert legislative restrictions on dog experimentation.

Supporters of animal experimentation warned that restrictions on the practice of vivisection would lead to unwarranted and dangerous experiments on hospital patients. Already suspicious of the intentions of medical scientists, antivivisectionists interpreted this warning as evidence that such experimentation was in fact the goal of physicians and physiologists. Seeking to persuade others about the abuse of "human guinea pigs," antivivisectionists collected and published excerpts of cases of "human vivisection." In the 1890s and well into the twentieth century, reports of studies involving the deliberate infection of unsuspecting patients with the pathogens of such diseases as syphilis, gonorrhea, and leprosy appeared in both popular magazines and newspapers.[19]

At the same time, defenders of medical research pointed to spectacular advances made possible by human experimentation. In the most famous case of human experimentation in the nineteenth century, Louis Pasteur's use of his new rabies vaccine on ten-year-old Joseph Meister raised few qualms about the ethics of human experimentation. In 1885, when Meister recovered from his injuries after receiving the Pasteur treatment, a few critics challenged both the ethics and the theoretical basis of the vaccine. But the overwhelming success of Pasteur's product insulated him from harsh and searching criticism. As historian Gerald Geison has convincingly demonstrated, the French chemist, who clearly appreciated the problems posed by experimenting on human beings, failed to meet his own criteria for an ethical trial of his treatment before he (actually a colleague) administered the vaccine to the child. Not only did Pasteur's own laboratory notebooks "provide no evidence that Pasteur had actually completed the animal experiments to which he appealed in justification of his decision to treat Meister," but they revealed that the French scientist had only just begun "vaguely comparable" experiments on a series of dogs when he opted to test the vaccine on the boy.[20]

Pasteur's patient would presumably have died without treatment. Experimenters who used healthy subjects in research on dangerous diseases faced different ethical questions. One strategy in the early twentieth century to disarm potential critics of human experimentation was to obtain written permission from subjects. Introduced by American physician Walter Reed in the course of research involving yellow fever, such documents identified the experimental conditions and benefits for the subjects. In 1900, Reed, working with members of the United States Army Yellow Fever Board in Cuba, used human beings to demonstrate how mosquitoes transmit yellow

[19] Susan E. Lederer, *Subjected to Science: Human Experimentation in America before the Second World War* (Baltimore: Johns Hopkins University Press, 1995).

[20] Gerald L. Geison, *The Private Science of Louis Pasteur* (Princeton, N.J.: Princeton University Press, 1995), pp. 251–2.

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fever, a deadly disease for which physicians had no cure. The Reed Commission required the mostly Spanish participants to sign a written agreement (in both Spanish and English) that outlined the risks to the participants and the benefits, including one hundred dollars in gold, for agreeing to the experimental conditions. Although such an agreement would not meet consent standards in the early twenty-first century, the document represented a significant departure at a time when surgeons were just beginning to obtain written permission from their patients for surgical treatment. The successful demonstration of the mosquito vector of yellow fever did not entail the death of any nonscientist participants, further insulating Reed and his colleagues from criticism over risking the lives of human subjects.

In the first four decades of the twentieth century, explicit discussions about ethical standards for the conduct of human experimentation and legal restrictions on the practice followed public disclosures of unethical research. In the United States, press reports of experiments on orphans and inmates of mental institutions involving syphilis prompted leaders of the American research establishment to attempt to amend the American Medical Association's Code of Ethics in 1916. Although leading American researchers supported the effort to introduce an explicit provision in the code outlining the necessity of obtaining the consent of the subject for participation in medical experiments, their effort ended in failure. Fears that such a provision would hinder the progress of medical science contributed to a lack of support for altering the code.

In the 1920s and 1930s, leaders of the American medical profession publicized the willingness of medical men and women to use their own bodies for experimentation. Self-experimentation has long played an important role in the history of medicine. In the twentieth century, self-experimentation produced such dramatic results as the development of heart catheterization; Werner Forssmann, who performed the procedure on himself, won the 1956 Nobel Prize in Physiology or Medicine.[21] In the 1980s, Australian physician Barry Marshall demonstrated the role of *Helicobacter pylori* in stomach disease by ingesting the bacteria and developing gastritis. Before World War II, the commemoration of such "heroes and martyrs" of scientific medicine – Jesse Lazear, Clara Maas, Hideyo Noguchi, and Adrian Stokes, all victims of research-related yellow fever – served to deflect attention from research involving orphans, insane patients, or other vulnerable populations.[22]

In Europe, in response to public scandals over human experimentation, legislators attempted to place restrictions on human experimentation. In 1900, the Prussian Ministry of Culture issued an ordinance forbidding medical experimentation on underaged and debilitated persons and requiring that

[21] Lawrence K. Altman, *Who Goes First? The Story of Self-Experimentation in Medicine* (New York: Random House, 1986).

[22] Lederer, *Subjected to Science.*

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subjects of medical experiments give consent for their participation. This ordinance followed the controversy over Breslau researcher Albert Neisser's use of prostitutes and young girls in tests of a syphilitic serum for which Neisser, the discoverer of the gonococcus, received an official reprimand and a fine of three hundred reichsmarks.[23] In 1931, on the heels of the "Lübeck disaster," the German Reichs Minister of the Interior issued Regulations on New Therapy and Human Experimentation. The Lübeck tragedy involved tests of BCG vaccine for tuberculosis conducted in 1930 by the director of the municipal hospital. Trials of the vaccine, contaminated with virulent tuberculosis bacilli, led to the deaths of seventy-six children and infants. In addition to jail terms for the physicians, the Ministry of the Interior called a special meeting of the Council on Health to discuss human experimentation, resulting in regulations requiring animal experimentation precede tests on human beings and requiring the consent of the subject and special protections for children, the "socially needy" patient, and the dying. These regulations, which remained technically in effect through 1945, were completely ignored by Nazi physicians in the years after 1933.[24]

## SCIENCE IN THE SERVICE OF THE STATE

Even before Adolf Hitler's rise to power in 1933, significant numbers of physicians and biologists had joined National Socialist professional organizations in Germany. Nazi physicians anticipated increased responsibilities under the new regime as they prepared for the transition "from the doctor of the individual, to the doctor of the nation."[25] Medical professionals played an important role in the creation and implementation of such Nazi racial policies as the Nuremberg Laws and the Genetic Health Courts, which authorized the compulsory sterilization of more than 400,000 people. Doctors participated in both the killing of large numbers of people considered racially inferior or mentally and socially defective and the notorious medical experiments performed on concentration camp inmates. In order to gather information to serve the needs of the German military, Nazi physicians conducted a series of harrowing experiments, including immersing Dachau prisoners in ice water to determine how long German pilots could survive in the frigid waters of the North Sea, forcing inmates to drink seawater to determine how long a man could survive without fresh water, locking prisoners in low-pressure chambers to simulate atmospheric conditions at the high altitudes experienced

[23] Barbara Elkeles, "Medizinische Menschenversuche gegen Ende des 19. Jahrhunderts und her Fall Neisser," *Medizinhistorisches Journal*, 20 (1985), 135–48.

[24] Hans-Martin Sass, "Reichsrundschrieben 1931: Pre-Nuremberg German Regulations Concerning New Therapy and Human Experimentation," *Journal of Medicine and Philosophy*, 8 (1983), 99–111.

[25] Robert N. Proctor, *Racial Hygiene: Medicine under the Nazis* (Cambridge, Mass.: Harvard University Press, 1988), p. 73.

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by German pilots, and performing mutilating limb operations in order to develop medical and surgical techniques to save wounded soldiers.

Many of the gruesome details concerning the Nazi experiments only became clear at the "Doctors' Trial," an American military tribunal conducted in 1946 and 1947. During the trial, the accused (twenty-three defendants, all but three of them physicians) defended their conduct, noting that the lives of prisoners were being used to save the lives of others. German defense attorneys for the accused medical personnel, moreover, argued that Nazi use of camp inmates for medical experimentation was hardly unique. They pointed to the use of prisoners and other institutionalized populations by American investigators, as well as the exclusion of black physicians from American medical organizations.[26]

American investigators energetically disputed any equation of American-conducted experimentation and Nazi research. Andrew C. Ivy, a physiologist at the University of Illinois who served as the American Medical Association's adviser to the American prosecutors at the "Doctors' Trial," argued that inmates of American prisons were able to consent without coercion to participate in research. As he helped prosecutors prepare for the trial, Ivy orchestrated the adoption by the American Medical Association (AMA) of its first formal position on the ethics of human experimentation. In December 1946, the AMA identified three requirements for ethical human research: the voluntary consent of the subject; the need for prior animal experimentation; and the necessity of proper medical oversight. These principles were cited in the successful prosecution of the Nazi doctors.[27]

Despite the AMA's pronouncement of the conditions for ethical human experimentation, much American medical research during and after the Second World War failed to meet the first requirement. In the years 1941–5, for example, American investigators routinely used populations unable to exercise voluntary consent, including children and the mentally ill. Under the auspices of the Committee on Medical Research, a branch of the Office for Scientific Research and Development established by President Franklin D. Roosevelt in 1941, researchers investigated such diseases as dysentery, influenza, malaria, and venereal diseases, and such physical hardships as exposure to frigid temperatures, as part of the American war effort, using prisoners, inmates of mental institutions, and children in orphanages. Indeed, access to institutionalized populations such as the children at the Ohio State and Soldiers Home or the New Jersey State Colony for the Feeble-Minded enhanced an investigator's grant application to the committee.[28] During the war and after, American investigators similarly used the bodies of conscientious

[26] George J. Annas and Michael A. Grodin, *The Nazi Doctors and the Nuremberg Code: Human Rights in Human Experimentation* (New York: Oxford University Press, 1992).

[27] Jon M. Harkness, "Nuremberg and the Issue of Wartime Experiments on US Prisoners," *Journal of the American Medical Association*, 276 (1996), 1672–5.

[28] David J. Rothman, *Strangers at the Bedside* (New York: Basic Books, 1991).

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objectors for trials of new drugs, vaccines, and procedures. American servicemen also participated in large-scale, clandestine tests involving exposure to such warfare agents as mustard gas and nuclear radiation.[29] In the conduct of such research, American investigators expressed some concern about the potential for adverse outcomes, especially the injury or death of research subjects. During the Second World War, for example, Harvard University blood researcher Edwin Cohn made inquiries about insurance to cover risks to research subjects and to indemnify the university against damages in case of a bad outcome, but university officials dismissed such policies as too expensive. In 1942, when clinical trials of an experimental bovine blood substitute, conducted in a Massachusetts prison, resulted in the death of a prisoner, Cohn and his sponsors at the Committee for Medical Research worried about the potential consequences, including the prospect of a lawsuit. The prisoner's mother, as historian Jon Harkness has shown, did not sue the investigator. She quietly accepted her son's posthumous pardon; her son's funeral costs were charged to Edwin Cohn's research grant.[30]

In Great Britain, conscientious objectors participated as volunteers in medical research in the 1930s and 1940s. In 1945, medical entomologist Kenneth Mellanby chronicled British wartime research on scabies using this population. Mellanby went on to serve as the *British Medical Journal*'s correspondent at the Nuremberg "Doctors' Trial," where he challenged American claims about the worthlessness of Nazi medical research. Historian Paul Weindling has argued that Mellanby was not alone among British researchers in regarding Nazi medical crimes as the predictable outcome of state interference in medicine and medical research. The British Medical Association subsequently marshaled these arguments to oppose the National Health Service and to avert any state oversight of clinical research.[31]

As the Nuremberg medical trial concluded, U.S. military leaders investigated the large-scale testing of bacteriological warfare agents by Japanese military physicians. The story of Japanese military medical research, like that of the Nazi physicians, became public only after World War II. Following the Japanese invasion of Manchuria in 1931, physician Ishii Shiro, a major in the Japanese Army and a major proponent of bacteriological warfare, established a research installation in Manchuria where extensive testing of bacteriological agents and delivery systems was undertaken. For thirteen years and with substantial funding from the Japanese military, Shiro conducted tests on thousands of Chinese prisoners using the pathogens that cause plague, cholera, typhoid fever, dysentery, anthrax, and glanders. At his command, frostbite

[29] Constance M. Pechura and David P. Rall, eds., *Veterans at Risk: The Health Effects of Mustard Gas and Lewisite* (Washington, D.C.: National Academy Press, 1993).

[30] Jon M. Harkness, "Research Behind Bars: A History of Nontherapeutic Research on American Prisoners" (PhD diss., University of Wisconsin–Madison, 1996).

[31] Paul Weindling, "Human Guinea Pigs and the Ethics of Experimentation: the *BMJ*'s Correspondent at the Nuremberg Medical Trial," *British Medical Journal*, 313 (1996), 1467–70.

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studies involving the repeated freezing and thawing of human beings were performed, ending with the death of the subject. Historian Sheldon Harris has estimated that more than three thousand people perished before Ishii dismantled his operation at the end of the war. Ishii and his colleagues were not prosecuted for their crimes; the U.S. government extended amnesty to the Japanese investigators in exchange for access to their data on germ warfare and to ensure that details of the research would not become publicly known, especially to the Russians.[32] The Japanese government, which has formally apologized for some of its wartime atrocities, has so far declined to apologize for the bacteriological research program undertaken by Ishii.

