SCIENTISM

THE NEW ORTHODOXY

EDITED BY RICHARD N. WILLIAMS
AND DANIEL N. ROBINSON

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Scientism: The New Orthodoxy

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Introduction

Richard N. Williams

Perhaps the most obvious and impressive effect of science in contemporary culture is the technology that science has produced. On most accounts, technology and what it has produced are positive; however, it has also created some dramatic, and some subtle, shifts in how we live and what things mean to us. Some technologies can even create moral issues that would not exist if it weren't for the technology itself. Examples include decisions as to whether to prolong life or to terminate life before birth by means of technology. The important point is that technology, and therefore the science underlying the technology, have significant effects on the meaning and morality of our lives. Thus, both science and how we understand it have real effects on many aspects of our lives even though we do not usually think about science in this way.

This introduction is not meant to focus attention on technology. Rather, the intent is to introduce a much broader issue—how it is that intellectual endeavors, including metaphysical assumptions, find their way into cultures and into individual lives. The effect of technology is only a model of that process, a process that is more subtle than it may seem. In his essay entitled "The Question Concerning Technology," Martin Heidegger (1977) writes about the effect of modern technology and how we understand ourselves and our lives. Heidegger proposes that

> [t]he threat to man does not come in the first instance from the potentially lethal machines and apparatus of technology. The actual threat has already afflicted man in his essence. The rule of enframing threatens man with the possibility that it could be denied to him to enter into a more original revealing and hence to experience the call of a more primal truth. (p. 309)

Heidegger here expresses the concern of many, that successful effective technology subtly invites us to entertain the possibility that all problems—including distinctly human problems—are merely technological problems. Technology is the ultimate pragmatism, and in a world dominated by technology, and intoxicated by technological solutions to practical problems, everything is viewed as "standing in reserve," ready to be used by or subjected to some sort of technology or technological process. Human problems are regarded as if they were analogous to technological problems. Solutions are sought that consist chiefly in introducing a change agent into the closed causal system of the entity (machine) that has gone wrong. In such a world, it is almost irresistible to "enframe" or perceive ourselves as subject to technological problems, or waiting for technological solutions to our problems and technological enhancements to our lives. It is as if technology has become the sweeping metaphor of our age.

It is not a stretch to suggest that our modern age really is, in some important senses, the age of science. Success at manipulating the world, as technology provides, strengthens our sense that science is showing us what really is the case in the world we inhabit. This book deals with the effects of some ways of thinking about science, what it is, and what it can do. A particularly problematic way of understanding science has been described as "scientism." Scientism is an enframing of life and world that has generated such enthusiasm and enjoyed such popularity that it often receives unquestioned acceptance by scholars and by the public. Partly because scientism has become the new orthodoxy, understanding the implications of enframing ourselves and our world in the way scientistic thinkers do is a matter of great significance and consequence. However, it is crucial to distinguish between science and scientism. The chapters in this volume seek to clarify the distinctions, challenge the ideas that undergird scientism, and defend science from misappropriation.

Science and scientism

The concerns expressed by many about the effects of technology on culture, human life, and even on our understanding of ourselves are mirrored by substantive concerns about the effects of over-reliance on and overconfidence in science as the source of knowledge regarding all aspects of human life and,

ultimately, all human problems. This overconfidence and over-reliance is the starting point for understanding scientism, which is the central focus of the chapters in this volume. It must be made clear at the outset, however, that to express a concern about, or to criticize over-reliance on and overconfidence in science is not to oppose science or to diminish its accomplishments. Indeed, as will be argued in the chapters in this volume, scientism itself does no good service to science *qua* science. Rather, it attempts to hijack science to support metaphysical commitments in which science has no particular interest, and to which it owes no particular debt. While science and scientism share an intellectual heritage, scientism has taken a turn toward an ideology that goes well beyond what science itself requires, and in directions that may not facilitate the open, exploratory, expansive nature of science. Scientism entails a zealous metaphysical commitment and a requisite orthodoxy in method and in thought regarding the nature of the world and how understanding of the world is to be approached. Thus, the title of this work speaks of scientism as the new orthodoxy.

Why scientism is not science and science is not scientism

It is important to keep in mind that criticism of scientism is not a criticism of science. Scientism, like science, certainly reflects Enlightenment thought, but its contemporary purveyors seem to have forgotten or ignored something many of their most illustrious intellectual forebears from the Enlightenment period understood. It is that even as they rejected and went beyond mere metaphysical speculation, they understood that they could not escape the questions that had been at the heart of metaphysics. Reductive materialists have tried to escape metaphysics in virtually every age; however, reductive materialism is a response to the metaphysical question and is thus equally a metaphysics. It does not do away with the metaphysical project.

The point is that scientism entails a metaphysical commitment to naturalist, reductive, or emergent materialism and tries to define science in a way that includes not only a commitment to empirical methods, but also to this particular metaphysics. Within scientism then, questions are framed in terms of this particular metaphysical perspective, and the propriety of any method is evaluated primarily in terms of whether it deals with the world as if it really is

as naturalist materialist metaphysics claims it is. Therefore, a scientistic science can only produce results compatible with or affirming of the same metaphysical commitment it started with, since the validity of the metaphysics itself is not a question that can be answered scientifically. This strategy is, however, not science. Within an intellectual regime of scientism, science is caught in a sort of Kuhnian (1962) paradigmatic stage with no revolutions imaginable. In critically assessing scientism then, it is important to understand what science really requires and what it does not require. This consideration, in one form or another, runs through the chapters in this volume. The nature of science and the foundations of truly scientific knowledge is no small topic. This volume is not a text in the philosophy of science but it does introduce and elaborate lines of analysis that can make clear the distinctions between science and scientism.

It would be noncontroversial to say that science assumes and requires that the world be rational; that is, that it can be made sense of by a rational mind. The world must be orderly rather than random. There is no reason, however, that science should require that the rationality and order derive from, or have their roots in, any particular sort of reality. Whether order inheres in the nature of the world and things within it, or order is a reflection of laws working on brute material, the fundamental empirical approach of science should work equally well. There is certainly no obvious reason to believe that the metaphysical commitments of any particular scientist, or of an entire school of them, should affect science as an enterprise of empirical investigation, or the conduct of science, so long as the scientists adhere to properly designated methodological standards, and they craft their methods to be appropriate to whatever they think the world is like. One might wonder, perhaps, just which research laboratories would have to close down, if it happened to be the case that Darwin got it wrong, or that there is a God. Metaphysics will affect what questions scientists choose to research. It will also affect how specific methodological procedures are crafted, and it will affect the theories scientists propose as explanations of what the methods of the science produce. However, it should not affect science as science and its epistemological project—discovering and reporting empirical facts. Furthermore, science will be most productive if the metaphysical commitments of the scientists actually conform to the nature of reality; however, science and its fundamental empirical approach are metaphysically impartial and uncommitted.

Science does not require that naturalist, materialist metaphysics be true in order to proceed in its project of gathering and reporting empirical findings. The fact that scientific methods are crafted to probe and pursue certain phenomena in ways appropriate to the presumed nature of the phenomena means that science will be more or less effective in answering questions about the world, more or less impressive in the results it produces, and more or less truthful in its explanations if it is operating under a set of metaphysical assumptions that really are true of the world. It is important to get the metaphysics right in order for the results of science to be reflective of truth, but science as science could operate perfectly well given any number of metaphysical truths. Aristotle (1987) (*Nicomachean Ethics* 1.1.20) reminds us that in our reasonings (i.e., science) we can achieve "such clearness as the subject matter admits; for it would be . . . wrong to expect the same degree of accuracy in all reasonings." However it is not the nature of the world and its phenomena that determine whether science is possible; nor does science require that the world have any particular nature. Paul Feyerabend (1995) reminds us that the rigid metaphysical requirements inherent to scientism are not required by science:

> Is science the best type of knowledge we possess, or is it just the most influential? This way of putting the question has by now become obsolete. Science is not one thing, it is many; it is not closed, but open to new approaches. Objections to novelty and to alternatives come from particular groups with vested interests, not from science as a whole. (p. 809)

Science, then, is not tied to or dependent upon any particular metaphysical reality. Scientism, on the other hand, is not just a straightforward confidence in a generic science; it is a metaphysical commitment. Bastiaan van Fraassen, in this volume (see also van Fraassen 2002), has described how thinkers committed to materialist metaphysics have tried, over the centuries, to subsume empiricism and, consequently, science as well. Contemporary scientism is the latest such attempt. To resist this hostile takeover is to defend science.

What is scientism?

While the term scientism is not widely used, even in scholarly discourse, there are a number of readily available definitions that, when taken together, convey

what is intended by the word. They also allow us to focus on the intellectual issues at the foundation of scientism that are of sufficient importance to inspire orthodoxy in some scholars and critique in others. Across various definitions, four principal aspects of scientism can be distilled. The various definitions differ in terms of which of these aspects of the construct they emphasize. All definitions make claims, and rest on assumptions, about the nature of science and about the nature of knowledge claims that can be made in the name of science. The definitions also uniformly, without explicitly acknowledging doing so, take science to be a particular, specifiable, epistemological endeavor, wedded to a particular supporting metaphysics, descended in a direct line from the period of the Enlightenment. The four common features of scientism can be described in terms of the following tenets.

1. It is a tenet of scientism that only certifiably scientific knowledge counts as real knowledge. All else is mere opinion or nonsense. Scientific knowledge is defined in terms of the method by which it is obtained. Scientism, therefore, must also hold that there is *a* certifiable and specifiable method (or manageable set of methods) that counts as scientific. More sophisticated purveyors of scientism will allow that there is not a single scientific method, but, nevertheless, there will be a common core at the heart of all surface-level methods that constitute science. It should be noted here that underlying this claim about "scientific method" there is another assumption—that the world just is (metaphysically) of a nature that yields itself to such methods and not to others, thus making scientific methods not only legitimate, but necessary for real knowledge.

2. If the first tenet holds, then a second tenet follows unproblematically. It is that the methods and assumptions underlying the natural sciences, including epistemological and metaphysical doctrines, are appropriate for all sciences, including, prominently, the social and human sciences. A corollary doctrine is that the arts, if they seek to be more than myth and self-expression, must somehow be brought under the umbrella of science. If they cannot be brought under the umbrella of science, perhaps a rapprochement can be achieved, the terms of which shall be dictated by science (or, more precisely, by scientism).

3. This attitude toward the arts and softer sciences illuminates a third tenet of scientism. Scientism exudes and promotes an exaggerated confidence in science (i.e., natural science in all its avatars) to produce knowledge and solve

the problems facing humanity. Such confidence would be reasonable only if the world, in every aspect of reality, were of a nature that lends itself to study by a science conceived and constructed as our current natural science is, and that yields knowledge to such methods as we have concocted for our current natural sciences to use. In this confidence, we are again made aware that scientism makes assumptions about the nature of reality that are metaphysical in nature.

4. The fourth tenet of scientism articulates the heretofore implicit assumption underlying the other three. Scientism is not just an attitude about science and the power of science. Scientism makes metaphysical claims. Sometimes hidden, and sometimes not, among the claims of scientism is that the world must really be like the methods of contemporary natural science assume it to be. Otherwise, claims of the legitimacy and power of science would be without grounds. Just as the science of scientism has come through intellectual history fairly intact from the Enlightenment period, so has the metaphysics of scientism. Scientism assumes, and requires a naturalist, materialist, rather mechanistic metaphysics. Such a metaphysics is so important to scientism that it has become a core feature of the orthodoxy of scientism. Commitment to this metaphysics is preeminent among the ideas that give energy to defenders and promulgators of scientism.