## THE WORLD MEDICAL ASSOCIATION AND RESEARCH AFTER NUREMBERG

In the years following the Nuremberg Trials, the "whiff of the concentration camp" continued to haunt exploration of the ethical issues posed by human experimentation.[33] Discussions about the appropriate use of human subjects continued on many fronts but with little apparent urgency. In 1951, American cancer researcher Michael Shimkin organized a public symposium at the University of California School of Medicine at San Francisco about the conduct of human subject research, which he eventually published in the journal *Science*, together with the Nuremberg Code.[34] In Europe, organizers of the First International Congress on the Histopathology of the Nervous System invited Pope Pius XII to address the "Moral Limits of Medical Methods of Research and Treatment." In his 1952 address, the pope identified the necessity of obtaining consent from participants in research and emphasized the lessons of the recent Nuremberg Trials. Virus researcher Tom Rivers recalled that the speech had a "broad impact on medical scientists" in both the United States and Europe.[35]

One outcome of the lingering revulsion for the Nazi doctors was the attempt to develop professional guidelines for ethical human experimentation. A leader in this effort was the World Medical Association, founded in 1947 by physicians from countries invaded by the Nazis as well as doctors from Australia, the United States, Canada, Britain, New Zealand, and South Africa. In the 1950s and 1960s, the association struggled to adopt standards that balanced the need for ongoing human experimentation with the rights of

32 Sheldon H. Harris, *Factories of Death: Japanese Biological Warfare 1932–45 and the American Cover-Up* (London: Routledge, 1994).

33 Peter Flood, *Medical Experimentation on Man* (Cork: Mercier Press, 1955), p. 11.

34 Michael B. Shimkin, "The Problem of Experimentation on Human Beings," *Science*, 117 (1953), 205–7.

35 Ruth R. Faden, Susan E. Lederer, and Jonathan D. Moreno, "US Medical Researchers, the Nuremberg Doctors Trial, and the Nuremberg Code," *Journal of the American Medical Association*, 276 (1996), 1667–71.

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volunteers and patients. In 1964, the Eighteenth World Medical Assembly in Helsinki endorsed an influential set of guidelines for clinical research. Part of the impetus for adoption of the "Helsinki Declaration" was international concern over the recent thalidomide tragedy and proposals in the United States to tighten federal regulations on drug trials involving human subjects.[36] Unlike the Nuremberg Code, which identified the voluntary consent of the research subject as an absolute requirement for ethical experimentation, the Helsinki Declaration endorsed proxy consent for research on persons unable to give consent (children, the comatose, the mentally ill) and permitted investigators to forego consent if it was "not consistent with patient psychology."[37]

That such decisions could be safely left to the conscience of individual experimenters disturbed some medical observers. In Britain, physician Maurice Pappworth began to gather cases of published research that he considered to be ethically suspect. His 1967 book *Human Guinea Pigs* (expanded from an article first published in 1962) included descriptions of experiments on infants and children, mental defectives and the mentally sick, criminals, the dying and the old, surgical patients, and nonpatient volunteers.[38] Although some have claimed that Pappworth's explosive charges sparked only limited interest on the part of the British public and exerted little influence on practicing physicians, historian Rachel McAdams argues that Pappworth's influence was much greater.[39] In the 1960s and 1970s, major British medical organizations, including the Medical Research Council and the Royal College of Physicians, issued recommendations and guidelines for the conduct of human experimentation.[40]

In both Great Britain and the United States, discussions over the rights of research subjects occurred against the backdrop of growing suspicion of authority in general and medical experts in particular. In the 1960s, environmentalists, feminists, civil rights activists, and antinuclear and peace activists attacked established values and called for radical changes in the status quo. In the 1970s, the women's health movement challenged male-dominated medical theory and practice, especially in obstetrics and gynecology, and questioned the instrumental use of women's bodies. Concerns about medicine's increasing depersonalization, growing dependence on sophisticated technology, and widening gap between doctors and patients promoted a medical malpractice "crisis," an explosion in litigation against physicians and medical

[36] Annas and Grodin, *Nazi Doctors and the Nuremberg Code.*

[37] Paul M. McNeill, *The Ethics and Politics of Human Experimentation* (Cambridge: Cambridge University Press, 1993).

[38] Maurice Pappworth, *Human Guinea Pigs: Experimentation on Man* (Boston: Beacon Press, 1967).

[39] M. H. Pappworth, "'Human Guinea Pigs' – A History," *British Medical Journal*, 301 (1990), 1456–60; Rachel McAdams, "Human Guinea-pigs: Maurice Pappworth and the Birth of British Bioethics" (MSc thesis, University of Manchester, 2005), copies held at the John Rylands Library, University of Manchester, and at CHSTM.

[40] Robert J. Levine, "Protection of Human Subjects of Biomedical Research in the United States: A Contrast with Recent Experience in the United Kingdom," *Annals of the New York Academy of Sciences,* 530 (1988), 133–43.

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institutions. American legislatures moved to restrict such medical abuses as psychosurgery, especially lobotomy, and the misuse of both human and animal subjects in biomedical research. At the same time, the character of medical research was changing. In the 1970s, British physician Archie Cochrane advanced the idea that health services should be evaluated on the basis of scientific evidence. The call for what would become "evidence-based medicine" stimulated a growing number of randomized clinical trials involving large numbers of patients to establish the efficacy and cost-effectiveness of all kinds of medical interventions.

In the United States, Harvard anesthesiologist Henry K. Beecher rocked the medical establishment when he called attention to ethical lapses in clinical research in mainstream American medical research. Selecting twenty-two cases of ethically questionable research in his 1966 article "Ethics and Clinical Research," Beecher argued that such lapses occurred in major medical research centers and under the direction of investigators who received major funding from the National Institutes of Health.[41] Unlike Pappworth, Beecher did not provide identifying names to the experiments he identified as morally suspect, but some of his examples became infamous in the annals of human experimentation. These included the Willowbrook studies, in which New York University physician Saul Krugman and his colleagues deliberately infected residents of the Willowbrook State School for the Retarded with hepatitis, and the Jewish Chronic Disease Hospital studies, wherein Dr. Chester Southam conducted investigations involving the injection of cancer cells into elderly and senile patients.[42]

Public concern about abuses of research subjects continued with the revelation of the Tuskegee Syphilis Study. In 1972, Americans learned that an agency of the U.S. government had been conducting a forty-year study of untreated syphilis in African American men. Between 1932 and 1972, when the Tuskegee Syphilis Study was halted, the U.S. Public Health Service had followed more than four hundred men, monitoring the clinical manifestations of the disease as the men aged and performing autopsies after their deaths. In hearings before the U.S. Congress, legislators heard how government doctors had actively deceived participants in the Tuskegee Syphilis Study, many of them poor and illiterate, who believed that they were receiving treatment for their "bad blood." The outrage provoked by the government's role in the Tuskegee Syphilis Study, as historian James Jones has argued, more than any other event prompted the U.S. Congress to pass federal regulations to protect the human subjects of medical research.[43] In 1974, the U.S. Congress

[41] Henry K. Beecher, "Ethics and Clinical Research," *New England Journal of Medicine*, 274 (1966), 1354–60.

[42] Albert R. Jonsen, *The Birth of Bioethics* (New York: Oxford University Press, 1998).

[43] James H. Jones, *Bad Blood: The Tuskegee Syphilis Experiment*, expanded edition (New York: Free Press, 1993). p. 214. See also Susan E. Lederer, "The Tuskegee Syphilis Study in the Context of American Medical Research," in *Tuskegee's "Truths": Rethinking the Tuskegee Syphilis Study*, ed. Susan M. Reverby (Chapel Hill: University of North Carolina Press, 2000) pp. 266–75.

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passed the National Research Act, which required that research institutions receiving federal dollars create institutional review boards to monitor all proposals that involved human subjects and that investigators obtain the written informed consent of the research participants.

One irony of the increasing protections for women and minority populations was the growing recognition some two decades later that these groups were "underrepresented in medical research" and thus not benefiting from medical knowledge gained from clinical trials. In the 1980s, womens' health advocates and AIDS activists drew attention to inequities in research funding and the exclusion of women and minorities from studies. In response, the National Institutes of Health in 1993 introduced new requirements for the inclusion of women and minorities in federally funded clinical studies (except where the exclusion of such groups could be justified).[44] Investigators have found that some minorities, especially African Americans, have been reluctant to participate in clinical trials because of the history of abuses.[45]

The growing interest and concern about the abuses of human experimentation and such medical ethical issues as "fetal research" and organ transplantation accelerated the development of a new academic field in the United States. Bioethics, a term first used in 1970, grew in part in response to the desire on the part of the federal bureaucracy for expertise on the moral problems posed by advances in biology and medicine.[46] A provision of the National Research Act mandated the formation of a National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research (1974–8), which as part of its work commissioned papers on ethical issues in human experimentation. A second national commission, the President's Commission for the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research (1980–3), included further issues such as brain death, decisions to forego life-sustaining treatment, and access to health care. In addition to the federal impetus for bioethical expertise, medical schools and universities offered opportunities to individuals interested in bioethical issues. As of 1998, nearly two hundred centers, departments, and programs for bioethics existed in the United States.[47]

## ANIMALS AND ETHICS

In 1966, the same year that Henry Beecher published his exposé of clinical research, the American animal protection movement achieved its longtime

[44] Anna C. Mastroianni, Ruth Faden, and Daniel Federman, eds., *Women and Health Research: Ethical and Legal Issues in Including Women in Clinical Studies* (Washington, D.C.: National Academy Press, 1994).

[45] Vanessa N. Gamble, "Under the Shadow of Tuskegee: African Americans and Health Care," *American Journal of Public Health*, 87 (1997), 1773–8.

[46] For the origin of the term bioethics, see Warren T. Reich, "The Word 'Bioethics': Its Birth and the Legacies of Those Who Shaped Its Meaning," *Kennedy Institute of Ethics Journal*, 4 (1994), 319–36.

[47] Jonsen, *Birth of Bioethics*.

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goal of federal regulations on the use of animals for experimentation. Long championed by various animal protection groups, this legislation followed public outrage over a photographic essay in *Life* magazine. "Concentration Camps for Dogs" included photographs showing malnourished and mistreated dogs maintained in filthy conditions by animal dealers, many of whom furnished animals to research facilities.[48] In 1966, the U.S. Congress, which had received more mail in one week on such issues as dog theft and licensing of animal dealers than it had on civil rights and the Vietnam War, enacted the Laboratory Animal Welfare Act. The legislation required all research facilities and animal dealers to register with the Department of Agriculture, included provisions intended to protect pet owners from having their animals stolen, and identified six groups of animals – dogs, cats, primates, rabbits, hamsters, and guinea pigs – as meriting humane care and treatment. Since 1966, the U.S. Congress has modified the law, renamed the Animal Welfare Act, several times and included new provisions to mandate the use, where appropriate, of pain-relieving drugs and to create Institutional Animal Care and Use Committees to review protocols involving animal subjects.[49]

In the 1970s, the emergence of new critics of animal experimentation, especially animal rights activists, intensified debates over the use of animals in research, factory farming, and for their fur. Crucial to the development of the new activism on behalf of animals were philosophers, especially Australian philosopher Peter Singer, whose 1975 book *Animal Liberation* has been hailed as the "bible" of the animal rights movement.[50] Singer's work bestowed intellectual legitimacy on the animal rights movement, enabling activists to shed "the stigma of sentimentality" that had crippled the movement's political course in the preceding decades."[51] Partly fueled by the environmentalist movement, the civil rights movement, and the women's movement, the animal rights movement rejuvenated the older and larger animal welfare movement, drawing large numbers of new members in the 1980s. In Britain, the publication of Richard Ryder's *Victims of Science* (1973) spurred debate on the use of experimental animals and fostered renewed and radical activity on the part of animal protectionists.[52] One result of the renewed interest in laboratory animals was the passage of the Animals (Scientific Procedures) Act in 1986. The act introduced new regulations for licensing individual researchers and procedures, as well as registration of animal breeders and suppliers. As part of the "three Rs" (reduction, refinement, and replacement)

[48] "Concentration Camp for Dogs," *Life*, 60 (February 4, 1966), 22–9.

[49] F. Barbara Orlans, *In the Name of Science: Issues in Responsible Animal Experimentation* (Oxford: Oxford University Press, 1993), p. 50.

[50] James M. Jasper and Dorothy Nelkin, *The Animal Rights Crusade: The Growth of a Moral Protest* (New York: Free Press, 1992).

[51] Andrew Rowan, "The Development of the Animal Protection Movement," *Journal of NIH Research*, 1 (1989), 97–100, at p. 100.

[52] E. M. Tansey, "'The Queen Has Been Dreadfully Shocked': Aspects of Teaching Experimental Physiology Using Animals in Britain," *Advances in Physiology Education*, 19 (1998), S18–S33.