It is scientism's insistence on naturalist, materialist, metaphysical orthodoxy that is of most concern to many of its thoughtful critics. Overconfidence in science might be dismissed as largely harmless youthful exuberance. Dismissal of the arts and humanities and what they reveal about us might be tolerated as narrow minded, but understandable on the part of scientifically minded scholars. Prescribing for the social sciences the methods and assumptions of the natural sciences might even be seen as a historical recurrence of the ill-fated overconfidence of the logical positivists, one which must merely be outlived—again. The intellectual opportunity costs of hammering away at problems in the human sciences with methods not developed or well adapted to deal with them might, of themselves, be sufficient to question and even reject scientism in the social sciences. However, the metaphysical claims on which scientism is based have significant consequences that reach beyond those just mentioned. When this naturalist, materialist, mechanical metaphysics that has been somewhat pragmatically useful in the natural sciences, and that seems relatively

compatible with the natural world, is applied to the human world, the result is a fundamental alteration of our understanding of our nature and ourselves. The very meaning of what it is to be a human being, our lives and our actions, the possibility of morality, and the essence of our human relatedness, are all called into question. This is because under the intellectual regime of scientism, to understand these fundamental aspects of our humanity is to explain them in terms of things outside of them, and to explain them in that way is to destroy them. This problematic tension between reductive naturalistic explanation and the understanding and preservation of our humanity is probably most clearly visible in the interface of science (as scientism understands it) and religion. However, the tension is present throughout contemporary culture.

Of concern to many in various intellectual traditions is the fact that the metaphysics of scientism is being enforced in cultural and intellectual discourse by purveyors often lacking adequate training in metaphysics or even in the philosophy of science. This is surely true within the social sciences and the humanities, and even in the hard sciences. Most scientists, as scientists, are not trained in metaphysics—they simply inherit it from the intellectual culture in which they learn and practice their craft. Thus, as scientism invades and co-opts intellectual and cultural discourse, as, for example, in the popular rhetoric of the so-called new atheists, it brings with it sets of assumptions and implications that are portentous in terms of their effect on not only our scientific, but also our moral, political, and aspirational discourse. The effect of scientism is the same as what Daniel Dennett (1995, p. 63) described as the effect of Darwinism—as a universal (intellectual) acid that eats through everything, leaving everything somewhat recognizable, but fundamentally changed. What is at stake as scientism becomes the new orthodoxy is our humanity itself.

The Enlightenment roots of scientism

There is no question regarding the philosophical and methodological roots of modern science. It is the product of that period of intellectual history known as the Enlightenment. For some scholars, the Enlightenment consisted essentially of the rise of science and the demise of speculative metaphysics. This attitude

can tend to portray the history of ideas, and consequently, of culture, as on a smooth upward trajectory impeded only by reactionary ideas and reactionary persons who either cling to older metaphysical systems, and cosmologies, or who have doubts about the power of rationality and empirical science to uncover the true nature of the world and how it works. This progressive enthusiasm is a key characteristic of scientism. For those riding the crest of this presumed wave of progress, even if no one particular science is complete, science itself is—or will be. Other scholars understand our intellectual history and the current state of the world of ideas to be more complex and nuanced. This moderated enthusiasm comes not only from what have come to be known as "postmodern" perspectives, but is also based on careful and rigorous understanding of the roots of what are taken to be "enlightenment ideas," and the assumptions and implications in which they are imbedded. While scientism and its enthusiasts regard science as the next, perhaps final, progressive step, away from speculative metaphysics and epistemology to simple factual truth, other scholars recognize that science cannot replace metaphysics or epistemology. While science may bring to light facts and phenomena, it will always be the case that these must be interpretively understood and accounted for. That is, facts, even scientific ones, always require interpretation. There will always be a metaphysical project accompanying a scientific one. Thus, there is constant need for good metaphysics and sound epistemology.

In his excellent history of the Enlightenment, Louis Dupré (2004, pp. 7–8) summarizes two widely recognized concepts underlying the revolutionary thought of the period. The first was "rationalism" in two senses: a confidence in *a priori* processes of the mind as essential to knowledge and understanding, and a more radical sense that the mind was the sole source of truth and, thus, of authority in matters of knowledge. The second concept was "emancipation," by which was meant freeing humankind from the consequences of believing in anything other than what can be derived from reason. In fact, as Dupré (2004, p. 3) suggests, the modern turn in philosophy involved an understanding that reality was no longer intrinsically intelligible, by its nature; rather, intelligibility was a product of the human mind and imposed on the world. This philosophical turn contributed to a view of the universe as mechanical and impersonal. While such a universe was orderly, and the order was imposed or captured by the mind, there was no higher order, purpose, or reality, reflected in that order.

This understanding of reality constituted the break of philosophy and science from the classical and scholastic intellectual traditions.

Of course, a fundamental change in our understanding of the nature of reason necessarily entails a fundamental change in our understanding of the "reasoner," that is, the human being. Dupré (2004) explains:

> Two consequences followed from the transformation of reason. One, the subject, now sole source of meaning, lost all objective content of its own and became a mere instrument for endowing an equally empty nature with a rational structure. Two, since reality thereby lost the inherent intelligibility it had possessed for the ancients and the Schoolmen, the nature of *theoria* fundamentally changed. Thinking ceased to consist of perceiving the nature of the real. It came to consist of forcing reality to answer the subject's question. . . . Contemplation . . . became an instrument in the hands of, and for the benefit of, an all-powerful subject. (p. 17)

More particularly relevant for our understanding of the development of science and scientism, Dupré (2004, p. 16) observes that Enlightenment thinkers

> imposed [on reality] the rules of the one science that the mind could indeed claim full authorship of and which depended on no external content, namely, mathematics. The mind thereby acquired an unprecedented control over nature, yet it ceased to be an integral part of it. (p. 16)

Control over nature as the principal legitimating goal of science and reason was a chief impetus for the turn toward the empiricism that informed the work of Francis Bacon and others, and has been at the heart of scientific method, and, in some form, at the heart of the epistemological commitment of most scientists since.

Scientism, as it has emerged, and as it runs through contemporary thought, reflects commitment to a set of constructs and ideas with roots in Enlightenment thought. Not all of these, of course, will be found in every scientistic expression or sentiment. Scientism is, in its basic form, a dogmatic overconfidence in science and "scientific" knowledge. But, more importantly it is overconfidence in science, defined by, constructed around, and requiring that, the world must be made up of physical matter following particular lawful principles, and that all phenomena are essentially thus constituted. This gives

scholars the great confidence that characterizes scientism. The confidence associated with this worldview is seen in the insistence that any scholarly endeavor that does not ground itself in that required set of constructs and ideas must be rejected as unscientific, and any knowledge claims made as a result of such endeavors are suspect. Such knowledge claims are to be rejected as being only metaphysical speculation, reflecting mere subjective bias, or, ironically, a devotion to religious orthodoxy.

The set of ideas that characterizes scientism (as opposed to science *qua* science) is fairly easy to specify and to identify as having roots in Enlightenment thinking. The chapters in the volume deal in detail with these ideas, how they function in scientistic thinking, and how the ideas and issues that surround them can be otherwise understood. Importantly, the chapters suggest how science can be separated from scientism and how the antagonism that scientism sometimes generates toward other ways of thinking can, and should, be overcome. In this introduction the ideas will be summarized very briefly.

Scientism entails acceptance of a particular, quite literal, reading of what has come to be called "Cartesian dualism," meaning that the world is assumed by scientism to consist of two realms, the powerful mind that produces knowledge by the processes of the rational intellect and the realm of extended matter, that is, physical material.

Scientism entails acceptance, therefore, of the proposition that there is no rational order *inherent in* the world itself, that is as part of its nature and status within a larger meaningful whole. There is an order to things and events, but an order governed by the external laws that determine the behavior of matter from the outside and/or reflect how the rational mind works. For scientism, this lawful order is manifest in cause–effect relationships and in particular types of "hard" determinism.

Knowledge, as pursued within scientism, does not reflect harmony with the order inherent in the world. Understanding is not of the order present in the nature of things, but rather of the laws that operate on things. Knowledge offers not harmony and enlightenment, but explanation and power.

Scientism holds that, absent a fundamental order inherent in the world, there is likewise no *telos* in the world. This means that for all things and events, causality must be understood as imposition or impetus coming from outside,

not from within. In the human world, commitment to this principle obviates freedom of the will and any meaningful human agency. In the human as well as the nonhuman world, causality is only understood as a particular kind of "efficient cause," imposed from outside.

The fundamental example of this sort of efficient causality is to be found in mechanism. Scientism thus entails a commitment to mechanism. Most technology consists of, or relies upon, sophisticated mechanism. Because of this commitment to mechanism, scientism tends to view all problems as, in some sense, "technological" problems. They result from lawful malfunction and are to be dealt with by interventions into the mechanism. When mechanistic, technological models are applied to human behavior, people become not actors, but stages upon which or within which actions occur.

For scientism with its intellectual commitment to a world in which only minds and matter, and perhaps "laws," can be considered real, there is no need, or, perhaps even no sense, in positing the existence of anything other than matter. Matter operates according to laws, and mind imposes meaning on such operations. Thus, materialism is the metaphysics of scientism, and metaphysics is really just physics. The materialist reduction will be complete when the mind itself can be definitively shown to be just operations of the matter of the brain.

Scientism rejects most meaningful kinds of transcendence. If materialism captures the essence of reality in terms of matter and law, and physics is complete, then there is no need for, or indeed no place for, transcendence. The only transcendence is found in the limited creative operations of the rational mind, and mind itself is emergent from matter and mired in material circumstance. It follows from this that, within a scientistic intellectual regime, there are no transcendent ethical realities or principles. Moral realism and deontological ethics are without substance or foundation. The most sophisticated ethical systems that can be formulated or justified will be derived entirely from rational considerations and grounded in utilitarian concerns and social contracts.

It must be acknowledged that the foregoing is not an exhaustive roster of intellectual commitments constitutive of scientism. It does, however, lay out some intellectual and moral implications of some of the commitments, which often form the basis for scholarly critiques of scientism. Some scholars critique

scientism because it is bad science and bad philosophy, while others reject it because it is bad anthropology and bad moral theory. Still others criticize it because it is bad for us as the kind of beings we are, and bad because it cannot provide a foundation for a good and flourishing life for the kind of beings we are—morally or politically. As the chapters in this volume illustrate, critiques of scientism come from a wide array of intellectual perspectives, although there is a common core of intellectual and moral concerns.

The presumptions of scientism

A book written to critically examine an intellectual position like scientism is, in a sense, an unusual endeavor. Scientism is not like other intellectual positions that, in some cases, end up as schools with largely consensual tenets and propositions, and at least a roughly identifiable set of adherents who will allow themselves to be referred to as such. In the case of scientism, those who might be identified as adherents, most often repudiate the term, and, in many ways, deny there is such a thing as "scientism." For them, the very idea of scientism is merely an uninformed and unfounded criticism of the intellectual project of science itself. So, the critique of scientism articulated in this volume is aimed at a way of thinking, a set of problematic assumptions, while those who may think that way or make those assumptions may reject the very concept of scientism. Nonetheless, the issues articulated here are very important ones to raise with regard to an intellectual position that is emerging and has become for many, the new orthodoxy.

An article by Professor Steven Pinker (2013) serves as an example of how an adherent and popularizer of scientism rejects the very term "scientism" as it is used by critics, sloughs off the criticisms leveled against scientism, and then invokes the very kind of thinking that has prompted the criticisms. Pinker first claims all great thinkers as scientists, then assures his readers that science simply pursues intelligibility and not reductionism, and concludes that science, as he describes it, provides the best foundation for belief, morality, and essentially all human endeavors. Pinker's article also, however, embodies the presumptions and intellectual commitments that have led thoughtful people to identify and critique scientism in the first place. He begins by claiming Rene

Descartes, Baruch Spinoza, Thomas Hobbes, John Locke, David Hume, Jean-Jacques Rousseau, Gottfried Wilhelm von Leibniz, Immanuel Kant, and Adam Smith as fellow scientists. A thoughtful reader must wonder what definition of "scientist" is so broad as to meaningfully capture such a diverse array of great thinkers. This claim is clearly a linguistic maneuver, a strategy of arguing by seizing the definition of the term "science," as proponents of scientism are wont to do. Thus, Pinker claims by definition that science engulfs all good and careful thinking, implying thereby that there really are no important intellectual projects outside the province of science. This is, however, a useless definition. If science really is all good thought, then science is not distinguishable from any other worthwhile intellectual endeavor, and it contrasts only with that which is deemed nonsense. But these are precisely the questions that are at issue. It leaves unanswered how one is to decide what is nonsense. If Pinker answers, as I am sure he would, that one decides by scientific method, then the circular argument reaches closure.

Pinker clearly does, however, have in mind a more particular understanding of science. On the other hand, a critic of scientism might point out that if all scientists were as educated, skilled, and insightful in metaphysics, moral philosophy, the philosophy of mind, mathematics, aesthetics, and epistemology as were these eminent "scientists" Pinker mentions, then there would quite possibly be no scientism of the sort that has caused the type of deep concerns that scientism has engendered. It is apparent by Pinker's own writing, however, that not all contemporary scientists rise to the level of those mentioned earlier.

In dismissing the contemporary concerns that have arisen about scientism, Pinker (2013) provides a paradigmatic example of those very concerns. Of science he writes:

> To begin with, the findings of science entail that the belief systems of all the world's traditional religions and cultures—their theories of the origins of life, humans, and societies—are factually mistaken. We *know*, but our ancestors did not, that humans belong to a single species of African primate that developed agriculture, government, and writing late in its history. We *know* that our species is a tiny twig of a genealogical tree that embraces all living things and that emerged from prebiotic chemicals almost four billion years ago. . . . We *know* that the laws governing the physical world (including accidents, disease, and other misfortunes) have no goals that pertain to

> human well-being. There is no such thing as fate, providence, karma, spells, curses, augury, divine retribution, or answered prayers—though the discrepancy between the laws of probability and the workings of cognition may explain why people believe there are. (para. 15, italics added)

Pinker (2013) goes on to report that "the worldview that guides the moral and spiritual values of an educated person today is the worldview given us by science" (para. 16). To this Pinker adds:

> The facts of science, by exposing the absence of purpose in the laws governing the universe, force us to take responsibility for the welfare of ourselves, our species and our planet. For the same reason, they undercut any moral or political system based on mystical forces, quests, destinies, dialectics, struggles, or messianic ages. (para. 16)

The foundation of our morality in this scientistic age is that "we are social beings who impinge on each other and can negotiate codes of conduct . . . that maximize the flourishing of humans and other sentient beings" (para. 16).

That Pinker's conclusions about what we know might not be derivable from science itself is suggested by how closely his conclusions about the origin, nature, destiny, and meaninglessness of human life follow those reached by Bertrand Russell as early as 1903 in his proclamation, *The Free Man's Worship*. Professor Russell, notably absent from Pinker's list of scientists, did not have the benefit of a century of scientific progress in reaching his conclusions about the nature of the world including all humanity. A skeptic begins to suspect at this point that Pinker's declarations, like Russell's, are not really scientific, but conjectural and reflect a kind of intellectual orthodoxy. They arise from shared metaphysical, epistemological, and moral commitments and are not the necessary conclusions of scientific endeavor. In fact, as an oft-quoted statement from Richard Lewontin (1997) suggests:

> It is not that the methods and institution of science somehow compel us to accept a material explanation of the phenomenal world, but, on the contrary, that we are forced by our *a priori* adherence to material causes to create an apparatus of investigation and a set of concepts that produce material explanations, no matter how counter-intuitive, no matter how mystifying to the uninitiated. Moreover, that materialism is absolute, for we cannot allow a Divine Foot in the door. (para. 31)

Summary

The need for a critical examination of scientism is articulated by philosopher and author, Marilynne Robinson (2010) when she writes of "parascientific literature," which she describes as

> a robust, and surprisingly conventional, genre of social or political theory or anthropology that makes its case by proceeding, using the science of its moment, from a genesis of human nature in primordial life to a set of general conclusions about what our nature is and must be, together with the ethical, political, economic and/or philosophic implications to be drawn from these conclusions. . . . One of the characterizing traits of this large and burgeoning literature is its confidence that science has given us knowledge sufficient to allow us to answer certain essential questions about the nature of reality, if only by dismissing them. (pp. 32–33)

What is at stake in the tension between scientism and the science it seeks to dominate, as well as the understandings it seeks to replace, is our very humanity—the nature of our existence, the meaning of our lives, and our understanding of the best to which we can aspire. Noted philosopher Thomas Nagel (2012) points to the intellectual path that must be followed if we are to benefit from science in understanding the world and our place in it. Commenting on "the implausibility of the reductive program" that is a key assumption of scientism, Nagel notes: "If we want to try to understand the world as a whole, we must start with an adequate range of data, and those data must include the evident facts about ourselves" (p. 20). Rather than overwriting the data available to us by and through our very humanity, we need to take account of what our own lived experience as the kind of beings we are tells us about life and the world.

Objections by many scholars notwithstanding, there is a discernible and identifiable family of intellectual positions having the clear and unmistakable characteristics that may be referred to as scientism. The constellation of ideas, assumptions, and conclusions are well known and easily identified. Furthermore, as Pinker has noted, this constellation of ideas has, to a considerable degree, and perhaps to an alarming degree, become "the worldview that guides the moral and spiritual values of an educated person today." In other words, it has become the New Orthodoxy. One might view

any concern about scientism to be just an intellectual disagreement, perhaps the latest phase of confrontation of materialist naturalism and those who find it unconvincing. There is, however, something new in the current ascent of scientism that lends greater significance to the discussion. Scientism seeks to claim authority, not only from its philosophical arguments, but it also seeks to lay claim uniquely to the authority of science as validation of its metaphysics. In so doing, opposing views are cast not only as mistaken, but as making no sense, since scientific knowledge is held to be the only real knowledge that can make sense. This renders much of what human beings think about themselves and much that they believe, meaningless because nonsensible. All of this has the potential to affect our view of our own human nature, such that we see ourselves as mere natural organisms. The extension of the influence of scientism into the human sciences, the humanities, and even religion, calls into question all transcendence along with trans-situational values and morality generally, offering in return only a severely impoverished, strictly utilitarian, moral landscape. In light of these implications, scientism as the new orthodoxy inevitably raises intellectual concern and scholarly discourse to a level higher than a mere intellectual squabble. The chapters in this volume do an important work in unpacking and disarming this new orthodoxy by bringing to bear unimpeachable scholarship of the highest standards.

The chapters in this volume

Daniel Robinson's chapter deals with the important question of explanation. He makes clear that explanation, even causal explanation, is necessarily grounded in human experience and reason. It is scientism, not science, that demands a particular type of explanatory account grounded in physical, material, and generalizable laws. The insistence on this type of explanation comes from orthodoxy rather than from experience or the tenets of science itself. Explanation always requires a starting point that scientism cannot provide and offers a type of understanding that scientism cannot accommodate.

In his chapter entitled "Scientism and the Religion of Science," Lawrence Principe provides an excellent history of the rise and development of

science and scientism. The analogy of scientism and religion is both apt and informative. Principe describes the purpose of the public proponents of contemporary scientism as to "preach a political, philosophical, and religious agenda that cannot be supported by either their expertise or the methods they claim that science follows." He suggests that what is taken to be the traditional, although never clearly defined, conflict between science and religion is better characterized as conflict between scientism and religion, and in this context, it is more obvious how scientism has become a religion to many. When properly understood, much of the supposed conflict between science and religion is mostly myth. Today the myth is kept alive and promulgated with the greatest dedication by scientism. Principe reminds us that before the late nineteenth century there were not two distinct camps of scientists and religionists; before that time there was a single intellectual enterprise, pursued by people who were mostly religious while engaged in scientific investigation. The best prescription to deal with tensions related to the scientism–religion conflict involves strong doses of humility and wonder.

The chapter by Bastiaan van Fraassen traces the history of the relationship between naturalism and empiricism, making clear where they have common ground. He stresses that, properly understood, naturalism and empiricism are to be understood as a "stance" toward the world that informs investigation. One of the important shared perspectives is antifoundationalism. Scientism takes naturalism and empiricism both from this appropriate context and elevates naturalism to the status of metaphysics and empiricism to a method for doing what philosophy should do. Science, from the point of view of scientism, consists not only of empirical methods, but the content that those scientific methods have produced. Van Fraassen characterizes the tension between two competing views of science active today as whether the aim is truth or "empirical adequacy." Scientism goes wrong in part because of its "fundamentalist" advocacy of the former and rejection of the latter and the kind of interpretive project it finds at the center of science.

Peter Hacker concentrates on the question of whether cognitive neuroscience is capable of validating the reductive materialist claims of scientism about the origins of consciousness and mental life. He makes the distinction between claims that are simply false and might be overcome by scientific method, and the claims of scientism that are "intellectual illusions and conceptual confusions,"

and cannot be overcome by recourse to data. He demonstrates in some detail that there are questions that neuroscience experiments cannot explain but for which we do know the answers. Such is the case with most human questions that are historically meaningful and reflective of consciousness. The proper strategy in response to the confused explanations of scientism is not to show that they are wrong, but rather that they are confused and incoherent. In this vein, Hacker argues carefully that contemporary neuroscience is equipped to study and understand the brain and physiological pathologies, but that it is not equipped to validate physiological origins of thought and consciousness; nor, indeed, is there any coherent reason to attempt it. It is incoherent to claim that the "brain is the subject of psychological attributes." It is the hubris of scientism that motivates the claim in the first place.

Richard Swinburne's chapter extends the analysis of explanation into the issue of physical determinism in great analytical detail. He points out that physical events, if they are accessible by any observers at all, are, by their nature, public events, to which all investigators have equal access. In contrast, mental events are events to which there is always privileged access by the person whose mental event it is that provides understanding other observers will always lack. We all have privileged access to our own intentions. Any physical explanation, such as scientism requires, must ignore that privileged access and regard personal mental phenomena as epiphenomenal. Swinburne provides a critique of the interpretations of the famous experiments by Benjamin Libet (2004), purported to prove that brain events produce mental intentions, arguing carefully that no experiment can ever establish such epiphenomenalism. One of the fundamental tenets of scientism is unprovable by science. He extends this line of argument to conclude that physical determinism is implausible.

Roger Scruton traces the history of the division of the scholarly landscape into the sciences and the humanities, and the more recent crisis in the humanities that derived in large part from that division. Central to the division and to the crisis are issues of method, and the related contrast in the different kinds of knowledge claims that can be made. Some in the humane disciplines have responded to this problem by "assimilating their subject matter to one or other of the sciences." More recently, evolutionary psychology and neuroscience have invaded—or been invited into—the humanities. Scruton uses this historical phenomenon as a stage upon which to clarify the distinction between

science and scientism, as well as the humanities. Central to the discussion of science, scientism, and the humanities is the core role that meaning plays in understanding specifically what it is to be a human being.