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first suggested in the 1950s, the number of scientific procedures performed on experimental animals in Britain had dropped from 3.6 million in 1987 to 2.8 million in 1994.[53]

## LIVING WITH THE PAST HISTORY OF HUMAN EXPERIMENTATION

In the 1990s, the history of human experimentation, especially research conducted in World War II and after, has been a cause of national self-examination, formal apology, financial compensation, and litigation. Although the U.S government had paid ten million dollars in compensation in 1974 in an out-of-court settlement to participants in the Tuskegee Syphilis Study, it was only in May 1997 that President Bill Clinton formally apologized on behalf of the nation to the handful of remaining surviving participants of the study and to relatives of the men who had died without treatment for syphilis. "The United States government," Clinton observed, "did something that was wrong – deeply, profoundly, morally wrong. It was an outrage to our commitment to integrity and equality for all citizens."[54]

The formal apology to the participants of the Tuskegee Syphilis Study followed President Clinton's apology to the American men, women, and children who had participated in human radiation experiments conducted under the auspices of the U.S government during the Second World War and continuing into the cold war decades. Although the revelation of human radiation experiments was not new (Massachusetts congressman Edward Markey had called attention to America's "nuclear guinea pigs" in 1986), human radiation experiments received national attention in 1993 when reports in the American press on plutonium injections involving U.S. citizens nearly fifty years earlier provided the catalyst for a massive inquiry into America's nuclear history and cold war research.[55] Coinciding with the Department of Energy's openness initiative, Secretary of Energy Hazel O'Leary called for a full investigation of her agency's sponsorship of radiation research involving human beings. In 1994, President Bill Clinton appointed an independent advisory committee to investigate experiments involving ionizing radiation conducted between 1944 and 1974 to determine the ethical and scientific standards by which to evaluate these early experiments and to make recommendations about the adequacy of current protections for human subjects in American

[53] Robert Garner, *Political Animals: Animal Protection Policies in Britain and the United States* (New York: St. Martin's Press, 1998).

[54] Alison Mitchell, "Survivors of Tuskegee Study Get Apology from Clinton," *New York Times*, May 17, 1997; "Remarks by the President in Apology for Study Done in Tuskegee," The White House, Press Release.

[55] Subcommittee on Energy Conservation and Power, House of Representatives, "American Nuclear Guinea Pigs: Three Decades of Radiation Experiments on U.S. Citizens," 99th Congress, 2d session.

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research.[56] Since 1994, several U.S. universities and corporations that sponsored radiation-related research have paid compensation to participants in the human radiation experiments; a number of lawsuits remain pending against both institutions and investigators.

Claude Bernard reminded his readers in 1865 that experiments must be made either on man or on animals. In the 1990s, most researchers would argue that both are necessary to the biomedical sciences. In the nineteenth and twentieth centuries, the use of human beings and animals in research raised issues about the appropriate limits and restrictions on experiments with both types of subjects. Concern about nonhuman animals antedated similar concerns for the human subjects of research, but for most of the last two centuries, the fates of both the four-legged and two-legged guinea pigs have remained closely intertwined.

[56] *Final Report of the Advisory Committee on Human Radiation Experiments* (New York: Oxford University Press, 1996).

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# 32

# ENVIRONMENTALISM

*Stephen Bocking*

Environmentalism is a moving target, always changing position and appearance. Some see it as a state of mind or a way of life; others assume it is a critique of contemporary society or a political platform. Even its single most widely understood meaning, concern about the state of, and human impacts on, the natural environment, has diverse implications, from merely recycling cans and bottles to rejecting industrial society. Environmental values vary across cultures: One society's bustling, prosperous city is another's smog-choked hell; a stagnant swamp fit for draining is also a diverse wetland worth preserving.[1] Where consensus has formed on environmental problems, their definition as matters of personal or societal responsibility nevertheless varies across social contexts.

Clearly, a linear, sequential history of environmentalism is not possible. As a result, studies of environmentalism have often focused on specific places: the American West, New England, Canada, Britain, Sweden, or India. Conversely, historians attempting a general account have sometimes been tempted to constrain this diversity within a single narrative, grounded in a search for the "roots" or "origins" of environmentalism.[2]

In seeking these roots, historians have most often found them in individuals such as Gilbert White, Henry David Thoreau, John Muir, and George Perkins Marsh or, more recently, Rachel Carson or Aldo Leopold. Recent studies have provided a wider view of these origins by demonstrating how ideas have emerged from colonial contexts to eventually shape European perspectives or by showing the significance of places and disciplines not

[1] Mary Douglas, *Purity and Danger: An Analysis of the Concepts of Pollution and Taboo* (London: Routledge and Kegan Paul, 1966).

[2] See, for example, Richard Grove, *Green Imperialism: Colonial Expansion, Tropical Island Edens and the Origins of Environmentalism, 1600–1860* (Cambridge: Cambridge University Press, 1995); David Pepper, *The Roots of Modern Environmentalism* (London: Croom Helm, 1984); Donald Worster, *Nature's Economy: A History of Ecological Ideas*, 2nd ed. (Cambridge: Cambridge University Press, 1994).



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usually considered central to environmentalism, such as industrial hygiene and the "workplace roots of environmentalism."[3] These studies have also indicated the importance of circumstance: of the fortuitous combination of observations, individuals with the background and inclination to derive conclusions from this evidence, and institutional and political contexts that provide opportunities to express these conclusions.

But the notion of identifying the "origins" of environmentalism is also problematic. It implies that environmentalism can be reduced to a set of essential ideas that, having originated in a specific context, become, paradoxically, universally significant. Viewed in terms of origins, the history of environmentalism risks becoming a linear account of the expression (or suppression) of these essentials that lifts the "forerunners" of ideas now considered important out of their historical context. An emphasis on origins also privileges a particular perspective on environmentalism as the product of a few perceptive individuals (often scientists) whose insights are eventually disseminated into society. Finally, it denies the possibility that the key issue is not so much identifying who first expressed environmental concerns but understanding how the notion emerged that these concerns demand collective responses. As has been the case for other social issues, such as crime, the most historically interesting innovation may not be the simple identification of a problem but its definition as an element of public, not merely personal, responsibility.[4]

In contrast to the perspective that emphasizes its origins in one or a few opinion leaders, some writers have attributed the emergence of environmentalism to wider changes in society. According to Samuel Hays, for example, rising affluence led North Americans and Europeans to seek not just the necessities of life but also such amenities as clean air and water. Greater leisure and mobility also played a role by enabling more people to experience natural areas, thereby giving them a stake in their preservation.[5]

The origins of environmentalism have also often been found in science, the source of much of our knowledge of the environment. However, throughout most of history there has been no one distinct area of knowledge considered especially relevant to the environment. Applying the contemporary notion of "environmental science" historically is therefore usually inappropriate. Neither have any areas of science had an intrinsic, inevitable relationship to environmental values. Even ecology, the discipline most often assumed to be tied to environmentalism, has in fact had a very complex relationship with environmental values, contingent on local circumstances.

[3] Grove, *Green Imperialism*; Christopher G. Sellers, *Hazards of the Job: From Industrial Disease to Environmental Health Science* (Chapel Hill: University of North Carolina Press, 1997), p. 12.

[4] Theodore Porter, *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life* (Princeton, N.J.: Princeton University Press, 1995).

[5] Samuel Hays, *Beauty, Health and Permanence: Environmental Politics in the United States, 1955–1985* (Cambridge: Cambridge University Press, 1987).

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Environmental perspectives have provided important revisionist accounts in the history of science. Numerous authors have argued that ever since the scientific revolution Newtonian science, by imposing a strictly materialistic, mechanistic perspective on nature, has served as an instrument for its manipulation. By this account, the domination and exploitation of nature is central to the modern scientific enterprise.[6] In addition, the fragmentation and specialization of science has been said to legitimate the division of nature into separate units, to be studied in isolation and exploited for short-term profit. In contrast, an "environmentalist" science has sometimes been described as necessarily holistic and antimaterialist: rejecting both modern industry and the science that is said to be its servant. Such interpretations exemplify the view of science as a reflection of the underlying values of Western culture.

But historians have also shown that the relation between modern science and environmentalism is more complex than these dichotomous views imply. The historical significance of science to environmentalism cannot be captured by identifying it as simply hostile or supportive or by establishing links between it and underlying cultural values. This complexity makes it necessary to question monolithic theories of science as necessarily exploitationist or preservationist. Rather, this relation can only be understood on the basis of close examination within specific contexts. Scientists may be directed toward specific objectives defined outside their discipline, or they may define their own agendas and ideological perspectives. They may play a variety of complex and ambiguous roles in relation to environmentalism, from assessing policy alternatives to legitimating ethical preferences. Science has often been used as a resource in conflicts over natural resources or environmental risks, and its authority has been subject to critiques from a variety of perspectives.

This chapter will begin with a survey of the history of environmentalism and science since 1800, followed by a discussion of several themes encountered in this history, including the roles and authority of science in environmentalism, and the politics of scientific expertise.

## ENVIRONMENTALISM AND SCIENCE IN THE NINETEENTH CENTURY

By 1800, science could provide a considerable commentary on humanity's relationship with the natural world. From the perspective of natural theology, some argued that nature, having been created by a wise and beneficent Creator, would tend toward stability and balance. Humans were therefore free to manipulate it to serve their needs. Science could assist, by surveying and classifying nature, in identifying useful resources. Carl Linnaeus's

[6] Carolyn Merchant, *The Death of Nature: Women, Ecology, and the Scientific Revolution* (New York: HarperCollins, 1980).

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(1707–1778) *The Oeconomy of Nature* (1749) exemplified this "imperialist" view and the grounding of the authority of its descriptions of a rational, mechanistic nature in an attitude of detached objectivity.[7] Such a perspective complemented the view engendered by the industrial revolution of confidence in the mastery of nature through technology.

But other lessons could also be drawn from the study of nature: that humans are only one among many species and should seek not domination but peaceful harmony through lives of simplicity and humility. Gilbert White (1720–1793) provided the classic statement of this "arcadian" view in *The Natural History of Selborne* (1788). White, and subsequent authors within the Romantic tradition, such as Henry David Thoreau (1817–1862), grounded this perspective in sympathetic, holistic observation by naturalists who saw themselves as part of, not separate from, nature.[8]

Darwinian evolution had ambiguous environmental implications. Although it tended to imply a view not of arcadian harmony but of continual struggle, it also suggested a kinship between humanity and other species. By the end of the nineteenth century, in popularized form, through "realistic" animal stories such as those by Ernest Thompson Seton, and as a result of the work of Darwin, George Romanes, and other students of animal behavior, animals were portrayed as feeling, thinking, and suffering beings, encouraging more humane treatment.[9]

The advent of Darwinism, and these portrayals of animal character, extended into our century a long-standing debate on human–animal relations, largely framed in terms of the ethics of vivisection and the adequacy of mechanistic explanations of animal physiology and behavior. From René Descartes' argument for the irrelevance of human ethics to insensible, unthinking animals to John Locke's contrasting assertion of a moral duty to avoid cruelty to animals and John Ray's description of animals as God's creatures, requiring benevolent stewardship, ethical attitudes toward animals eventually became the focus of organized campaigning. In Britain, agitation by the Society for the Prevention of Cruelty to Animals (formed in 1826, becoming the Royal Society in 1840) culminated in the 1876 Cruelty to Animals Act, which focused on vivisection. Debates concerning this act elicited the participation of much of Britain's scientific elite.[10]

Beyond these broad perspectives – of domination, or harmony with nature – specific environmental conditions, and political and social circumstances, shaped the contribution of science to environmentalism. The history

[7] Clarence J. Glacken, *Traces on the Rhodian Shore: Nature and Culture in Western Thought from Ancient Times to the End of the Eighteenth Century* (Berkeley: University of California Press, 1967), pp. 510–12; Worster, *Nature's Economy*, pp. 31–55.

[8] Worster, *Nature's Economy*.

[9] Thomas Dunlap, *Saving America's Wildlife: Ecology and the American Mind, 1850–1990* (Princeton, N.J.: Princeton University Press, 1988).

[10] Roderick Frazier Nash, *The Rights of Nature: A History of Environmental Ethics* (Madison: University of Wisconsin Press, 1989).

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of environmentalism is inseparable from the broader stream of modern history and such developments as imperial expansion, the growth of economies and cities, the expansion of government, and the evolution of the scientific community.

## THE EMERGENCE OF THE ADMINISTRATIVE STATE

The field sciences were integral to European imperial expansion as botanists, naturalists, and other scientists traveled across the globe, intent on identifying natural resources or research opportunities. But by the early nineteenth century some also began to express concerns regarding the consequences of this expansion. On the island of Mauritius, Pierre Poivre (1719–1786), like a few scientists elsewhere, developed a sophisticated environmental perspective on colonial practices, eventually convincing authorities to enact laws to control deforestation. This tended to occur first on tropical islands, whose small size and status as symbols of paradise enhanced scientists' persuasiveness. Alexander von Humboldt (1769–1859) also contributed, by demonstrating that forest loss, and resulting climate change, was of continental significance, buttressing the "desiccationist" theory linking forest cover to climate (and ultimately to the economic security of the colonies). Through their influence, forest conservation became in India, and eventually in southern Africa and elsewhere, an accepted task of colonial governments, effectively extending the role of the state.[11] George Perkins Marsh (1801–1882), in *Man and Nature* (1864), reinforced concerns about species extinctions and forest clearing, drawing on international evidence of human impacts, including arguments by European colonial scientists, particularly in India. Marsh's portrayal of a harmonious world torn apart by greed and ignorance was widely influential, particularly in the United States.[12]

Governments of industrial nations expanded and transformed themselves during the nineteenth century, assuming a wider range of responsibilities. Economies were demanding more resources, cities were growing, and environmental dangers to health were multiplying. In response, aspects of the environment with immediate economic or health implications began to be perceived as public responsibilities. It became accepted that government should address public concerns, even to the extent of regulating or restricting private activities. For example, stagnant water, a source of malaria and other diseases, could be "conquered" through drainage or by installing fountains, as in France and elsewhere.[13] By the end of the century, urban water supplies

[11] Grove, *Green Imperialism*.