Kenneth Schaffner's chapter provides a thoughtful and current introduction to the contemporary field of neuroscience and some of the implications of present-day knowledge for issues such as the free-will debate. He also introduces the emerging field of neuroethics and gives a good summary of how and where neuroscience can make its most substantial contributions. His chapter provides examples of the proper purview of neuroscience and the directions it can and should take when it is not pressed into service by the constraints inherent in the orthodoxy of scientism.

James K. A. Smith, in his chapter on the dialogue between science and religion, argues that the playing field is not level because of a category mistake. Even those who defend religion have accepted and adopted a view of science that is really scientism instead. It is scientism, not science, that fuels the tension, and stakes the intellectual ground so that religionists are always "fighting uphill." Smith's response, aimed at leveling the playing field, is to take science out of the domain of scientism by emphasizing its nature as a human, cultural activity, rather than the constrained, reductive activity that is science from the perspective of scientism. Part of this redefinition of science will be a proper emphasis on the practice of science rather than on the contents of sciences that tend to get filtered through the metaphysics of scientism. Smith concludes on a hopeful note, with reason to believe that scientism is ultimately not viable.

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1

Science, Scientism, and Explanation

Daniel N. Robinson

> *With faith man steps forth into the world. Faith is far ahead of understanding and knowledge; for to understand anything, I must first of all believe something. Faith is the higher basis on which weak understanding rears its first columns of proof; reason is nothing but faith analysed.*
>
> Franz Schubert (as cited in Frost 1885, p. 63)

A persistent question raised by schoolchildren and by seasoned scholars asks simply and directly, What is science? Any number of activities and achievements are readily recognized as scientific, but one is hard pressed to identify some shared feature that, when present, renders the adjective apt and informing. One might be tempted to regard measurement as the relevant feature. However, various schemes of classification and identification in the sciences do not depend on quantitative modes of measurement. Similarly, many endeavors that are clearly scientific in nature are not *experimental.* Astronomy, for example, is a developed but not an experimental science. One is on no firmer ground by taking recourse to consensus. In their own time astrology, alchemy, phrenology, palmistry, and physiognomy were widely regarded as scientific.

The topic of explanation is as vast as the domain in which objects and events seem to call for one. Members of a jury are asked to judge whether a defendant had both opportunity and motive. One cannot judge the defendant as possibly guilty if, in fact, he had no conceivable way of finding an opportunity to commit the crime and if, in fact, the victim was a total stranger. Passengers expect that excessive delays will be accounted for, and students must find an excuse

more credible than the dog having eaten their homework. Political leaders, especially during election years, are supposed to be able to make clear why various initiatives have failed and why the more promising ones were never tried. On a larger scale, historians seek to account for the French Revolution or the fall of the Roman Empire. Scientists must greet incredulity as they offer evidence that the universe is or seems to be expanding. At what might be called the metaphysical level, various experts bear the burden of establishing just how the jumble of physical events in a human brain somehow constitute the conditions under which oaths become binding and affections withheld.

In these and myriad other contexts, what are sought are *explanations*, but it is not clear just what we have when they are found or, for that matter, just how we know that we found one. In scientific contexts, there is general but certainly not total agreement that an object or event has been explained when its cause has been identified. Of course, this simply raises the same question, now in somewhat different terms: How do we explain the fact that some object, event, or state of affairs reliably results in some different object, event, or state of affairs? The cock crows after a clock on a mantle 30 miles away strikes six. It would be nonsense to say that the clock's chimes caused the cock to crow.

We speak of causal relations as being distinct from coincidences and mere correlations, distinct even from what we take to be necessary conditions, but this, too, is problematic. When Isaac Newton paused to consider gravitational effects, he was clear in distinguishing between knowledge of the effects of gravity and knowledge of just how gravitational forces *cause* the effects. The distinction is important. A scientific law sets down the interrelationship among different properties of physical bodies. In classical mechanics, it is well established that the force required to impart an acceleration to a body depends up on the mass of the body, with the familiar equation being $F = MA$. We can say that the equation stands as something of a rule, but it certainly does not explain *why* the rule is of this form rather than, say, $F = \log_2(MA)$.

Sometimes, in searching for explanations, we are content to identify factors that are reliably associated with certain events. Not much by way of conjecture is needed to account for persons having umbrellas on days calling for rain. However, the more committed one is to a complete explanation, the more complicated the story would become.

In light of the broad and diverse range of contexts, the impulses behind attempts at explanation are themselves various and often highly individuated. The explanation Jack seeks to account for Jill also going up the hill may be a matter of total indifference to Jeff who had his own pail of water. Granting variety and individuation, one source of the impulse to explain is a state of *wonder*; a state of perplexity or confusion promising to be relieved by reaching an understanding, itself the result of a successful explanation. Consider young Newton, pondering the mysteries of astrology while shaded by one of his grandparents' apple trees. Down falls an apple on that head enclosing so fertile a brain. What might Newton have wondered? Alas, "With all those apples falling, why, pray tell, does not the moon?"

There is a typical series of steps launched by a state of wonder; a series that begins with confusion or perplexity soon includes a more careful description, and then daringly or tentatively through various hypotheses and toward a satisfactory explanation. In this way the world becomes less strange, more predictable. Once we can explain the occurrence of "X," there is the reasonable expectation that future events of type X can now be anticipated and dealt with more effectively. Were this not the case, every nuance at the level of experience would require yet another explanation.

Various accounts of scientific explanation have been offered. One of the most influential was advanced by Carl Hempel: the *deductive–nomological* model of explanation (nomological from the Greek word *nomos* for "law"). On Hempel's account, science is virtually identified by its mode of explanation. A scientific explanation, unlike other forms of explanation, avails itself of general or universal laws known to be true. The process begins with an event calling for an explanation. This is typically in the form of an *explanandum* statement (e.g., "The apple fell"). To account for or, as it were, explain the *explanandum*, an *explanans* statement is advanced, typically in the form of a law (e.g., the universal law of gravitation), which, if true, permits one to *deduce* the *explanandum*. Hence, the deductive–nomological model of explanation.[1]

Little of the process is as cut-and-dry as textbooks tend to suggest. Although the deductive–nomological model is a useful sketch of what is common across a number of scientific explanations, it is porous to counterexamples. Suppose as a general law it is asserted that *Men who drink mango juice never*

have xx-chromosome pairs. Suppose further that this explains why it is that Tom, who consumes mango juice daily, has only xy-chromosome pairs.

As would be expected, Hempel's account has been subjected to many such critical appraisals over a now long course of time and no longer enjoys the dominant position it once held. Some of the criticism is based on what are clearly the counterintuitive consequences of applying the model across the board. Consider yet another example: The explanandum statement, "John Doe has two legs." Now the explanans statement: "All chickens have two legs." Finally, note this fact of direct experience: "John Doe has as many legs as does the chicken standing near the barn." On Hempel's account, if we wish to explain John Doe as having two legs, the Hempelian model provides a general law to the effect that all chickens have two legs. Then, there is the empirical fact that John has the same number of legs as the chicken standing near the hen house. However, it would be nothing short of ridiculous to suggest that the right explanation of John Doe having two legs is by way of a deduction from a general law pertaining to the anatomy of chickens! There is simply no coherent or rational connection between the fact needing to be explained and the covering law from which that fact is deduced.

One begins to see that there are whole domains of reliable observations accounted for in a reasonable and practically useful fashion but without the benefit of universal laws or the Hempelian framework. The model advanced by Hempel leads to entirely implausible explanations unless weighty presuppositions are already established. This will be even clearer later in the chapter when considering scientific approaches to explanation in history.

Presuppositions are the rule. The search for explanations begins with events or objects for which an account is needed, but events and objects are chosen, not simply given. The process finds us attending to specific features, more or less disregarding others. Objects and events are seldom static, if only because observers themselves are not static. The famous maxim according to which no one enters the same river twice refers not merely to the ever-changing flow of the river but the never-static condition of the person. This raises interesting questions. Albert Einstein claimed on more than one occasion that his progress toward the General Theory of Relativity owed substantial debts to David Hume and Ernst Mach.[2] According to Einstein, what they had made clear to him is that concepts are meaningless unless tied to an actual experience, and that our

concepts arise from our experiences or have no real meaning. "Experience," however, is itself a term that eludes simple definition. To have an experience is to experience *something*. It is to transform what is otherwise a clutter of sensations into a coherent entity now recognized as a type or kind of thing. To appreciate this is to abandon that philosophical fiction, the *tabula rasa*.

There has never been a living human being who entered or lived in the world as a "blank sheet." Immanuel Kant, more than anyone earlier, recognized that our cognitive powers must be assumed if we are to account for the fact that an otherwise indifferent constellation of physical impingements becomes organized into a recognizable "something." All may begin at the level of a bald sensation, but nothing reaches the level of an *experience* until these impingements are absorbed into a categorical framework. Kant makes the point in a much-cited passage in his *Critique of Pure Reason*: "Thoughts without content are empty, intuitions without concepts are blind" (Kant A51/B76).)

To think requires content, of course, but the mere engagement of sensory organs can amount to nothing, in the literal sense of *no thing*. One would see *no thing* and would, therefore, be essentially blind. It is the conceptual framework that raises a bundle of sensations to the level of experience. On this understanding, the meaningfulness that Einstein would tie to experience is actually a reflection of the ordering principles of the understanding itself.

As noted, the processes leading to explanation require that we select an object or event from a large and shifting pool of candidate objects and events. Identifying something in the environment includes both passive and active *selection*. The human senses are responsive only to some, not all physical impingements. We hear sounds in the range of frequencies extending from about 20 to about 20,000 hertz. We see light whose wavelengths are longer than about 380 millimicrons and shorter than 760. Thus, even as passive witnesses, we have access only to a defined portion of physical reality. What falls beyond this will be as nothing at the level of experience. Even more pronounced is the active form of selection; the unequal apportionment of attention reflecting what, for the given observer, is important, standing as an interest, carrying special value, bound up with other and highly individuated features of a given life. One hears one's own name in a noisy room, but not that of others. One finds the face of a loved one in a crowd even as all other faces merge into a homogeneous background.

Years ago, the psychologist Daniel Berlyne, whose writing on this subject still repays close attention, distinguished between *perceptual curiosity* and *epistemic curiosity* (Berlyne 1954, p. 180). With human beings, it is epistemic curiosity that is itself so curious. As Berlyne observed, "many of the queries that inspire the most persistent searches for answers and the greatest distress when answers are not forthcoming are of no manifest practical value or urgency" (Berlyne 1954, p. 180). True, there are great gains promised by bold adventure, but few sea captains are more devoted to a search for the elusive than is the hunched figure in the café now attempting to complete the day's crossword puzzle. The exploration I refer to, then, is of the sort Berlyne dubbed *epistemic* to distinguish it from an attempt to overcome the monotony of a perceptually static environment.