[12] George Perkins Marsh, *Man and Nature: Or, Physical Geography as Modified by Human Action*, ed. David Lowenthal (Cambridge, Mass.: Belknap Press, 1965).

[13] Jean-Pierre Goubert, *The Conquest of Water: The Advent of Health in the Industrial Age*, trans. Andrew Wilson (Cambridge: Polity Press, 1986; 2nd ed., 1989).

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in several industrial nations were being provided by municipal governments, reflecting the new importance of government as mediator of the relationship between society and the environment. In 1863, the British government passed the Alkali Act, intended to control air pollution, and established the first pollution control agency, while concerns about water pollution and its impact on fish stocks led to Salmon Acts in 1861 and 1865.[14] Landscapes themselves began to receive sustained attention from the state. This was the case, for example, in Germany, where forests had long carried great cultural significance, inspiring myths of national origins and national identity: a forest-dwelling people, strong and self-reliant, rooted in their own landscape. In the nineteenth century, these forests were portrayed by such writers as Wilhelm Heinrich Riehl as important not only to Germany's past but to its contemporary economic status and identity. Their protection and management accordingly came increasingly to be seen as an appropriate responsibility of government.[15]

As government expanded its roles, so did science. In Britain, chemists aggressively asserted their authority as objective experts on water purity, even though their advice was of uncertain reliability. These efforts exhibited the developing role of expertise, as reflected in new professional groups, such as those of sanitary engineers and public health physicians that formed in the United States. These groups would dispute heatedly over their rival prescriptions for protecting public health. By the turn of the century, ideas of reform also encouraged perceptions of a close relation between the urban environment, human behavior, and social order.[16]

Concerns regarding aspects of the environment, resulting in an enhanced role for government, were also evident in the application of science to managing natural resources. By the 1870s, Germany had created a model of science-based forestry, effectively reiterating the historic importance, already noted, of trees to German culture and national identity, but in the lexicon of nineteenth-century professional science: university chairs, research programs, and an extensive specialist literature. This model of professional management was subsequently disseminated to India and elsewhere, including the United States, where Gifford Pinchot (1865–1946) applied lessons from Europe to the formation of the U.S. Forest Service. In the arid American West, John Wesley Powell (1834–1902) of the U.S. Geological Survey began in 1878 to assert the need for a scientific basis for water and land management. Science, in his view, could determine how land should be allocated, which areas should be

[14] Roy MacLeod, "The Alkali Acts Administration, 1863–84: The Emergence of the Civil Scientist," *Victorian Studies*, 9 (1965), 85–112; Roy MacLeod, "Government and Resource Conservation: The Salmon Acts Administration, 1860–1886," *Journal of British Studies*, 7 (1968), 114–50.

[15] Simon Schama, *Landscape and Memory* (Toronto: Random House of Canada, 1995).

[16] Christopher Hamlin, *A Science of Impurity: Water Analysis in Nineteenth Century Britain* (Berkeley: University of California Press, 1990); Joel Tarr, *The Search for the Ultimate Sink: Urban Pollution in Historical Perspective* (Akron, Ohio: University of Akron Press, 1996).

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irrigated, and where forests should be protected. Powell subsequently began in 1888 a large-scale survey of the American West to ensure its planned, rational development. Although his opponents would eventually veto this project, expertise would come to help American water development agencies justify large-scale irrigation works, portraying this as the epitome of rational, scientific agriculture, understood as meaning control over nature. Expertise became closely associated with bureaucratic authority, with resource professionals basing much of their authority on the ideal of rational objectivity.[17]

Fisheries research programs were also initiated, beginning in the early 1860s in Norway, in the United States in 1871 by the U.S. Fish Commission, in Britain in 1884 by the Marine Biological Association, and elsewhere. A persistent controversy in fisheries science also became apparent: Whereas Thomas Henry Huxley argued in 1883 that ocean fisheries were potentially inexhaustible, Ray Lankester argued on ecological grounds that fishing would reduce numbers.[18]

By 1900, expertise had come to be seen as essential to the control of nature. In the North American conservation movement, resource management was defined by businesspeople, politicians, and scientists as a technical issue analogous to scientific management in business: Science, backed by government, could help ensure the efficient use of resources for the general welfare.[19] The rise of professional resource management also marked a divergence in social roles for scientists between those acting as activists outside industry and government, often providing wide-ranging critiques of resource use, and managers, who viewed social activism as unprofessional and who focused on enhancing production and solving problems in forestry and other areas of resource management. California scientists provided one model of how activism could be linked to professional identity. By the 1890s, they had developed a distinctive professional role, emphasizing field study and environmental advocacy. Many scientists became activists, particularly as cofounders or participants in the Sierra Club (led by John Muir [1838–1914]). But by World War I this distinctive role had dissipated as California became integrated into the national scientific community.[20]

In Russia, conservationist attitudes had begun to be expressed by the 1850s, particularly by zoologists and agronomists at Moscow University and the Moscow Agricultural Society. These attitudes eventually began to receive official recognition, in a forest code in 1888, a new hunting law in 1892, and efforts to conserve the Northern Pacific fur seal. By 1900, Russian scientists

[17] Donald Worster, *Rivers of Empire: Water, Aridity, and the Growth of the American West* (New York: Oxford University Press, 1985; 2nd ed., 1992).

[18] Tim Smith, *Scaling Fisheries: The Science of Measuring the Effects of Fishing, 1855–1955* (Cambridge: Cambridge University Press, 1994).

[19] Samuel P. Hays, *Conservation and the Gospel of Efficiency: The Progressive Conservation Movement, 1890–1920* (Cambridge, Mass.: Harvard University Press, 1959).

[20] Michael Smith, *Pacific Visions: California Scientists and the Environment, 1850–1915* (New Haven, Conn.: Yale University Press, 1987).

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had developed several arguments for conservation, including the need for areas to be set aside for study.[21]

Although the nineteenth century was marked by governments and scientists asserting important roles in environmental affairs, this did not exclude other actors, particularly those involved in issues less readily addressed by professional expertise. In Great Britain, for example, widespread enthusiasm for amateur natural history study – manifested in a proliferation of field clubs and naturalists' societies – eventually led to concerns about the state of the countryside in the face of industrialization, loss of natural habitat, and, ironically, specimen collecting by naturalists themselves. Numerous organizations advocated preservation of the countryside, such as the Commons, Open Spaces and Footpaths Preservation Society (formed in 1865), the National Trust (1895), and the Society for the Promotion of Nature Reserves (1912).[22]

## ENTERING THE TWENTIETH CENTURY

During the closing decades of the nineteenth century and into the twentieth, the workplaces of rapidly industrializing Europe and the United States became a new arena of interaction between workers, business, the state, and experts. Particularly in Germany and in Britain, a professional scientific perspective on workplace health emerged as the new discipline of industrial hygiene. Secure positions, in universities and government (as in Germany) or in the factory inspectorate (as in Britain), provided vantage points from which researchers could build general perspectives on workplace conditions. In both countries, physicians employed by the state to help ensure occupational health provided a ready audience for these perspectives. Active labor unions and, eventually, employers (motivated at least in part by workers' compensation requirements) also supported attention to workplace conditions.

Industrial hygiene developed less readily in the United States than in Europe, and this difference illustrates the importance of context, particularly firm support from the state, in fostering a new scientific discipline. Official support for expert attention to workplace conditions was at first much less apparent in the United States. Nevertheless, a few researchers, drawing on the European example and seeking to define a new public role for themselves, began to order fragmented knowledge about hazards such

[21] Douglas R. Weiner, *Models of Nature: Ecology, Conservation, and Cultural Revolution in Soviet Russia* (Bloomington: Indiana University Press, 1988).

[22] David E. Allen, *The Naturalist in Britain* (London: Allen Lane, 1976); John Sheail, *Nature in Trust: The History of Nature Conservation in Britain* (Glasgow: Blackie, 1976); P. D. Lowe, "Values and Institutions in the History of British Nature Conservation," in *Conservation in Perspective*, ed. A. Warren and F. B. Goldsmith (Chichester: Wiley, 1983), pp. 329–52.

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as lead and other toxic substances into this new discipline. Alice Hamilton (1869–1970) was the first American to make occupational disease research a full-time career. Through close observation of conditions, and persuasion of company managers, she provoked action on the readily apparent dangers of workplaces. By 1930, industrial hygiene had become recognized by both U.S. health professionals and employers. But while establishing their importance to industry and regulators, industrial hygienists, like resource managers, also asserted their professional autonomy as disinterested holders of a special kind of knowledge, quantitative and uniform.[23]

The turn of the century also saw the emergence of a new scientific discipline that lacked the ties with industry or government characteristic of professional resource management and industrial hygiene. Originating within a variety of institutional contexts, and in response to diverse professional ambitions and opportunities, ecology had coalesced as a distinct discipline by 1894. It had no necessary link with environmentalism: Far from being a unified, holistic perspective, ecology was an amalgam of fragmentary perspectives on the world. The various subdisciplines of ecology, such as plant ecology, animal ecology, and limnology, would retain distinct identities far into this century.

Ecologists had similarly diverse perspectives on environmental questions. While some advocated protection of natural areas in order to preserve research sites, others contributed advice on managing and controlling nature, especially in the service of agriculture. In the United States, ecology developed in both "pure" university contexts (such as the University of Chicago) and more practically oriented land-grant colleges. Thus, after 1880, Stephen Forbes (1844–1930) at the University of Illinois linked ecology with the work of the Illinois Natural History Survey and the state entomologist so that ecology could be applied to the problem of insect pests troubling farmers. And at the University of Nebraska, Charles Bessey and his student Frederick Clements (1874–1945) promoted a school of plant ecology that had significant ties with agricultural research and the challenges facing prairie farmers.[24]

Private patronage also became significant for ecology, supporting research that government lacked the resources and mandate to pursue. Such support helped ecologists develop a "niche" for their research distinct from the focus on immediate problems characteristic of agricultural research stations. Such work did sometimes have environmental implications. For example, ecologists such as Daniel Trembly MacDougal (1865–1958) encouraged Americans to appreciate the desert environment by portraying it not as harsh

[23] Sellers, *Hazards of the Job*.

[24] Stephen Bocking, "Stephen Forbes, Jacob Reighard and the Emergence of Aquatic Ecology in the Great Lakes Region," *Journal of the History of Biology*, 23 (1990), 461–98; Ronald Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895–1955* (Berkeley: University of California Press, 1981).

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and unforgiving but as a place where life could flourish if adapted to those conditions.[25]

By the 1920s, ecology was traveling a variety of pathways. In Britain, concern about pollution and its impacts on salmon and other species, together with a belief in the need for scientific knowledge as a basis for government action, encouraged study of the ecology of freshwater pollution. The Water Pollution Research Board was established in 1927, followed two years later by the research station of the Freshwater Biological Association at Windermere.[26] In the United States, the Bureau of Biological Survey, while fostering research on wildlife, also sought to control, and even eliminate, predators. Eventually, ecologists, led by Charles Elton and Aldo Leopold (1887–1948), among others, began to build a scientific basis for wildlife management. Their efforts also laid the groundwork for changing American attitudes toward wildlife by showing how nature is organized: predator and prey relations, trophic levels, niches, and food chains. Their influence was evident in how nature writers wrote less about individual animals and more about the "web of life." This broader view of nature increasingly encompassed predators, who came to be seen less as evil pests than as a natural part of this web. Leopold's influence was especially noteworthy. He drew on ecology in formulating his land ethic, presented in *A Sand County Almanac.* Published posthumously in 1949, it became an influential statement of environmental ethics.[27]

In the 1920s, Soviet ecologists had considerable influence, advocating protected areas for ecological study and the application of ecology to regional planning and the rehabilitation of degraded land. Russian ecologists such as V. V. Stanchinskii (1882–1942) were also highly innovative theoretically, pioneering phytosociology and the paradigm of ecological energetics. But by the early 1930s their message had been obliterated by Stalinists intent on creating a new society on the basis of a conquered, broken nature. Nevertheless, while it existed, Soviet conservation demonstrated the complexity of factors influencing environmental politics: interagency conflict and the desire of agencies to protect their own interests; contrasting ideas concerning the value of basic science and the value of undisturbed nature; the role of expertise in determining human–nature relationships; and the efforts of a scientific community to respond to changing political conditions.[28]

During the 1930s, industrial hygienists came to dominate studies of emerging issues such as pesticides, industrial chemicals, and air pollution. Researchers who had previously focused on the workplace began to enlarge

[25] Sharon E. Kingsland, "An Elusive Science: Ecological Enterprise in the Southwestern United States," in *Science and Nature: Essays in the History of the Environmental Sciences*, ed. Michael Shortland (Chalfont St. Giles: British Society for the History of Science, 1993), pp. 151–79.

[26] John Sheail, "Pollution and the Protection of Inland Fisheries in Inter-war Britain," in Shortland, *Science and Nature*, pp. 41–56.

[27] Dunlap, *Saving America's Wildlife.*

[28] Weiner, *Models of Nature.*

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their view to the wider environment, and industrial hygiene began to be transformed into environmental health science. Industrial hygienists worked to allay (through stringent standards for proof of harm) concerns about some of the most dangerous industrial products, from leaded gas to DDT. (Eventually they would provide, when drawn on by Rachel Carson [1907–1964] and other writers and activists, the basis for action on these and other contaminants.)[29] By mid-century, concerted action on controlling smoke had also begun within many European and American cities, often only after decades of largely ineffective attempts. Circumstances that now made controls possible included not only greater scientific knowledge of the nature and impacts of air pollution but changing economic conditions, such as the availability of alternatives to coal (particularly gas and electricity) for industrial, transportation, and domestic uses, and a few dramatic events (such as the London smog of 1952) that focused attention on the problem and overcame resistance to the notion of regulating private activities that generate pollution.[30]

In the 1930s, some ecologists presented their discipline as being able to provide a synthetic critique of human society. Perhaps the most prominent exponents of this view were Frederick Clements and Paul Sears (1891–1990), notably in Sears's *Deserts on the March* (1935). This synthetic perspective was, in part, rooted in their experience of the prairie dust bowl and their conviction that destructive land use practices could be reformed on the basis of the equilibrium possible in natural communities, as had been demonstrated by ecologists. Clements's views on ecological succession and climax implied that undisturbed nature could serve as the best guide for land use.[31] Nevertheless, such arguments did not lead to sustained government interest in ecology, and farmers resisted ecologists' prescriptions. And as Arthur Tansley's (1871–1955) critique suggested, Clements's message had less resonance for ecologists in countries such as Britain, where virtually the entire landscape displayed the marks of human activity. Tansley, like other British ecologists, argued that ecological theory should incorporate human agency, not treat it as an invasive impact on otherwise natural communities.[32]

Plant ecologists were not alone in considering the implications of their discipline for society. Animal ecologists at the University of Chicago, under Warder Clyde Allee (1885–1955), argued that human society could learn from the widespread cooperation found in nature.[33] Nevertheless, many ecologists

[29] Sellers, *Hazards of the Job*.

[30] Tarr, *Search for the Ultimate Sink*; Peter Brimblecombe, *The Big Smoke: A History of Air Pollution in London since Medieval Times* (London: Methuen, 1987); Timothy Boon, "*The Smoke Menace*: Cinema, Sponsorship and the Social Relations of Science in 1937," in Shortland, *Science and Nature*, pp. 57–88.

[31] Worster, *Nature's Economy*.

[32] Stephen Bocking, *Ecologists and Environmental Politics: A History of Contemporary Ecology* (New Haven, Conn.: Yale University Press, 1997), pp. 13–37.

[33] Gregg Mitman, *The State of Nature: Ecology, Community, and American Social Thought, 1900–1950* (Chicago: University of Chicago Press, 1992).

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resisted incorporating humans within their discipline, maintaining a long-standing skepticism toward the incipient discipline of "human ecology." They also tended to stress the need to preserve undisturbed areas for ecological study; only "pristine" areas could provide reliable knowledge.

## THE ENVIRONMENTAL REVOLUTION

After the Second World War, some environmental concerns began to reflect a global perspective. In the 1950s, public concern about radiation – likely the first global environmental hazard – emerged in debates about the health effects of nuclear fallout. Studies indicating the accumulation of radioactivity in the Arctic, and of strontium 90 in cow's milk and eventually in milk-drinkers' teeth and bones, added to these concerns. In 1948, two books offered Malthusian, and to some extent ecological, perspectives on a growing human population in a finite world: William Vogt's *Road to Survival* and Fairfield Osborn's *Our Plundered Planet*. New institutions, some at least partly instigated by scientists (such as the International Union for the Conservation of Nature and Natural Resources, formed in 1948) also indicated the growth of an international perspective on environmental issues.

By the early 1960s, public interest in natural areas and pollution was increasing rapidly, and controversies over pesticides, air and water pollution, dams, and other issues proliferated. The Stockholm Conference of 1972 marked the international prominence of environmental concerns. These concerns also became more visible within governments in the developing world (such concerns had long been integrated within strategies of resource use employed by many of their citizens). By 1980, over one hundred nations had one or more environmental agencies, and many had enacted pollution controls, protection of species and natural areas, or assessments of the impacts of proposed developments.[34]

In the 1970s, oil price increases, recession, and a loss of consensus regarding environmental protection encouraged a reaction against environmental initiatives and in favor of requirements that these initiatives be evaluated in terms of their economic implications. Nevertheless, environmentalism continued to evolve as concern about the more obvious forms of pollution broadened to encompass more persistent but less visible forms, such as toxic chemicals, as well as international issues such as acid rain, depletion of the stratospheric ozone layer, and climate change. The last two decades have also been marked by a decreasing tolerance for certain risks, at least in part as a result of improved knowledge and detection capabilities, and a stronger focus on hazards to human health.

[34] John McCormick, *Reclaiming Paradise: The Global Environmental Movement* (Bloomington: Indiana University Press, 1989).

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Science played a variety of roles in the environmental revolution. Research drew attention to problems imperceptible to the public, from depletion of the ozone layer to climate change. The focus on global issues in recent years has reflected, in part, the influence of environmental science, much of which has focused on understanding global systems, under the aegis of programs such as the Intergovernmental Panel on Climate Change and the International Geosphere-Biosphere Program. Since the 1980s, ecologists and conservation biologists have also asserted a more significant role in environmental affairs by presenting loss of biodiversity as a major international concern.[35]

Science also provided ethical inspiration, perpetuating to some extent the arcadian perspective of the nineteenth century. Scientists and science writers, led by Rachel Carson and her book *Silent Spring* (1962), asserted the need for harmony with nature. Eugene Odum, Barry Commoner, and Frank Fraser Darling, among others, elaborated the political implications of ecological ideas, from the cycling of nutrients to the supposed role of diversity in ecosystem stability. Wetlands, forests, and deserts, once seen as worthless, became perceived as interesting, attractive habitats worth protecting. In the United States, the Marine Mammal Protection Act of 1972 and the Endangered Species Act of 1973 reflected the view derived from ecology that not just some but all species, together with their ecosystems, should be protected.

Environmental values also led to new arenas for applying ecological expertise. In North America, the professionalization of park management during the 1960s and 1970s and its reorientation toward ecological priorities generated a larger role for ecological knowledge in national parks.[36] In studies of water pollution, the focus on chemical characteristics most relevant to human uses of the water, such as bacterial content, was broadened to encompass ecological parameters that measure the overall health of aquatic ecosystems.

But at the same time, the relation of science to environmentalism remained ambiguous, being viewed not only as a source of knowledge about environmental problems but as their cause. Thus, while in writing *Silent Spring* Carson drew on scientific evidence of the ecological impacts of pesticides to strengthen her case for a more cautious approach to nature, she also raised questions regarding the authority of certain forms of expertise, and related interest groups, to make decisions that have implications for the general public.[37]

*Silent Spring* was an eloquent example of how a more general critique of science and its application emerged from environmentalism. Resource

[35] David Takacs, *The Idea of Biodiversity: Philosophies of Paradise* (Baltimore: Johns Hopkins University Press, 1996).

[36] Richard West Sellars, *Preserving Nature in the National Parks: A History* (New Haven, Conn.: Yale University Press, 1997), pp. 204–66.

[37] Thomas Dunlap, *DDT: Scientists, Citizens, and Public Policy* (Princeton, N.J.: Princeton University Press, 1981).

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management was criticized as being too narrowly defined, excluding both ecological realities and public concerns. The concept of maximum sustained yield in fisheries became viewed as too simplistic and as ineffective in preventing resource depletion and ecological damage. Institutions such as the U.S. Forest Service and the Forestry Commission in Britain that emphasized efficiency of wood production came under intense scrutiny. The problem, in the view of many, was the centralization of authority within specialized professions and the close ties between professions, interest groups, and government agencies – known in the United States as "iron triangles." The nuclear industry became a special focus of criticism not only because of its environmental implications but because it served as the paradigm of secretive decision making, buttressed by the notion that expertise could solve any problems. Internationally, the Green Revolution, once lauded as a successful effort to use science to meet the food needs of a growing population, was criticized for neglecting the social factors of food production. Such critiques reflected a rejection of the view that technical expertise could be divorced from the social and political contexts of its application.[38]

However, these critiques of science, and a decline of deference toward expertise generally since the 1970s, did not mean that science became less important in environmental affairs. The environmental regulatory system, based on the empirical assessment of environmental risks, has had an enormous appetite for scientific expertise. The emergence of new environmental professions, large-scale research initiatives by government and industry, and the use of expertise by public interest groups testify to the continuing importance of science in environmental politics. Industry has also become since the 1960s increasingly influential in environmental science through its own accumulated expertise and through its insistence on high standards of proof of harm before action could be taken.[39] However, certain criteria, regarding both process and participation, have been applied more extensively to science to reinforce its credibility and legitimacy. Environmental regulations have also imposed requirements, particularly in terms of quantifying impacts and hazards, that have favored certain disciplines able to provide information according to these criteria.

Environmentalism, and the challenges it presents, has created considerable debate among scientists. Many ecologists have resisted involvement in controversy. For example, in the mid-1970s, James Lovelock formulated his Gaia hypothesis: that organisms have the homeostatic capability to maintain global conditions appropriate for life.[40] The idea, and especially its message

[38] Brian Balogh, *Chain Reaction: Expert Debate and Public Participation in American Commercial Nuclear Power, 1945–1975* (Cambridge: Cambridge University Press, 1991); Vandana Shiva, *The Violence of the Green Revolution: Third World Agriculture, Ecology and Politics* (Penang: Third World Network, 1991).

[39] Hays, *Beauty, Health and Permanence*, pp. 359–62.

[40] J. E. Lovelock, *Gaia: A New Look at Life on Earth* (Oxford: Oxford University Press, 1979).

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of the interdependency of all life, has attracted wide public interest. But while Lovelock drew on his scientific understanding of the global atmospheric system in developing his hypothesis, many scientists have resisted its seemingly mystical implications (is the earth a living organism, sharing a name with a Greek goddess?) or have been reluctant to embrace the challenge it poses to the fragmented perspectives of conventional scientific disciplines and to explanations of ecological phenomena strictly in terms of individuals. Only in the late 1980s did scientists begin to give it sustained attention as a scientific concept.[41]

More generally, scientists' reluctance to define critiques of human society as part of their work increased after World War II.[42] Thus, in the last two decades, efforts to synthesize ecology with critiques of society have largely developed outside the natural sciences. Deep ecologists such as Arne Naess, Bill Devall, and George Sessions, and social ecologists such as Murray Bookchin, have drawn selectively on ecology to develop critiques of consumerism, capitalism, and other aspects of Western human society.[43] Their work exemplifies how, outside the scientific community, ecology has often been viewed not merely as another specialized discipline but as a holistic, integrative perspective. For many, "ecology" is not science at all but an ethical perspective or political movement.[44]

Environmentalism has imposed a variety of demands on science for knowledge about air and water quality, patterns of land use, health, or cleaner technologies. Beyond generating new research agendas, the influence of environmentalism also illustrates how, to some extent, scientific knowledge of the natural world has come to be structured in terms of public concerns. Environmental and conservation concerns have encouraged formation of a range of new disciplines, from forestry and wildlife management to toxicology and environmental chemistry. These disciplines represent the outcome of negotiation between public concerns and scientific perspectives. For some scientists, these negotiations have been very positive, particularly in terms of greater research funding. But whereas some scientists have welcomed public prominence and social relevance, others have retreated into their labs because of perceived risks to their scientific credibility and autonomy.

Science and environmentalism have undoubtedly each been important to the other, but scientists' influence has sometimes been muted by their inability to provide a clear "message" for environmentalists. For example, underpinning much of the confidence ecologists had in asserting a prominent

[41] Stephen Schneider and Penelope Boston, eds., *Scientists on Gaia* (Cambridge, Mass.: MIT Press, 1993).
[42] Eugene Cittadino, "The Failed Promise of Human Ecology," in Shortland, *Science and Nature*, pp. 251–83.
[43] Introductions to these and other perspectives can be found in Carolyn Merchant, ed., *Ecology* (Atlantic Highlands, N.J.: Humanities Press, 1994).
[44] Ibid.