For an object or event to have saliency, the observer must already be tuned to perceive it; he must also have a framework or set of working assumptions with which to classify it; and, owing to the individuated nature of things, it must be something in which the candidate-observer has some investment of interest. To say, then, that a concept is only meaningful when it arises from experience is, in a way, to put the cart before the horse. As noted, one might say instead with Kant that, for there to be experience as such, sensations must be given organization and structure.[3]

We begin to see that the ingredients of an explanation are complex and that it is less than clear as to what makes some explanations better than others. Indeed, what, after all, do we have when we have an *explanation*, and what is it about any declarative sentence—or an entire book of such sentences—that stands as an explanation? Are all explanatory principles drawn from a common stock, or do events of a certain kind call for explanations right for that type but not for other types? Is the best model of explanation that adopted in the developed sciences and, if so, how is that model to be characterized? The theme connecting all chapters in this book is *scientism*. Perhaps it is past time to indicate how that term itself might best be understood. In my judgment, the most economical summary of the *-ism* just is an affirmative reply to this last question: *The best model of explanation is that found in the developed sciences.*

I refer to a model. I might have used the more technical term, *algorithm*, to refer to a series of steps or rules that culminate in the solution to a problem. Consider a dictionary as a *translation algorithm*. One enters a German word

and out comes the corresponding English word. I submit that scientism is the conviction that every feature of reality can be recast in the language of the developed sciences such that every feature of reality is, in principle, explicable in scientific terms. In the manner of an algorithm, it is by way of the actual and promised *laws of science* that objects and events at the level of conscious experience are translated into events that are without exception *physical.* At the most basic level of reality, *physics is complete*.

The claim that physics is complete is sometimes expressed as the principle of *causal closure*. At the bottom, the thesis finds reality exhausted by physicality. There are no real objects or events that are in principle nonphysical. Defenders of the thesis are under no illusion that at some distant time history, literature and the arts—let alone law, politics, and human relationships—will be explained by way of the elementary laws of physics. It is in the nature of human nature that it must deal with its own humanity in humanistic terms. However, nothing is conceded here at the level of ontology. The consistent physicalist requires of anything claiming real existence that it be physical. Matters of understanding and explanation may of necessity call for more, but ontology does not.

Surely that nodal point toward which physicalism and humanism are most confrontational is in what are now referred to as the brain sciences. Events in the nervous system are now readily examined in real time and at increasingly detailed levels of analysis. Knowledge of the functional anatomy of the nervous system grows by leaps and bounds. There is growing confidence among scientists that some of the most vexing aspects of human nature can be and perhaps soon will be understood at the level of neurochemistry and computer science. As with the thesis according to which physics is complete, so also is much claimed by the thesis that cognitive neuroscience is complete! There may always be good reasons to retain the perspective and the idiom of the humanities, as long as one recognizes that such habits are finely grounded in the functions of the nervous system. On this understanding, every feature of reality in which human life might find its place promises to be settled by science.

This is all so problematical as to resist every effort to condense. I refer to "every feature of reality," but this is hopelessly grandiose. One must mean features of reality available to the perceptual and cognitive resources of a certain kind of creature, in this case, a human creature. Then, too, to speak

of the language of the developed sciences is to speak of words, terms, and propositions that have had a history of radical recasting. A reality that once seemed readily expressed in the language of the science of Newton and Galileo would now be closer to mythology than to reality. My own father was alive and well when the best minds in physics regarded nothing as more certain than the *aether*. The same term today seems as if it were taken from astrology.

Affirming scientific explanation as the right model of explanation—the gold standard—carries still other questions and associated bones of contention. Philosophy of science today is in a state of confusion marked vividly by arguments between so-called realists and antirealists. The former would require of scientific laws and explanations that they record truthfully the real facts of the world. The latter argue that this is neither possible nor necessary, and that it is sufficient for scientific laws to be empirically *adequate*. Adequacy here refers not to rock-bottom reality, but to those manifestations of reality that do or can be expected to take part in the world as an object of experience. Bastiaan van Fraassen's *Constructive Empiricism* exemplifies this position (van Fraassen 2001). Perhaps it yields too much to an older logical positivism, but that is a small price to pay for liberation from the hegemonic pretensions of *scientism*; realists and antirealists provide different answers to the questions, "What is it that one has when one possesses an explanation?" and "What it is about a 'scientific' explanation that would preserve it as the gold standard?"

Resistance to the ultimate triumph of scientific explanation has a long history. In his William James Lectures of 1934, eminent psychologist Wolfgang Köhler recognized that the aesthetic, moral, and social dimensions of life were either ignored or depreciated within the framework of science. He addressed the issue at book-length in *The Place of Value in a World of Fact,* published under that title several years later (Köhler 1938). The first chapter of the book is titled "The Case Against Science" where a literary person informs Köhler that scientists traffic in false facts. In response to the claim that this is a contradiction, the critic offers the example of distant beings eager to know just what a human being is, only to learn about concentrations of sodium, potassium, water, etc. Every description is factual but, in light of the question asked, the overall account is false: It offers itself as a description of what it is to be a human being whereas it provides no more than a summary of the physical ingredients. Clearly, the problem here is not that the account

is incorrect, but that it seems to be the answer to a different question. It is important, then, to be clear on the various senses in which questions are plausibly taken. "What is man?" may be taken as a request for some sort of biochemical assay or as an expression of sheer wonder or as an inquiry into the proper location of a certain variety of animal within a given genus or species. It is a hallmark of scientism that it would legislate on the permissible senses in which questions are to be taken, but the source of this legislative authority must be established, not self-proclaimed. When Homer declares that the most pathetic of all creatures is the lawless, stateless, hearthless man, we are not to classify the claim as meaningless simply because we have no means by which to quantify pathetic! This point should be underscored: There are often several different senses of a question, equally clear in intent and equally worthy of reply unless we have some defensible means by which to rule out one or another sense. Thus, if one asks whether human rights are to be understood as universal or merely situational, the fact that the question does not admit of a scientific appraisal has no obvious bearing either on the meaningfulness or the importance of the question. Nor does immunity to scientific appraisal raise any serious reservations regarding the prospect of arriving at a plausible and even convincing answer.

No magic lantern is needed here. Scientistic skepticism toward questions that cannot be cast in terms familiar to scientific modes of address is either feigned or based on an indefensible narrowing of the range of things covered by human understanding. Native English speakers are not hopelessly confused by a question regarding the nature and possession of rights. Most are quite able to follow the course of challenges and assertions within our legal system as well as the opinions of judges and the assumptions on which the opinions are based. Persons understand the terms of agreements and promises and readily understand how such terms explain the actions required by them. These are not daunting tasks. They are well rehearsed at the evening meal. Billy has no right to a second helping of ice cream but surely has a right to be treated fairly, full stop.

There are more momentous versions of the same question. In determining rights and duties, society reaches the most fundamental dimension of civic life. This is not to be trivialized by glib predictions that some future development in the brain sciences will settle such questions. An older generation will recall

the turmoil plaguing American cities late in the 1960s and early 1970s. The assassination of public figures, combined with the prolonged and unpopular war, had led to episodes of urban mayhem. Unsurprising was the appearance of a small library of scientistic explanations. One of the more compelling accounts would have this civil unrest understood as a species of brain pathology! (See Mark & Ervin 1970.) Apparently brains that had functioned in a fairly normal way over a period of decades suddenly expressed pathological deviations under conditions of political unrest. One begins to see how simplistic thinking readily metamorphoses into dangerous applications.

The basis on which questions and answers in the moral and civic realms are understood is not fundamentally different from the basis on which scientific questions and answers are understood by nonspecialists. That basis—and here apologies are expressed in advance—is *pragmatic*. Apologies are in order owing to the sometimes reckless abandon with which the term is used, not to mention its illicit use as the favorite fig leaf of rank ignorance. I clarify my use of the term with an example: Reginald leaves the restaurant, gets behind the wheel of his car, turns the ignition key, and nothing happens. At the most general level, there are only two explanations likely to come to mind: *Either the laws and principles governing electrical phenomena and the performance of internal combustion engines have been suddenly and inexplicably suspended, or there's something wrong with the car*.

We are fairly confident that we know the explanation Reginald will adopt, not because we are able to read minds but because we and he understand that what makes scientific laws and principles what they are is that they are not suddenly suspended! If they were, they wouldn't count as laws; we couldn't count on them. They wouldn't work. It is in virtue of a pragmatic standard that we attach ourselves to one type of account over competing types. What Reginald has done, if we are to use a philosophically respectable expression, is made an *inference to the best explanation*. The best explanation for the success of the U.S. space program is that the physics and engineering principles on which it is based are correct; the laws are true; the image of reality fashioned by those laws and principles is veridical. This is why Reginald phones a towing company and not the Theoretical Physics group at the local university. Similarly, we understand that high rates of unemployment, stressful and divisive political climates, disruptions of family life, and the overall abandonment of traditional

moral values will conspire to create chaos. We require neither fMRI data nor psychiatry's version of *Star Trek* to reach a defensible explanation of just what it is we are looking at.

The reasoning that guided Reginald to the towing company exemplifies what Charles Sanders Peirce called *abduction.*[4] One reasons from data—from an event or a state of affairs—back toward the most economical hypothesis that plausibly explains it. Of course, to speak of an inference to the *best* explanation requires some measure of goodness, as in good, better, best. On Peirce's understanding, one such measure is *explanatory economy*. Thus, we awaken early in the morning and notice that water is dripping from the pitched roof. We assume it rained during the night. Reasoning takes us from an observed fact to the simplest hypothesis able to account for it. The movement of thought from the fact to the hypothesis just is a leap of abduction.[5]

Reggie's decision, based on sound abductive reasoning, led him to contact a towing company. Suppose, however, the source of wonder that night was the question, What is man? Reginald might again have two very general options. He could phone the Biochemistry Department of the local university, the most economical hypothesis here being that any entity qualifying as man must be a physical body made up of parts; or he could begin to read innumerable texts on the history and achievements of human communities. According to *scientism,* this latter option would ultimately be absorbed into or explained by or understood to be an expression of the former. Human history would, itself, be understood as the product of an inexhaustible interplay of physical processes.

Note, however, that both in fact and in principle this is not an especially economical explanation. One would face obstacles at every level, each requiring new and even untestable hypotheses to account for the appearance of hopes and dreams, motives and passions, worries and joys, thoughts and beliefs somehow emerging from a chemical soup. Worse, still, the result of the analysis would not pick out any life in particular or even any life that actually was or will ever be *lived.* One might have some sort of algorithm at the end of the process; a coding device such that, for a given hope or desire or belief, it would be possible to read out a set of chemical events. This read-out, however, would make no sense whatever by itself. For it to mean anything, the sequence would have to be reversed: The meaningless chemical record would have to

be tied to the felt or reported hopes, desires, and beliefs of the person. One is reminded of Wittgenstein's (1958) reflections at §120 in the *Philosophical Investigations:*

> When I talk about language (words, sentences, etc.) I must speak the language of every day. Is this language somehow too coarse and material for what we want to say? *Then how is another one to be constructed?*—And how strange that we should be able to do anything at all with the one we have! In giving explanations I already have to use language full-blown (not some sort of preparatory, provisional one)

The point here is perhaps so obvious as to be missed. When, in a state of *scientistic* optimism, experts assure us that it is only a matter of time before the folk language and folk understandings of ordinary persons becomes translated into the precise and objective terms of science; we have every right to ask what the validation procedure would be to make sure that these new objective terms have done the job. Of course, any validation procedure must accept just those personal states of thought, feeling, and experience so well expressed in what Wittgenstein refers to as "the language of every day." More to the point, even on the assumption that the psychological, moral, and aesthetic dimensions of life match up with data at the biogenetic and microphysiological levels, there will remain that seemingly unbridgeable gap between the meaningless occurrences at the levels of atoms and molecules and the phenomenologically rich level of mental life.