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role in environmental affairs during the 1960s was the notion that undisturbed nature is essentially stable. This implied a significant role for ecologists in describing this stability and how it could be maintained or restored. However, in the 1970s and 1980s, many ecologists lost this sense of balance and predictability in nature as it was replaced by impressions of chaos and unpredictability.[45] In recent years, the uncertainty accompanying complex phenomena such as climate change has made it difficult for environmental scientists to argue effectively for effective action, even in the presence of broad scientific consensus.

## THE ROLES AND AUTHORITY OF SCIENCE

The contribution of scientists to the origins of environmentalism raises the issue of their roles throughout the history of environmentalism. In discussing these roles, historians have often focused on factors internal to scientists and their work. For many scientists, personal values have been seen as shaping their role: the arcadian impulses of Gilbert White, the Romantic outlook of Thoreau, the utilitarian perspective of Gifford Pinchot, or the ecological sensibilities of Rachel Carson. Alternatively, their roles have been defined in terms of the content of their science. For example, it has been argued that ecologists have drawn on ecological concepts of stability, balance, competition, and cooperation to derive lessons concerning human conduct. Accordingly, these themes have shaped ecologists' role in environmental politics. Furthermore, whether nature is seen as orderly, deterministic, and balanced or as chaotic, unpredictable, and unstable has influenced the ability of scientists to assert their expertise. Scientists have been more influential when they have had a clear message, and this has been more readily available when nature appears intelligible, not chaotic.[46]

However, effective roles for scientists have also required that they assert the authority of their accounts of the world. In this century, scientists have most often done this by presenting themselves as objective, detached observers, with firm boundaries between their science and their social or political attitudes. Scientists have also often resisted close identification with environmental values, even while presenting their work as relevant to the environment in order to justify state or private patronage. This tension has had special meaning for ecologists, who have often claimed relevance to environmental problems but have differentiated ecology from practical research programs focused on agricultural and resource management while at the same time seeking to assert their neutrality and objectivity.[47]

[45] Worster, *Nature's Economy*.
[46] Ibid., pp. 388–433.
[47] Kingsland, "Elusive Science"; Bocking, *Ecologists and Environmental Politics*, pp. 38–60.

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Such strategies are historically contingent. If contemporary scientists have sought to construct firm barriers between their work and social perspectives, in other contexts, as in nineteenth-century European colonies, scientists' calls for ecological reform were accompanied by demands for social reform, such as better treatment of indigenous peoples. Similarly, whereas in recent decades scientists have frequently distinguished their view of nature from that provided by indigenous knowledge, colonial scientists often drew on such knowledge, recognizing the benefits of drawing from long experience living off the land.[48]

Scientists have also often asserted their authority by arguing that their knowledge is independent of local conditions or experience: that it is standardized knowledge, applicable in any circumstances. This has been evident, for example, in the formation by Humboldt and other scientists of general theories of desiccation to make their observations of the relationship between deforestation and climate change more convincing; in the reduction by American industrial hygienists of the unique combinations of noise, dust, and danger in each factory to the toxicological effects of individual substances or conditions applicable to any workplace or indeed any environment; and in ecologists' development of concepts of matter and energy flows, permitting general principles of ecosystem functioning that need not consider particular species.[49]

Scientists have also often sought links with other disciplines already perceived as rigorous and authoritative. For example, Atomic Energy Commission ecologists adopted techniques used by health physicists in order to trace the environmental movement of radionuclides, while industrial hygienists studying the toxicology of lead poisoning and other occupational diseases focused on internal medical mechanisms, applying forms of explanation seen as legitimate by chemists and physiologists.[50]

In addition, scientists have asserted their authority in environmental affairs by using quantitative methods. But such methods have complex implications. As Theodore Porter has noted, cost–benefit analysis – a leading example of quantification – reflects both application of and distrust of expertise because, like hard, quantitative rules generally, it reduces the discretion available to experts.[51] In environmental contexts, the demand for quantification has added new dimensions to the image of the environment provided by science: necessitating new ways of attaching numbers to intangible or unmeasurable environmental values, as well as privileging those forms of expertise better able to provide quantitative results.

[48] Grove, *Green Imperialism.*
[49] Grove, *Green Imperialism*; Sellers, *Hazards of the Job*; Joel Hagen, *An Entangled Bank: The Origins of Ecosystem Ecology* (New Brunswick, N.J.: Rutgers University Press, 1992).
[50] Bocking, *Ecologists and Environmental Politics*, pp. 63–88; Sellers, *Hazards of the Job.*
[51] Porter, *Trust in Numbers.*

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## POLITICS AND SCIENCE

Environmentalism is not only about attitudes toward nature but about access to resources, division of responsibilities, and building support for one's position. The same can be said about science in environmentalism. For example, while competing strategies of pest control in the United States during the 1960s could be described as reflecting contrasting attitudes of domination over nature or coexistence, these strategies were also shaped by political and institutional factors, including antagonism between federal and state agricultural research establishments and efforts to obtain environmentalists' support in the competition for research funds.[52]

In asserting their roles, scientists have responded to the evolving political context of environmentalism. Of crucial importance was the view that the environment is a collective, not an individual, responsibility and therefore requires government attention. This evolution was part of the expansion of government – often described as the rise of the administrative state – that occurred during the last two centuries. As both the economic significance and the impacts of industries grew, it became more difficult for political authorities to ignore their requirements for natural resources or the risks to health and the environment that they created. In the nineteenth century, in British and French colonies dealing with deforestation or in European nations facing environmental and health threats as a result of industrialization and urban growth, and in the twentieth century in the formation of resource management agencies and, after 1970, environmental agencies, government has provided arenas for the assertion of regulatory authority, in association with expert authority, over economic activities. For scientists, a chief result of the creation of these arenas, particularly since the Second World War, has been the increasing importance of government support as science became accepted as an instrument for the exercise of state authority.

These arenas have taken numerous forms, reflecting differences in national political cultures, including different ideas about the relation between expertise, society, and the environment. For example, ecologists of the postwar British government's Nature Conservancy offered advice to landowners on conserving species and habitats within a corporatist framework that relied on informal persuasion and consensus. In contrast, scientists contributing to environmental controversies in the United States since the 1970s have often done so within a highly adversarial, legalistic framework. These contrasting arenas have imposed very different requirements on expert knowledge.

Demands on expertise have also been shaped by divergent views regarding the appropriate role of government in environmental affairs. Should

[52] Paolo Palladino, "On 'Environmentalism': The Origins of Debates over Policy for Pest-Control Research in America, 1960–1975," in Shortland, *Science and Nature*, pp. 181–212.

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government use its expertise to identify and seek goals for society, or should it merely provide an arena within which private interests can pursue their own goals? Should there be broad participation in environmental decisions, or should participation be limited to agencies and interests with official standing? Frederick Clements had one answer: Ecology could indicate what land use was best suited to a particular site, and this advice could then be implemented by governments, overriding individual greed for the good of society as a whole. The notion of the expert assisting government in identifying and then achieving societal goods has been a persistent theme in scientists' own ideas on how their expertise should be used as reflected, for example, in the Technocratic Movement of the 1930s.[53] But the idea of scientists contributing to more democratic forms of decision making has also been evident. John Wesley Powell, for example hoped to place knowledge of climate and land capabilities in the American West in the hands of individual landowners.[54]

These issues are sharpened by the political significance of environmental expertise. Its exercise has often had political or social implications, as for example when forest protection in colonial India separated indigenous forest users from essential resources, or in California, where science legitimated the exclusion of particular ethnic groups from the fisheries. More generally, the ideology of reliance on professional resource management expertise has had direct political consequences, limiting access to decisions. This has been evident in the reshaping of the rivers of the American West and the concentration of authority over these rivers within managerial agencies.[55] The environmental movement of the 1960s was in part a challenge to this ideology. It asserted the validity of peoples' own concerns and experiences and the need for more open, democratic forms of decision making. At the root of this challenge was the question, acquiring special force in the environmental sphere, of the place of expertise in society. Whereas democratic states assert their legitimacy in terms of the principles and procedures of elections and popular representation, expert authority is grounded in scientific expertise, not in the will of a majority.

Ideas regarding the appropriate scale of response to environmental problems have varied. Are environmental problems local? Or do they demand national responses, even international cooperation? Over the last century, the trend has been to define problems as being of increasingly larger scale: from local, to national, to global in extent. This trend reflects, in part, human society's tendency to spread effluents widely – further downstream, into

[53] Peter J. Taylor, "Technocratic Optimism, H. T. Odum, and the Partial Transformation of Ecological Metaphor after World War II," *Journal of the History of Biology*, 21 (1988), 213–44.

[54] Donald Worster, *An Unsettled Country: Changing Landscapes of the American West* (Albuquerque: University of New Mexico Press, 1994), pp. 12–20.

[55] Grove, *Green Imperialism*; Arthur McEvoy, *The Fisherman's Problem: Ecology and Law in the California Fisheries, 1850–1980* (Cambridge: Cambridge University Press, 1986); Worster, *Rivers of Empire*.

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continental wind patterns, or throughout the stratosphere – in a nearly endless search for the ultimate sink.[56] This trend has also paralleled the formation of national, and subsequently international, institutions and communities of environmental expertise.

In defining the environment as a political issue, specific concerns have been important, not least because they have influenced the perceived relevance of different forms of expertise. When, for example, concerns about protecting natural areas have been prominent, that has resulted in greater demand for ecologists. But when, as in recent decades, concern has focused on the human health implications of environmental risks, it has been toxicologists and epidemiologists who have been called on. This illustrates how, far from merely providing clues to more basic environmental values, environmental concerns themselves deserve focused historical attention.

Environmentalism has often tempted historians to generalize: about its origins, its contemporary significance, and its relation to science. As a result, we now have several synthetic perspectives on the relation of science to society, outlining the larger patterns by which science has been consistent with or opposed to industrial society. More recently, studies of science and the environment in specific contexts have shown how this relation has been shaped by evolving social and economic conditions, by environmental politics and controversy, and by novel scientific ideas and observations. There is no one story here but a multiplicity of stories, reflecting both the diversity of the natural world and the complexity of society.

Science remains a major means by which humanity seeks to comprehend its impact on the world. In a society in which environmental affairs can be the scene of intense disputes over divergent worldviews and conflicting interests, the portrayals of the environment provided by scientists are themselves often fiercely contested. Effectively addressing current environmental problems, and avoiding those on the horizon, will require using science effectively. Doing so can be furthered by understanding how science has been shaped by its own history and by the society within which it is created.

56 Tarr, *Search for the Ultimate Sink*.

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# 33

# POPULAR SCIENCE

*Peter J. Bowler*

Much recent historical work has focused on the role played by popular science in nineteenth-century culture.[1] This was indeed a period when major developments took place in the way science was related to the general public, but we must beware of the assumption that the growing specialization of science at the end of the century created a situation that has continued unchanged to the present. In this chapter, I take up some of the themes explored by authors writing on the nineteenth century and trace them to the present, especially with regard to keeping up the pressure on an older view of science popularization that most historians now find unsatisfactory. This is the "dominant" view of popularization, which came to the fore in the mid-twentieth century, according to which science is done by a specialized elite and the results are then simplified for transmission to a largely passive public by intermediary science writers who may not be scientists themselves but who have the interests of the scientific community at heart. Few now accept this "top-down" model as an adequate representation of the complex interaction between science and the public, and this chapter will try to show why. In effect, we shall see that the more complex situation that prevailed during the nineteenth century was temporarily and only partially eclipsed by the efforts of the scientific profession to adopt a more isolationist position in the early and middle decades of the twentieth.

## THE "DOMINANT VIEW" AND ITS CRITICS

The "dominant view" of science popularization was formulated as an explicit model in the 1960s, when the role of science seemed more secure and far less

[1] For a survey of this literature, see David Knight, "Scientists and Their Publics: Popularization of Science in the Nineteenth Century," in the companion volume *The Cambridge History of Science*, vol. 5: *The Modern Physical and Mathematical Sciences*, ed. Mary Jo Nye (Cambridge: Cambridge University Press, 2003), pp. 72–90.



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controversial than it is today.[2] It was very much driven by the assumption that popularization was the transmission of a message about science from those "in the know" to a public eager to learn. Writing about science was looked down on by active members of the scientific community because transmitting knowledge was less important than producing it. This was the era of the professional science writers, who took over the less glamorous job of popularization now rejected as beneath the dignity of the scientists themselves. Sociologists dealing with modern science have joined hands with historians in criticizing this model as inadequate to deal with the real world. It was premised on the existence of a secure scientific elite that spoke with one voice and expected its pronouncements to be accepted without question by the media, the government, and the public. Needless to say, this situation does not obtain in the modern world and was probably a gross oversimplification of the situation in the 1960s, although scientists then were less used to dissent among their own ranks and to public criticism of their work than they are today.[3]

There are two fairly obvious reasons why we should be suspicious of any model that sees the public as an essentially passive recipient of information about science. The first is that communication rarely if ever assumes a purely passive audience. Even when writing for other scientists, a scientist-author is seeking to persuade the audience that a particular interpretation of the facts is most plausible, and the success of the project depends on how the audience responds to this and rival suggestions. That is why scientific revolutions are complex processes that need to be understood at a sociological as well as a technical level. But the same rhetorical demands are required when writing for a general audience, and here, too, the audience has interests that will shape how it responds to the material being presented to it. As Moore (Chapter 29, this volume) and Bowler (Chapter 30, this volume) confirm, science is seldom being purveyed to the public in a neutral light. The science writer often has a point to make about the significance of the science he or she is describing, and the public (or rather the different groups that make up the public) will respond as they think fit. Even when the motive for writing is ostensibly educational, the science popularizer generally has a wider agenda in the form of encouraging more public support for science.