This, however, exposes yet another nest of vexing questions. Not every human life is phenomenologically rich. Some lives are psychologically constricted by disease or immaturity or enfeeblement. Still another specialty has been spawned to offer allegedly expert counsel in addressing such matters. It is the bioethicist, the specialist in medical ethics who is to be consulted as society must decide how scarce resources are to be allocated. Apparently common sense and common decency are no longer up to the task. Granting this much, it is still profitable to ask just what is to be added to common sense and common decency if it might claim parity with bioethics. Is it a matter of reading the number of books on ethics and moral philosophy? If one reads many such books one will discover that the principles on which ethical judgments depend vary considerably. Attaching

oneself to one set of principles will generate courses of action incompatible with those required by a different set of principles. Can the brain sciences—or some other collection of sciences—finally choose for us between utilitarianism and deontology? Between perfectionism and particularism? The reader must decide.

A few words are in order on the matter of the putative *objectivity* of the scientific versus the folk language of ordinary experience. As it happens, the concept of objectivity has its own history and a revealing one, as has been so carefully explored by Lorraine Daston (see Daston & Galison 2007). When botanists in the seventeenth century were able to avail themselves of microscopes for the first time, they were able to see beyond ordinary sight and thus render specimens with nearly photographic accuracy. The next task, however, was to commission artists to locate the specimen in the natural context in which it is ordinarily found, for only then was it presented *objectively.* In other words, the objective presentation is contextual and realistic. It would be over the course of succeeding centuries, with the development of laboratories and instruments, that objectivity would refer increasingly to the utterly *decontextualized* object or process. The aim was to remove the item of interest from putatively contaminating or distorting influences and to see or measure it in its pure state.

All this makes contact with that "The Case Against Science" with which Köhler began *The Place of Value in a World of Fact*. It also reaches the significant argument that William Dray (1957) advanced against Carl Hempel on the question of explanation in history. It is instructive to rehearse that debate for it captures what seems to be foundational in the differences that divide scientistic and humanistic conceptions of human nature.

I have provided a sketch of the Hempelian understanding of explanation and rehearsed some conceptual challenges raised against it. Hempel's conception of science was dominant during the early and middle decades of the twentieth century. Karl Popper, comparably influential, had written that "to give a causal explanation of an event means to deduce a statement which describes it, using as premises of the deduction one or more universal laws, together with certain singular statements, the initial conditions" (Popper 1935/1959, p. 20).

Consistent with this, Hempel developed the *deductive–nomological* (D–N) account of explanation. Summarizing the position, Hempel and Oppenheim stated that,

> It is this potential predictive force which gives scientific explanation its importance: only to the extent that we are able to explain empirical facts can we attain the major objective of scientific research, namely not merely to record the phenomena of our experience, but . . . to anticipate new occurrences and to control, at least to some extent, the changes in our environment. (Hempel & Oppenheim 1948, p. 138)

Thus, a scientific explanation should be at once predictive and retrodictive, preparing the observer for what will happen under a set of initial conditions as well as identifying the law that made the outcome logically inevitable. Hempel was satisfied that this approach was valid across all systematic bodies of knowledge, including economics, psychology, and history.

Alas, there is a large class of events for which on some accounts the model is utterly inapplicable in principle, events that nonetheless lend themselves to other modes of explanation in ways both warranted and entirely satisfactory. I refer to the class of events in which the elements of human rationality, motivation, and belief are not only present but present in a way that renders the events intelligible. The most obvious of events of this sort are those we denominate *historical*.

The thesis that the explanation of historical events calls for an approach distinctly different from Hempel's was influentially defended a half-century ago by William Dray in his *Laws and Explanation in History.* Dray was greatly influenced by Oxford's R. G. Collingwood who had written persuasively against unificationist approaches to explanation. He emphasized the differences between reasons and causes, between understanding the actions of a rational being and understanding the events in a mindless world of matter. As he put it in *The Principles of History*,

> People are born, eat and breathe and sleep, and beget children and become ill and recover again, and die . . . Yet none of these things have been traditionally regarded as possessing historical interest. Most of them have given rise to institutions like dining and marrying and the various rituals that surround birth and death, sickness and recovery; and of these rituals and institutions people write histories; but the history of dining is not the history of eating,

> and the history of death-rituals is not the history of death. (Collingwood 1999, pp. 45–46)

Dray expands the perspective and directs it against the claimed relevance of the natural sciences. In the first chapter of his book, he dubs Hempel's thesis the "covering law" model of explanation. Dray's central argument is that the subject of history, though entirely compatible with methods of systematic analysis faithful to empirical findings, is entirely immune to such a model. To begin, he emphasizes that explanations in history need not, do not, and finally cannot make appeal to general laws. He goes further, arguing that even in instances in which an event might be subsumed under such laws, it is not on this basis that they are explained.

In Chapter 5 of the book, Dray focuses on one of the essential ingredients in any plausible explanation of historical events: the motives, rationale, and overall intentions of the major participants as these are revealed in their actions. As with Collingwood, Dray is not proposing here a psychological analysis, but an analysis made possible by the fact that rational and striving beings can enter into empathic associations with others. To observe the actions of others—at least to the extent that the actions are of a sort of which one is capable—is to begin to understand the nature of the goals and beliefs that render the actions intelligible. The Battle of Waterloo is not deducible from some general law, nor is it explicable except by way of including Napoleon. That it occurred on a Sunday in June in the year 1815 cannot now be incorporated into some law we might fashion in order to explain a more recent battle that also takes place on a Sunday in June. Two years earlier, Beethoven composed *Wellington's Victory* celebrating the defeat of Joseph Napoleon's forces in Spain. It would be an odd sense of causation to claim that the best explanation of the composition is that it is deducible from some sort of general law that connects battles to orchestral music.

Dray acknowledges that terms such as war, revolution, etc., are general terms and do cover a large number and great variety of instances. This fact, however, should not be taken as evidence of the adoption of a law. There is no law that renders some activities revolutionary but not others. The French Revolution is not *explained* by showing that it is properly classified as a revolution. More generally, significant actions—actions worthy of the

attention of the historian or of the student of human nature—arise from conditions unique to the particular actors and generally unique in time, place, and overall context. We do not have the Battle of Waterloo without Napoleon and Wellington and the Prussian general, Gebhard von Blücher. The outcome was influenced to some indeterminate degree by Napoleon waiting a day for the wet ground to dry sufficiently for the movement of troops and ordinance. Again, there is no general law covering all this for there is no other battle like this one.

The point in examining the Hempel–Dray debate is that it draws attention to those realities of specific and actually *lived* lives for which the confines of the laboratory or the seminar room induce intellectual claustrophobia. Even though one might offer law-based alternatives to the D-N model, the alternatives themselves will suffer from the same faults as those besetting the D-N model. I say this not as a prophet but on the basis of the success of explanations in science and the logic on which that success depends. Were nature not orderly, were there not vast collections of events occurring with monotonous regularity, there would be nothing calling for a *scientific* explanation. Where there is such regularity, there is no need to go beyond the spatiotemporal facts. One need not wrestle with matters of culture, motive, conviction, passion, belief, delusion, competence, coercion, peer-pressure—the myriad factors within and acting on actual persons and rendering their actions intelligible to similar creatures.

The struggle of the poet at his desk is obvious to a mate who has seen it all before and knows the toll it takes. Meanwhile, Fido waits for the next tossed stick. We understand each other well enough, for our accounts of our own conduct are understood by others and neither we nor they require a vindication of judgment by appeals to a general law.

It might be countered that we understand each other owing to implicit laws that need not be consulted for, over a lifetime of experience, they are so thoroughly confirmed as to become part of our very modes of thought and action. But this is to confuse the relationship between actions and reasons for acting with causes and their effects.

On this point, Donald Davidson offers a useful challenge to theorists who would regard the relationship between reasons and actions as so regular as to suggest lawfulness in the scientific sense. Toward the end of his *Actions,*

Reasons, and Causes, and mindful of the Hempelian features some might find in the work, Davidson issues this caveat:

> I need to bring out what may be an important difference between the line I have been developing and Hempel's views . . . "Laws" are peculiar to individuals, and even to individuals at particular moments. So if a vagrant fancy for a ride on the fun wheel were to flit through Dora's mind, some law like this would be true: for the space of those few moments, if Dora had believed some action of hers would have produced a ride on the fun wheel (and a lot more conditions were satisfied), Dora would have taken that action. If this is the sort of law involved in reason explanations, we all know an enormous number of highly particular laws. It does not sound like much of an argument for the possibility of a scientific psychology. (Davidson 2001, p. 265)

It isn't. When all is said and done, the manner in which we make ourselves understood to one another is not beholden to the strictures of science or to those causal dependencies that are the stock in trade of the developed sciences. To insist that our folk psychology be abandoned for or translated into some radically different conceptual scheme—which, itself, is of our own making—is not a summons arising from authentic science. It is the preaching of a sect, of an *-ism*, of *scientism*. Serious persons are under no obligation to take it seriously.

Notes

1 For a full development of the thesis, see Hempel (1965, pp. 331–496).
2 An excellent discussion of these debts is John Norton's essay at: http://security2.net/h/how-hume-and-mach-helped-einstein-find-special-relativity-w356-pdf.pdf
3 I discuss this at length in Robinson (2012).
4 A useful source is McKaughan (2008).
5 An excellent analysis of Peirce's position, including relevant bibliographic material, is Psillos (2009).

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2

Scientism and the Religion of Science

Lawrence M. Principe

The past 30 years or so have been interesting times for science and scientists. There have been of course the many remarkable accomplishments and discoveries resulting from scientific inquiry that have renewed or revised our understanding of the natural world. But equally noteworthy have been the changes and challenges to the public image of the scientist and to the public face of science itself. In this regard, the 1980s and 1990s witnessed the so-called Science Wars that arose from postmodernist or, more precisely, social constructionist critiques of scientific objectivity and of the scientific establishment and its received authority. At the same time, religious fundamentalism began taking aggressive political and legal moves against the teaching and funding of specific scientific theories, particularly relating to evolution and cosmogony. Yet another development has been the rise of the celebrity scientist. Several such figures have authored best-selling books not so much to explain scientific ideas and discoveries, but rather to make expansive, sometimes shockingly bold, statements about larger epistemological and ontological issues and to declaim about the role of science, scientific methodology, and the scientist in human society and civilization generally. Gone (it seems) are the tweedy, bespectacled lecturers of the 1960s who were content to educate public audiences on the subjects of their specific expertise. Similarly diminished is the image of absent-minded professors and wild-haired Einsteins who, while thinking great and lofty thoughts, forget to change out of their pajamas for lecture. Instead, we find in their place today a handful of scientists who have become household names by leaving the confines of their narrow fields of expertise to preach a political, philosophical,

and religious agenda that cannot be supported by either their expertise or the methods they claim that science follows. Their position has been given the (pejorative) name of scientism. Given its current profile in the public sphere, scientism has attracted the attention of philosophers and many others. This chapter approaches the subject from a predominantly historical perspective. It explores scientism's historical roots and those of the closely related notion of a longstanding "warfare of science and religion," as well as the context of scientism's emergence and apparent popularity.