More generally, popular science covers a much wider territory than the popularization of science. In the nineteenth century, popular lectures and exhibitions were important vehicles by which science was promoted, and here the element of display was paramount. The exhibition played a key role in defining the perceived link between the physical sciences and technology,

[2] For an account of the dominant view, see Richard Whitley, "Knowledge Producers and Knowledge Acquirers: Popularization as a Relation between Scientific Fields and Publics," in *Expository Science: Forms and Functions of Popularization,* Sociology of the Sciences Yearbook, vol. 4, ed. Terry Shinn and Richard Whitley (Dordrecht: Reidel, 1985), pp. 3–28.

[3] See Stephen Hilgartner, "The Dominant View of Popularization: Conceptual Problems, Political Uses," *Social Studies of Science*, 20 (1990), 519–39.

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but the application of new technologies to medicine also received much attention.[4] A similar role was played by nonacademic educational venues such as London's Royal Institution, where the well-off public came to be impressed by lectures and demonstrations. The role of display was also increasingly crucial to the natural history museums that flourished into the twentieth century, those "cathedrals of science" that brought home to the European and American publics the breadth of the natural world now being conquered by science (see Winsor, Chapter 4, this volume).

In natural history and a few other areas, including astronomy, it was still possible for the informed amateur to make a serious contribution to science, despite the ever-increasing levels of specialization and professionalization (see Allen, Chapter 2, this volume). Even today, amateurs still discover important fossils and comets and play a role in ecological and astronomical surveys. Here the scientific elite cannot talk down to the public in quite the same way as they might, for instance, in nuclear physics. The ability of the public to control what is recognized as science has served as a significant counterweight to the influence of the scientific community, as for instance when phrenology retained a wide public acceptance despite being dismissed as a pseudoscience by the elite. Here popular science was able to challenge the authority of elite science – a far cry from the top-down scenario imagined by the dominant view of popularization. If the public's willingness to challenge the elite diminished temporarily in the mid-twentieth century, modern controversies over genetic engineering and the environment have once again convinced scientists that they have to work hard if they are not to lose the trust of those they claim to benefit.

## NINETEENTH-CENTURY POPULAR SCIENCE WRITING

From the viewpoint of the historian, the interesting question is how the somewhat oversimplified situation that may have temporarily gained currency in the mid-twentieth century came into existence. As the studies already cited show, the dominant view was certainly not applicable to the nineteenth century, when the deeper implications of science were openly debated and scientists were struggling among themselves to articulate a view of what the scientific community should look like. Our understanding of the popular print culture of the mid-nineteenth century, and of the role of science within it, has been transformed by studies such as those of James A. Secord on the reception of Robert Chambers's *Vestiges of the Natural History of Creation*. Drawing on the burgeoning field of the "history of the book," Secord not only shows us how a controversial work reached the public but also the diversity

[4] See Iwan Rhys Morus, *Frankenstein's Children: Electricity, Exhibition and Experiment in Early Nineteenth-Century London* (Princeton, N.J.: Princeton University Press, 1998); Carolyn Marvin, *When Old Technologies Were New: Thinking about Communications in the Late Nineteenth Century* (New York: Oxford University Press, 1988).

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of popular reactions to it in different walks of life and different parts of the country. Here we see popular writing directly influencing the way people thought about an important scientific issue and, according to Secord, directly shaping the way Charles Darwin's *Origin of Species* would be read.[5] And, as in the case of phrenology (which Chambers supported), the role of popular science was to challenge, not to endorse, the view promoted by the elite of the scientific community.

Chambers's book deliberately sought to define a niche between the radical, materialistic popular science uncovered by Adrian Desmond in the earlier decades of the century and the promulgation of natural theology by more orthodox writers. At this point, it was by no means unusual for those who were not directly practicing science to write about it for the general public. On into the later decades of the nineteenth century, there was a steady flow of literature developing the theme of natural theology, much of it written by women who had contacts with the scientific community. The situation began to change in the 1860s, although Alvar Ellegård's pioneering study of the response to Darwin still relied largely on the traditional periodical press.[6] But practical developments in print technology, taxation, and the postal system now allowed a new generation of popular journals to spring up. Moreover, the increasingly specialized scientific community was beginning to see the need for a new ideology of science to be articulated. This was the era of Thomas Henry Huxley's scientific naturalism, which sought to wrest cultural authority from the churches by identifying professional science with hostility to organized religion. Bernard Lightman suggests that Huxley was tempted into the arena of popular science writing precisely because he realized that the supporters of natural theology were still remarkably successful in defending the older view of science's implications. At the same time, though, he was reluctant to become identified with the more extreme opponents of religion, some of whom – especially C. A. Watts and the Rationalist Press Association – were active in the area of popularization.[7]

[5] James A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception and Secret Authorship of Vestiges of the Natural History of Creation* (Chicago: University of Chicago Press, 2000). See also Adrian Desmond, *The Politics of Evolution: Morphology, Medicine and Reform in Radical London* (Chicago: University of Chicago Press, 1989); Steven Shapin, "Science and the Public," in *Companion to the History of Modern Science*, ed. R. C. Olby, G. Cantor, and M. J. S. Hodge (London: Routledge, 1990), pp. 990–1007; Roger Cooter and Stephen Pumphrey, "Separate Spheres and Public Places: Reflections on the History of Science Popularization and on Science in Popular Culture," *History of Science*, 32 (1994), 232–67; Roger Cooter, *The Cultural Meaning of Popular Science: Phrenology and the Organization of Consent in Nineteenth-Century Britain* (Cambridge: Cambridge University Press, 1985).

[6] Alvar Ellegård, *Darwin and the General Reader: The Reception of Darwin's Theory of Evolution in the British Periodical Press, 1859–1872* (Goteburg: Acta Universitatis Gotenburgensis, 1858; repr. Chicago: University of Chicago Press, 1990).

[7] Bernard Lightman, "'The Voices of Nature': Popularizing Victorian Science," in *Victorian Science in Context*, ed. Bernard Lightman (Chicago: University of Chicago Press, 1997), pp. 187–211; Bernard Lightman, "The Visual Theology of Victorian Popularizers of Science: From Reverent Eye to Chemical Retina," *Isis*, 91 (2000), 651–80; Bernard Lightman, "Ideology, Evolution and Late-Victorian Agnostic Popularisers," in *History, Humanity and Evolution: Essays for John C. Greene*, ed. James R. Moore (Cambridge: Cambridge University Press, 1989), pp. 285–309.

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Ruth Barton and Roy MacLeod have explored the varying fortunes of journals (including *Nature*) and books such as the International Scientific Series, introduced to promote the professional scientists' image of their work.[8] As the balance gradually shifted away from overt appeals to divine wisdom, the new generation of professionals sought to create a domain of what Frank Turner calls "public science," in which concern for science's social impact required everyone to have a working understanding of its methods and conclusions.[9] Much of this rhetoric was inspired by frustration because up to the time of World War I, British government and industry had failed to live up to earlier promises of support for science. Significantly, the International Scientific Series, at first highly successful, eventually came to an end because later books were perceived as too similar to textbooks. Capturing the public's attention required something tailored more directly for the popular reader, and fewer scientists were now able to provide the right level of material.

Although much historical attention has focused on the situation in Britain and the United States, other countries were also experiencing rapid growth in science, and their publics were also having to come to terms with these developments. In Germany, the scientific profession became firmly established sometime before this was achieved in the English-speaking world, but like other German academics, scientists there had to argue for their subject being accepted on the grounds of its contribution to philosophy and culture, as well as to industry. Science was also promoted as an agent of political liberalism, and not just by radical materialists. Even Rudolph Virchow, easily dismissed as a conservative because of his opposition to Ernst Haeckel's monistic Darwinism, was prepared to present science as the basis for a value system that challenged traditional sources of moral and political authority. But by the end of the nineteenth century, it was Haeckel's synthesis of radical and Romantic ideas that was presented most effectively to the public as the symbol of science's ability to transform culture and society.[10]

France had highly popular (and widely translated) popular science writers such as Camille Flammarion, and the role played by Jules Verne's pioneering science-fiction novels should not be ignored. This reminds us that popular

[8] Ruth Barton, "Just before Nature: The Purposes of Science and the Purposes of Popularization in Some English Popular Science Journals of the 1860s," *Annals of Science*, 55 (1998), 1–33; Roy MacLeod, "The Genesis of *Nature*," *Nature*, 224 (1969), 423–40; Roy MacLeod, "Evolutionism, Internationalism and Commercial Enterprise in Science: The International Scientific Series, 1871–1910," in *Development of Science Publishing in Europe*, ed. A. J. Meadows (Amsterdam: Elsevier, 1980), pp. 63–93. MacLeod's articles are reprinted with the original pagination in Roy MacLeod, *The 'Creed of Science' in Victorian England* (Aldershot: Ashgate Variorum, 2000).

[9] Frank M. Turner, "Public Science in Britain, 1880–1919," *Isis*, 71 (1980), 589–608.

[10] Kurt Bayertz, "Spreading the Spirit of Science: Social Determinants of the Popularization of Science in Nineteenth-Century Germany," in Shinn and Whitley, *Expository Science*, pp. 209–27; Andreas Daum, *Wissenschaftspopularisierung im 19 Jahrhundert. Burgerliche Kultur, naturwissenschaftliche Bildung und die deutsche Offentlichkeit, 1848–1914* (Munich: Oldenbourg, 1998); Constantin Goschler, ed., *Wissenschaft und Offentlichkeit in Berlin, 1870–1930* (Stuttgart: Franz Steiner, 2000). See also J. Schikore, "The Task of Explaining Sight – Helmholtz's Writings on Vision as a Test Case for Models of the Popularization of Science," *Science in Context*, 14 (2001), 397–417.

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science writing and science fiction worked hand in hand to create an image of what science could do to transform people's lives.[11] This image was certainly based on accounts and predictions of technological marvels, but it also reflected new ideas about the origins of life and humanity produced by paleontology and evolution theory. Louis Figuier's *La terre avant le deluge* (*The World before the Flood*) of 1863 included striking visual representations of past life, which in Verne's *Voyage au centre de la terre* (*Journey to the Center of the Earth*) still survived in the vast cavern at the center of the earth. Flammarion's *Le monde avant le creation de l'homme* (The World before the Creation of Man) of 1886 also explored the new ideas about the past, and by the last decade of the century, the genre of the "prehistoric" novel was well established. The parallels between early paleoanthropologists' theories of human origins and traditional creation myths has been noted by Misia Landau.[12] But as Martin Rudwick points out, the visual recreations of the past were also potent influences on people's imaginations, eventually feeding into areas as diverse as museum displays and advertising. Waterhouse Hawkins's life-sized reconstructions of prehistoric animals, still to be seen at the Crystal Palace site in Sydenham, south London, brought the dinosaurs and other extinct animals almost to life, at least as far as the cartoonists were concerned.[13]

## THE EARLY TWENTIETH CENTURY

By the early years of the twentieth century, the situation had begun to change once again. The scientific community was fairly well established, although still lacking in adequate support in Britain and the United States. Serious differences of opinion began to emerge between those who wanted to continue the demand for more government and industrial support – and were prepared to appeal for it through the media – and those who wanted to preserve the scientists' traditional independence and who looked on popularization as at best a waste of good research time and at worst something that demeaned the profession. But the mass media were changing, too, beginning the creation of modern popular culture through the establishment of new mass-circulation newspapers and periodicals and cheaper books. It was by no means clear that those who edited these new vehicles actually wanted scientists to write for them: Peter Broks points to the editor W. T. Stead, who in 1906 openly proclaimed his reluctance to employ experts to write popular articles on the

[11] On science fiction, see for instance I. F. Clarke, *The Pattern of Expectation, 1644–2001* (London: Book Club Associates, 1979); Paul Fayter, "Strange New Worlds of Space and Time: Late Victorian Science and Science Fiction," in Lightman, *Victorian Science in Context*, pp. 256–80.

[12] Misia Landau, *Narratives of Human Evolution* (New Haven, Conn.: Yale University Press, 1990).

[13] Martin J. S. Rudwick, *Scenes of Deep Time: Early Pictorial Representations of the Prehistoric World* (Chicago: University of Chicago Press, 1992). See also Claudette Cohen, *The Fate of the Mammoth: Fossils, Myth and History*, trans. William Rodarmor (Chicago: University of Chicago Press, 2002).

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grounds that they could never avoid technicalities.[14] In fact, there was still a good deal of interaction between scientists and the public, but it was by no means on terms dictated by the scientific professionals. As Broks insists, the situation was one of negotiation, not of simple dissemination, because scientists, publishers, and members of the public themselves all had their own interests to bring to bear on the popular image of science.

The early twentieth century was indeed the period when the myth of the "disinterested" scientist who avoided public debate and popular writing was constructed. Later, scientists with left-wing views openly proclaimed their social conscience and their willingness to educate and engage with the general public. Scientist-writers such as Lancelot Hogben and C. S. Waddington lamented the cowardice of an earlier generation that had sold out to capitalist industry by refusing to think beyond their narrowly specialized research.[15] And it is true that some high-flyers such as Hogben himself and also the young Julian Huxley were seen to be risking their careers, especially their chances of earning the coveted Fellowship of the Royal Society, by wasting time on popular writing. From the opposite side, there were editors and publishers such as Stead, who thought that few scientists could write successfully for a nonspecialist readership and preferred that journalists do the job. Given that there were at this point hardly any specialist science journalists, this meant that an image of science could be constructed by outsiders who portrayed the scientist as a remote figure engaged in esoteric work that had little contact with real life. Areas such as natural history, where highly trained amateurs still played a role, were sidelined and presented as not quite the same thing as laboratory science.

Yet this was by no means the whole story. Scientists did write for the public in the early twentieth century, and we need to know how and why. Those who made a success out of it were evidently a selected group because Stead was right to suppose that only a few scientists had a real gift for nonspecialist communication. Most could write a semipopular book aimed at the serious reader, but few could write at the level that worked in popular magazines, let alone newspapers. The ones who did succeed at this broader level were not necessarily the big names that would be remembered by later historians of science. A few, including the biologist J. Arthur Thomson and eventually Julian Huxley himself, did abandon scientific research almost completely for nonspecialist writing. Others, including Hogben, Waddington, and most of the better-known figures mentioned later in this chapter, kept up their research and retained their credibility as professional scientists. Many junior figures almost certainly wrote for the money, given the very poor salaries offered to nonprofessorial academic and research scientists.

[14] Peter Broks, *Media Science before the Great War* (London: Macmillan, 1996), p. 34.
[15] Gary Wersky, *The Visible College: A Collective Biography of British Scientific Socialists of the 1930s* (London: Allen Lane, 1978).

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But, in the end, those who became well known for trying to reach out to the public almost certainly had some kind of wider motive, religious or ideological. Some wanted to spread an understanding of science to ordinary people, convinced that the creation of an educated general public was the best way of destroying social privilege. E. Ray Lankester's pioneering "Science from an Easy Chair" articles in the *Daily Telegraph* were a continuation of the efforts of his mentor, T. H. Huxley, at mass communication. The notion that science could play a role in moral education was widely promoted. The notion of *Discovery* (the title of one British magazine) was crucial for showing the scientist (including at this stage the archaeologist) as a disinterested, but also imaginative, searcher after truth.[16] Some wrote openly for religious or philosophical instruction, including J. Arthur Thomson, A. S. Eddington, and James Jeans, and were accused by their opponents of trying to re-create natural theology. Materialists complained that a small number of eminent and rather elderly figures were able to create an unrealistic impression of the wider implications of science because they were able to promote out-of-date ideas successfully to the general public. The later generation of socialist writers, including Hogben and J. B. S. Haldane, were more self-conscious about the political ideology they built into their books and articles and tended to stress the practical value of science for improving people's lives.

In the United States, scientists hoping to generate a new level of government support for science were active in reaching out through books and magazines, and here, too, there were other members of the scientific community who distrusted their motives.[17] The notion of science speaking with one voice to the public was an illusion, but wider appreciation of this fact depended on those with rival positions being willing to spend the time necessary to stop their opponents from creating the illusion of a monolithic enterprise.

Surveys of magazines in Britain and the United States both show that a significant proportion of the material about science in the early decades of the twentieth century was written by scientists or was based on interviews with them.[18] My own research on popular science books in the same period shows that the publishers of popular educational series were only too glad to have real "experts" writing for them. J. Arthur Thomson was the science editor of the "Home University Library," which issued short, cheap books on academic topics and paid an advance of £50 to the author (equivalent to

[16] See Anna-K. Mayer, "'A Combative Sense of Duty': Englishness and the Scientists," in *Regenerating England: Science, Medicine and Culture in Inter-war Britain*, ed. Chris Lawrence and Anna-K. Mayer (Amsterdam: Rodopi, 2000), pp. 67–106.

[17] Ronald C. Tobey, *The American Ideology of National Science, 1919–1930* (Pittsburgh, Pa.: University of Pittsburgh Press, 1971).

[18] Broks, *Media Science before the Great War*; Marcel La Follette, *Making Science Our Own: Public Images of Science, 1910–1955* (Chicago: University of Chicago Press, 1990).

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one-third of a junior scientist's annual salary).[19] A few scientists, and not just the famous ones, soon began to broadcast on the new medium of the radio. In Britain, radio was controlled by the British Broadcasting Corporation (BBC), which was instituted with an explicit bias toward education rather than entertainment. There is little audience research from this period, but anecdotal evidence suggests that most listeners found a "talk" given by an expert, scientist or otherwise, to be pretty boring.[20]

Although much of the material written to educate the general public was intended to provide general coverage of all areas of science, there were clearly certain topics and certain areas that stimulated press interest because of their perceived revolutionary nature. This was true for some aspects of biology and psychology. Julian Huxley leapt to fame because his work on growth hormones in the axolotl evoked images of an elixir of life, and the radical implications of Freudian psychology were widely reported. But perhaps the most dramatic coverage focused on new developments in physics and cosmology, especially relativity and quantum mechanics. Albert Einstein's elevation to almost mythic status as the author of an incomprehensible theory that overturned all traditional certainties has been explored at length by historians.[21] A more specific event that has also attracted attention is the rivalry between Arthur S. Eddington and James Jeans, who vied with one another (at the instigation of Cambridge University Press) to produce best-sellers exploring the ways in which the new physics overturned the old image of a clockwork universe. Michael Whitworth's study of these and other widely read authors offers a model of what can be done to understand the relationship between scientists, publishers, and the public when detailed publishers' records are available.[22]

It is widely supposed that the profession of science writer or science journalist only came into existence in the 1930s. In the United States, a science news service edited by Edwin E. Slossen was founded in 1920 to supply the press with information. The National Association of Science Writers was

[19] Peter J. Bowler, "From Science to the Popularization of Science: The Career of J. Arthur Thomson," in *Science and Beliefs: From Natural Philosophy to Natural Science, 1700–1900*, ed. M. D. Eddy and D. Knight (Aldershot: Ashgate, 2005), pp. 231–48. More generally, see Peter J. Bowler, "Experts and Publishers: Writing Popular Science in Early Twentieth-Century Britain, Writing Popular History of Science Now," *British Journal for the History of Science*, 39 (2006), 1–29.

[20] See the ascerbic comment from Collie Knox in the *Daily Mail*, May 29, 1934, quoted in D. L. Le Mahieu, *A Culture for Democracy: Mass Communication and the Cultivated Mind in Britain between the Wars* (Oxford: Clarendon Press, 1988), p. 275. For a more positive view of radio's influence, see Mark Pegg, *Broadcasting and Society, 1918–1939* (London: Croom Helm, 1983), p. 208. Even with today's high-tech TV programs, however, research suggests that the audience for popular science is quite limited.

[21] For example, Alan J. Friedman and Carol C. Donley, *Einstein as Myth and Muse* (Cambridge: Cambridge University Press, 1986); Michel Biezunski, "Popularization and Scientific Controversy: The Case of the Theory of Relativity in France," in Shinn and Whitley, *Expository Science*, pp. 183–94.

[22] Michael Whitworth, "The Clothbound Universe: Popular Physics Books, 1919–39," *Publishing History*, 40 (1996), 53–82.

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founded in 1934, initially with only eleven members, although its numbers increased dramatically after World War II.[23] In Britain, it is claimed that there were only three professional science writers before the war, and only in 1947 was the Association of British Science Writers formed. But such figures are misleading. Although many popular science books were written by working scientists, much of the copy for periodicals and newspapers had always been supplied by journalists who perforce had to educate themselves in science. What changed in the 1930s was the emergence of specialist science journalists who might have a degree in science and who were able to mix with the research scientists on terms that guaranteed them access to news about the latest discoveries. They were also able to present themselves to the scientists as representing the interests of science itself in a world where good publicity was becoming increasingly important.

## LATER DEVELOPMENTS

As the new profession of science writer consolidated itself, its members became increasingly anxious to persuade the scientists that only those with appropriate literary skills could present material on science in a form that the public would be able to assimilate. This reinforced the originally quite limited suspicions of some scientists against those in their own profession who dealt with the public directly. The middle decades of the century thus became the heyday of the "dominant view" of popularization, in which the production of scientific knowledge was quite distinct from its dissemination, and those who made the knowledge were not supposed to engage directly with the mass media. Dissemination was a one-way process because the special nature of scientific knowledge made it secure from questioning by a public that was only capable of passively absorbing a simplified version of what had been discovered. In the postwar years, there was an increased tendency within the science profession to exclude those who circumvented the peer-reviewed system of research publication by dealing with the press directly. The editor of the *New England Journal of Medicine*, Franz Ingelfinger, declared that he would not accept any paper from a scientist who had already announced his or her discovery to the mass media. The Ingelfinger Rule essentially consolidated the growing separation between the professional scientist and the science writer, but did so by making interactions between them a delayed-action and entirely one-way process. Small wonder that in the world of science-based industries, which began to emerge in the late twentieth century, such a rule became untenable – although the scientist may speak to a patent attorney

[23] B. Dixon, "Telling the People: Science in the Public Press since the Second World War," in Meadows, *Development of Science Publishing in Europe*, pp. 215–35; Jane Gregory and Steve Miller, *Science in Public: Communication, Culture, and Credibility* (New York: Plenum Press, 1998); Hillier Krieghbaum, *Science and the Mass Media* (New York: New York University Press, 1967).

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or commercial backer before he or she speaks to a journalist. Only in the presentation of broad-brush overviews has a selected company of scientists continued to write for the general public, with figures such as Carl Sagan and Stephen Jay Gould achieving international fame and colossal sales. Even here, one suspects that the ghost-writer will eventually come to provide an essential intermediary.

New vehicles for exploring science and its implications were becoming available. Mid-twentieth-century magazines were already much better illustrated than their nineteenth-century predecessors, and the advent of color photography added a new dimension to their ability to impress. Under successful organizations such as the National Geographic Society, magazines could create the impression that the reader was actually involved in exploration, and important expeditions were initiated by this kind of publicity. Applied to controversial areas such as primatology, *National Geographic* magazine was able to directly shape the public's perception of what was going on, in this case by promoting the work of female primatologists such as Jane Goodall.[24] Museums were also increasingly conscious of their ability to shape the public perception of science and nature, and Carl Akeley's dioramas at the American Museum of Natural History pioneered a new era of creating the illusion that the participant was directly experiencing what was, in fact, a carefully crafted representation of the natural world. Great exhibitions continued to focus public attention on those aspects of science and technology that government and industry most wanted to promote, sometimes with the active involvement of scientists seeking to shape public opinion.[25] Mention has already been made of radio, and by the mid-twentieth century the movie was increasingly being applied to present a scientific view of the natural world through documentaries.[26]

In the latter half of the century, television became the most powerful medium by which science could be popularized – or criticized. Now the science writers were joined by the TV producers in promoting what was initially perceived as the public understanding of an unquestionable body of scientific knowledge, and some scientists – Carl Sagan was a good example – adapted well to the new environment. But the naive expectations of the scientific community increasingly were challenged as the media and the public began to realize the extent to which the authorities sought to manipulate

[24] Donna Haraway, *Primate Visions: Gender, Race and Nature in the World of Modern Science* (London: Routledge, 1990). See also Ronald Rainger, *An Agenda for Antiquity: Henry Fairfield Osborn and Vertebrate Paleontology at the American Museum of Natural History, 1890–1935* (Tuscaloosa: University of Alabama Press, 1991).

[25] On exhibitions (or expositions as they came to be called in the twentieth century), see for instance Jacqueline Eidelman, "The Cathedral of French Science: The Early Years of the 'Palais de la Decouverte'," in Shinn and Whitley, *Expository Science*, pp. 195–207; Sophie Forgan, "Atoms in Wonderland," *History and Technology*, 19 (2003), 177–96.

[26] Greg Mitman, *Reel Nature: America's Romance with Wildlife on Film* (Cambridge, Mass.: Harvard University Press, 1999).

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their perception of what is going on. The Internet has now bypassed the official channels of communication and has made it virtually impossible for any decision by science, government, or industry to go unchallenged. In these circumstances, the image of a passive public receiving information as outlined in the once "dominant view" of popularization has been replaced by a growing acceptance by the elite and the professions that they have to engage with the public and respond to its concerns. It is an irony of history that this situation to some extent recreates the atmosphere characteristic of the earlier form of popular science that emerged before the scientific community began to think of itself – in C. P. Snow's terms – as a separate culture that everyone else ought to know something about.[27]

[27] C. P. Snow's Reith lecture of 1959 complained that the literary elite had begun to ignore science, but Snow himself was actively involved in the effort to popularize science to the general public. See C. P. Snow, *The Two Cultures: A Second Look* (Cambridge: Cambridge University Press, 1969).

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