Although a certain range of definitions exists for scientism, there is a consistent core of features that can be agreed upon by both its proponents and its opponents. Most central is the belief that science and its methods provide the only fully valid route to gaining knowledge and for answering questions, to the exclusion of other methods and disciplines. This definition is satisfactory only if we define in turn what those methods are, since the very term science and what counts as scientific and the scientific method have varied considerably over time and across cultures. What is customarily meant by science by those to whom the term scientism applies is current-day empirical natural sciences, and its method is that of observation—possibly including indirect or instrumental measurements, and very often with some level of mathematization—together with the method of induction. No one would (or should) take issue with the notion that current-day natural science should and must pursue its questions and endeavors by means of particular, more or less agreed-upon methods that we in turn choose to call scientific. Undoubtedly, the methods of modern science have proven enormously successful in answering a myriad of questions about the natural world. The real contention arises only when the exclusionary claim is made that *only* science as we understand and pursue it today is capable of providing valid and acceptable answers to *all* questions, and that any questions it cannot answer are either meaningless or simply not worth asking.[1] It is the hegemonic, universalizing, and exclusionary claims of what might be called strong scientism that lie at the crux of the argument. It is these claims that render scientism a subject for discussion and critique.

It bears making explicit that it is not merely the holding of scientistic beliefs that generates controversy. If these ideas were privately held and used to guide an individual's thinking and behavior, there might be interesting

discussions over coffee or in academic settings, but not a great deal more. But for scientism's most vocal advocates, a key feature of the system is—depending upon whether you prefer a political or a religious metaphor—imperialism or evangelization. This situation can arise naturally from the uncompromising claims of scientism. It claims science to be the only acceptable system for gaining knowledge, anything else is at best a poor second, or simply a delusion. If scientism were turned against the insights offered and expressed by poetry, art, music, or aesthetics, its advocates might simply be labeled as philistines and ignored. Scientism gains its highest public profile, however, and virtually defines itself, when it attacks the institution that has, more than any other, made use of and fostered poetry, art, music, aesthetics, and arguably science itself—namely, religion. Religion claims to have something significant to say about the human condition and human beings' place in the natural world, and a great many intelligent people consider it to offer a particular and valuable vision of the truth. The conflicts begin when scientism claims that only science (usually narrowly or not at all defined) can explain or address the human condition. Since this is the case, it is especially the interaction of scientism and religion that comes to our attention, and that bears closer investigation.

It is surely safe to say that everyone today has heard of the conflict between science and religion. Yet this expression is too broad and too imprecise to withstand scholarly scrutiny. It still comes as a surprise to many that no serious historians of science today acquiesce in the simplistic notion of a perpetual conflict or warfare between science and religion.[2] The interaction between science and religion has been complex and varied over time, and often cooperative.[3] There is also the question of what conflict actually should be taken to mean. It would be absurd to assert that scientific opinions and theological opinions do not at times conflict with one another, in the sense of putting forth discordant or apparently discordant or divergent claims. But this is the sort of conflict—a diversity of opinions and claims—that exists everywhere within science itself as well as within religion/theology, and without which little intellectual development of any sort could take place, and so such conflict need not be taken as a bad thing or a sign of trouble.[4] This mild meaning is far from the popular idea of conflict(i.e., a more or less perpetual state of warfare). Moreover, there are the thorny issues of the shifting boundaries of what constitutes either science or religion, and to what extent a given

individual is capable of speaking for the entire collective that he claims, or is assumed, to represent. I think it would actually be more correct to speak not of a broad and ill-defined conflict between science and religion, but rather of a conflict between *scientism* and religion. For scientism, at least among its strong advocates, has in fact *explicitly* declared itself the opponent of religion. Since the notion of a fundamental science–religion conflict has become a core belief of modern-day scientism, a closer examination of its historical background and motivations is necessary.

The roots of scientism and the origins of the myth of the science–religion warfare

The claim for science to have a special, or indeed an exclusive, access to truth and the idea of a more-or-less constant state of warfare between science and religion arise from the same set of historical sources. Central to these is the legacy of Comtean positivism, a formulation deriving from the ideas of the early nineteenth-century philosopher August Comte (1798–1857). In the 1820s, Comte began searching for ways to reground society in the aftermath of the French Revolution and of Napoleon's rise and fall. In the 1840s, he posited a Law of Three Stages for human societies, namely, that they progress through three successive phases of development—first the theological, then the metaphysical or philosophical, and finally the positive or scientific. Looking around at contemporaneous France, he viewed the Enlightenment as the end of the first phase, the attempts of Napoleon to reconstitute law and society on a secular basis as the second phase, and his current day of the mid-nineteenth century as the dawn of the scientific or positive phase, when science and scientific principles—he hoped—free from uncertainty and ambiguity would provide a rational and unassailable basis for the reformulation of human society according to a new science he called sociology. Later in life, Comte also promoted the establishment of an *ersatz* religion based on positivism, the *religion de l'humanité*, replete with services, rituals, priesthood, and sacraments, incarnated in several churches of positivism. Despite this unsuccessful attempt to launch a positivist religion, which alienated many of Comte's original followers, Comtean positivism implied that theistic religion belonged to a

primitive state and science to a mature state of humanity. The move from one stage to another resulted from an inexorable process of progressive evolution; human behavior, Comte believed, was determined by natural laws as certain as those of physics.

Comte's Law of Three Stages was soon both popularized and interwoven with the notion of perpetual struggle between science and religion in John William Draper's *History of the Conflict between Religion and Science*, first published in 1874 and frequently reprinted and translated thereafter.[5] Draper was born in England, the son of a Methodist minister; he immigrated to the United States where he became a professor of chemistry and physiology, and later an amateur historian. Draper's *Conflict* must be viewed in the context of his earlier historical publications and proposals, upon which it is in large part dependent. In the 1860s, Draper wrote two lengthy historical works, one on the American Civil War and another entitled *The Intellectual Development of Europe* (Draper 1863, 1867). In both of these works, he argued that all events in human history were caused by physiology and climate and asserted that a Universal Law of Development governed not only the evolution of the physical and biological worlds but also determined the physical and intellectual development of human individuals, the historical development of human societies, and even that of human civilization as a whole. In this regard he followed, indeed surpassed, Comte in asserting the absolute supremacy of supposed universal natural laws, akin to and as necessary and inescapable as those of physics, that govern humanity. But along with popular mid-nineteenth-century ideas about race, physiology, progress, and the effects of climate, Draper's reductionist interpretation of history also drew upon theories dating from earlier naturalists such as Georges-Louis Leclerc, Comte de Buffon (1707–1788), and Jean-Baptiste Lamarck (1744–1829), and especially upon ideas drawn from the contemporaneous philosopher Herbert Spencer (1820–1903).[6] Draper's amalgamation of these diverse notions—some already outdated by the 1860s—was effected in a slightly cartoonish, often self-contradictory manner, and illustrated with historical information much of which was unreliable even at the time. Nevertheless, his attempt comprehensively to explain all of human history scientifically led also to his *Thoughts on the Future Civil Policy of America* (1865), where he enunciated sociopolitical proposals for the future in a way akin to what Comte had advocated.

From this background arose Draper's later *Conflict between Religion and Science*, the book that gained him the greatest notoriety, such that his earlier (admittedly more turgid) books are nearly forgotten. Draper was commissioned to write the *Conflict* as part of the International Scientific Series, a publishing project devoted to the popularization of science. *Conflict* became a bestseller—passing through numerous editions and translations, and it remains easily available today. The main point to be stressed here is that while the book is a valuable historical artifact of its time and place of composition, it is *not* a reliable source about history. The book is guided by Draper's Universal Law of Development, now combined with a specifically anti-Catholic rant that often reaches the level of the hysterical. "Roman Catholicism and Science are recognized by their respective adherents as being absolutely incompatible; they cannot exist together . . . mankind must make its choice—it cannot have both" (Draper 1874, p. 363). (This statement would imply that a Roman Catholic scientist could not exist, contrary to the evidence of one's senses—both in 1874 and today.) The harmonious alliance of Protestantism and science, on the other hand "would easily take place" with only minor adjustments. Draper's hatred of Catholics is part and parcel of widespread late nineteenth-century American anti-Catholicism and xenophobia—founded on deep English roots—particularly against newly arriving Catholic immigrants. Draper had already declaimed against the "insidious agency" of immigration and of a "hybrid population" that he cited as the cause of the degeneration of the "pure Roman race" into an "adulterated festering mass" (Draper 1865, pp. 110–112, 150–151, 165, and 241).[7] Indeed, the *Conflict* might not have found so broad a readership at the time of its publication had it not been for the contemporaneous American panic over Catholic immigration. Today it is virtually unreadable by an educated person. Draper's animus against Catholicism presumably also stems from personal circumstances, namely his fury (as son of an English Methodist minister) at his sister Elizabeth's conversion to Catholicism, and the likelihood that his father's conversion to Methodism caused his ostracism by the rest of the Drapers, who were themselves Catholics (Fleming 1950, pp. 1–2, 31, 143 fn. 1, and 144 fn. 6). So vehement is Draper's hatred that he actually refers to the rise of Islam as the "Southern Reformation"—a precursor to the "Northern Reformation" of Luther—and contrasts Islam, supposedly ever beneficial to science, with Catholicism that was supposedly ever malevolent (Draper 1874, p. 68).

Some of this rancor, and the larger idea of a persistent science–religion conflict, had emerged in Draper's earlier writings: "ecclesiastical establishments . . . will ever continue to be what they have always been—the determined antagonists of science" (Draper 1865, pp. 235–236, 280–281).[8] Yet in these earlier works it is occasionally, and quite oddly, counterpoised by a curious admiration for the centralization and endurance of the Papacy as a political institution (Draper 1865, pp. 314–317). What seems really to have driven Draper over the edge by 1874, however, were conciliar and papal declarations in 1864 and 1870 that responded to the proliferation of philosophical and political ideas that the Catholic Church considered contrary to religion. For example, the 1864 "Syllabus of Errors" issued by Pius IX cited absolute rationalism (that scientific and rational inquiry can reveal everything there is to know, in other words, a thing akin to what we now call scientism), and the denial of God's action in the world, while documents of the First Vatican Council spoke out additionally against materialism (the idea that matter is all that exists). These and other documents of the time likewise asserted the role of faith in human life and understanding, the defense of Christian education, and the role that religion should play in governing and guiding society. Such statements stood in opposition to Draper's positivism that he adapted from Comte, and challenged Draper's deterministic "Law of Development." Moreover, by asserting the teaching role of the church and denying that the exercise of scientific reason could answer all questions, these declarations probably affected Draper more viscerally by placing limits on scientists' authority to speak on all matters whatsoever. This issue of authority is a crucial point that will be taken up again in this chapter.

Draper's 1874 *Conflict* was soon joined by an early version of the second major book to promote the idea of perpetual warfare between science and religion—Andrew Dixon White's 1876 *Warfare of Science*. White, the founding president of Cornell University, had begun speaking publicly about the battles between science and religion in 1869, and his efforts culminated in a massive two-volume *History of the Warfare of Science with Theology*, published in 1896. White's work differs in several respects from Draper's. It was less popular, does not show Draper's fervent anti-Catholicism, and was more extensively footnoted, which gave it a patina of scholarly authority. White's tone is somewhat more placid (while remaining melodramatic at turns), arguing that

religion is good in itself, but should stay out of the way of science, for their interaction damages both. Nevertheless—and despite White's standing as the first president of the American Historical Association (1884–1886)—the book is no more reliable as a historical source than Draper's, and it too originates in a very personal context. White's book is a response to criticism and opposition he received over the founding of Cornell as a secular institution, in contrast to virtually all other colleges of the time that had religious affiliations. Thus, the book's intent was predominantly to refute or at least to quell such criticism. The more scholarly format of White's final book meant that it was used as a source even by historians of science until the middle of the twentieth century.

The impact that these two books have had, and continue to have, on popular culture (or rather, on what is called common knowledge) is astounding. Few people today have actually read either of them, or perhaps even heard of them, but the mistaken historical notions they promulgated have become naturalized as *facts* in contemporary culture. For example, most schoolchildren are still taught that in the Middle Ages, or more specifically, before Columbus, people thought the Earth was flat. This canard was confected by the fiction writer Washington Irving in his romanticized *Life of Columbus*, but was presented as historical fact and broadly disseminated by Draper and further embellished by White (Irving 1828, p. 88 et passim; White 1876, pp. 10–20; Draper 1874, pp. 157–159; Cormack 2009). It is still encountered frequently in popular, supposedly nonfiction, writings to this day. Likewise, the assertion that the Catholic Church forbade and/or punished human dissection was another false claim that both Draper and White popularized—along with countless other falsehoods about the history of science and the history of religion (Park 2009). In short, both Draper and White used highly questionable or simply unreliable historical sources and played fast and loose with the facts in order to produce false historical narratives that would support their main claims about science and religion.[9] These claims and the evidence adduced for them have taken on a life of their own beyond their books and beyond the specific local and personal contexts that spawned them, thus providing the foundations for today's strong scientism and for the uncritical acceptance of scientistic claims among the wider public.[10] Even the crusading rhetorical tone of Draper's and White's prose seems to have set a pattern followed by the tenor of current scientistic declarations.[11]

The fundamental methodological error of both authors is their assumption of the existence of two groups—religionists and scientists—that simply did not exist as such before the late nineteenth century. Virtually every contributor to scientific knowledge before the nineteenth century was also a believing Christian, many very deeply religious, and many of them clergy. The great majority saw the exploration of the natural world as a kind of religious devotion because it revealed God's creative activity. This perspective is based in the well-known Doctrine of the Two Books, enunciated by several Patristic authors, and perhaps best by St. Augustine of Hippo in the fifth century. The African doctor argued that God reveals Himself to humanity in two different ways—on the one hand by inspiring human authors to pen the Book of Scripture, and on the other by the very act of creating the universe, which Augustine called the Book of Nature. If one can learn to read the Book of Nature by studying it, one discovers ever more about its Creator.

Historical examples of the inseparability of the supposed camps of scientists and religionists are easy to find and enumerate to the point of exhaustion. Robert Boyle (1627–1691), the champion of chemistry and discoverer of the fundamental gas law that bears his name, was called in his eulogy a "lay bishop" and his books divide fairly evenly between scientific and theological topics. He claimed that doing scientific experiments was especially appropriate on Sunday, because they were a kind of worship (Hunter 2010). Isaac Newton (1642–1727) devoted at least as much time to theology and biblical studies as he did to mathematics or physics (Westfall 1980, Force & Popkin 1999). And it is trivially easy to cite a long list of scientific work and achievements from the Middle Ages to the present carried out by ordained priests—St Albert the Great's work on mineralogy and embryology, Pierre Gassendi's revival of atomism, Athanasius Kircher's descent into the crater of an erupting Vesuvius to make observations, Giovanni Battista Riccioli's experiments to measure the constant of gravitational acceleration, Bl. Niels Steno's observations of fossils and strata that set the foundations of modern geology, John Ray's botanical classification system, and, in the twentieth century, Georges LeMaitre, the Belgian Catholic priest who held degrees in the sciences from Cambridge, Harvard, and MIT, and who first developed Big Bang cosmology.[12] To say a few words about perhaps the most famous case regarding science and religion, the supporters and detractors of Galileo were evenly distributed among

ecclestiastics and laymen, and both criticism and support of his system of the Earth's motions came from both theological and scientific knowledge. While some churchmen and secular professors criticized him, others praised him; for example, the Jesuits at Rome gave him a celebratory feast to mark his telescopic discoveries, and Galileo liked to quote Cardinal Cesare Baronio who quipped that "the intention of the Holy Spirit is to tell us how to go to heaven, not how the heavens go" (Finocchiaro 1989, esp. pp. 1–43).[13] So into which of Draper's and White's presumed camps should these figures be assigned?

The point is that Draper and White were illegitimately transporting the emerging social stratifications of their own era backward into earlier times. By constructing the notion that two rival camps—scientists and religionists—had existed throughout history, they set up an inherent and essential rivalry between science and religion that did not exist as such. Interestingly, despite their explicit use of a *military* metaphor, they implicitly—whether consciously or unconsciously—borrowed the imagery and structure that characterized the history of *religion* itself. They created a litany of martyrs—most notably Bruno and Galileo, but also Roger Bacon, Michael Servetus, and others—and a hagiography of sinless and oppressed reformers and visionaries that populated the scientists' camp.[14] They implicitly recast scientists as prophets and priests, the recipients of special favor and enlightenment, who brought forth truth and struggled to spread a gospel of science and progress against the darkness and ignorance of the pagans (i.e., the old priesthood of religion). In this way, they co-opted for themselves all the drama and emotional power of the story of the early Christians persecuted by—but finally victorious over—oppressive Roman paganism. This origin myth of science laid the foundations for setting up science as a religion of its own.

This origin myth of science remains extraordinarily powerful today, and it stands at the core of scientism. It is constantly repeated uncritically by a host of popular books and television programs, and as a virtual shibboleth by the prophets of scientism. Indeed, it has been my personal experience that it is dangerous (or at least foolhardy) to question its orthodoxy around those of a scientistic persuasion. I remember receiving an email from an undergraduate student in the sciences who had recently read some of my publications on early modern science. He was literally distraught because I had demonstrated that the heroes he had been taught to revere—Kepler, Boyle, Newton, and the

like—were actually . . . *religious believers*. How could this be, he asked. For him, it was a crisis of faith, with all the characteristics of a crisis of religious belief. And indeed it was, his faith in the origin myth and the religion of science had been shaken. I fairly regularly get mail from members of the public who have read my more popular books and lectures, and while I routinely critique the claims of both religionists and scientists, I rarely hear anything negative from the religious side. But when I present well-established historical evidence that undercuts the simplistic warfare version of the Galileo and the church narrative, or enumerate scientific and logical features of medieval theology, or, perhaps worst of all, point out false historical claims or sloppy reasonings made by scientistic prophets like Carl Sagan, then the criticism, the expressions of disbelief, and the declamations of ulterior motives fly freely.[15] Two features emerge: first, any positive statement about historical figures traditionally placed in the religion camp is unacceptable. Second, most such critics rely entirely on the origin myth mentioned earlier, and simply will not accept any evidence to the contrary, responding to such evidence with a simple no or by repeating now-discredited accounts. This is why I must conclude—as others have done—that the strong scientism of the modern day is not merely a religion, but is in fact a kind of *fundamentalism*.

The story told by Draper and White fits very neatly into a literary trope beloved in the nineteenth century—that of lone, enlightened, and oppressed genius. This trope was—and still is—applied in all directions: to scientists, to artists, to musicians—regardless of historical evidence to the contrary. For example, the notion that a misunderstood Mozart had a pauper's burial in a mass grave—in fact he had a middle-class funeral and burial typical of the day—and that now-celebrated artists were iconoclastic visionaries who struggled in poverty rather than being hired artisans who sought and were supported by patronage. Both schmaltzy television fiction and modern television documentaries purporting to recount history are routinely built on the same trope. The popularity of the trope provides evidence that most people do in fact respond positively to stories of oppressed genius pitted against all odds—no doubt in large part because we all like to think of ourselves as misunderstood and ignored geniuses. This reflection is more than a humorous quip; it points to a second key bit of context for Draper and White and the persistence of their mythological history, namely, the nineteenth-century rise of the professional

scientist and the concern over his social status and authority. In the nineteenth century, the very word scientist had only recently been coined. There were few persons who could be called professional scientists in that they were paid primarily for what we see as scientific endeavors (Ross 1991). Colleges and universities focused primarily on the classical liberal arts, and a clear academic pathway for becoming a scientist, and being recognized as such, was only beginning to develop.[16] Most investigators of nature were thus amateurs—interested persons with the leisure, funds, and abilities to devote themselves to such pursuits. Consequently, the social position of the emerging scientist was far from secure—who was he? What did he do, where did he fit in society, what established or guaranteed his authority? The identity, position, and authority of political and religious figures had been well established and understood for centuries, but those of the newly emerging scientist were not.

As a result, nineteenth-century scientists needed to craft an identity for themselves, to carve out a stable—preferably high—social position, and to invest themselves with an authoritative voice. Draper and White and many—though far from all—of their contemporaries did this not by creating new authority *de novo*, but by appropriating it from the already established authority of religion. This strategic move was facilitated, and presumably normalized, by the widespread secularization of governmental authority that was going on at exactly the same time. In due course, scientists succeeded in establishing their identity and public authority such that they are now essentially licensed by degrees and publications and are consulted and respected as experts on a variety of issues. Naturally, such redistribution of social authority provoked conflicts, and in this way, the false histories of such writers and White and Draper became, in effect, blueprints for the future. The struggle for authority undergirds the so-called science–religion controversy down to our day.

Scientism today and the public good

Let us then return to the present and examine current scientism's place and role. One observation worth discussing is the clear proliferation of scientistic claims—of increasing volume—during the past 20 or so years. Its partisans might well point to this fact as evidence of positivistic progress of the

Comtean kind—more and more people are seeing the light and advancing to their point of view. On the other hand, it is hard to overlook the fact that this growth in strong scientism has occurred simultaneously with the growth of aggressive fundamentalisms within all three of the Abrahamic religions and beyond. Once this observation has been made, the striking similarity between the claims and pretensions of the respective advocates of these groups not only becomes clear, but also suggests several interesting linkages and common origins.

Christian fundamentalism is a product of early twentieth-century America. Its origins lie in a set of social, demographic, and political shifts that include the rise of urbanism, the shift from an agrarian to an industrial base, the influx of non-Anglo-Saxon immigrants, and the development of mainline Protestant theologies that by 1900 had largely accepted (or at least accommodated) biblical higher criticism and new scientific ideas such as biological evolution.[17] Fundamentalism arose as an oppositional, reactionary movement by those—particularly in rural, southern, and Midwestern communities—who felt left behind, excluded, and even ambushed by these changes. They were suddenly confronted—with the help of improved communication—with the fact that America was not the place they thought (in other words, it did not look like their isolated hometowns) and that they were not the social, ethnic, political, or theological majority. Hence their position in society was not only not what they imagined, but was jeopardized. The result was frustration, anger, a hardening of positions, a banding together into more organized groups, and a crusading mentality. The fundamentalist movement largely retreated from view in the late 1920s, but reemerged in the 1960s in response to an organized program to update the woefully outdated American secondary school science curricula nationwide. As before, rural communities saw the introduction of new textbooks into their public schools—particularly the biological ones organized around the explanatory power of the theory of evolution—as an invasion, and undoubtedly, a reminder of their own displacement from the center of American society. Thus, fundamentalism has always been fueled by social and political insecurity and does not represent the mainstream of Christian thought—however much its proponents like to think it does.[18]

The scientific establishment and its social and political status have been shaken in very similar ways during the past generation. Gone are the heady

days when science and the scientist represented the unsullied champions of progress, truth, and advancement. The rise of postmodernist critiques of science and its claims, which led to the science wars of the 1980s and 1990s, assailed the supposedly uniquely authoritative character of science and its societal role, as well its methods and claims. Disillusionment over the failure of science's promise to solve humanity's problems and bring peace and prosperity to all, criticism over pollution from atomic and chemical waste, and that fine old standby of American culture, anti-intellectualism, all served to weaken the position that science—and by extension, scientists—had steadily achieved since the late nineteenth century. At the same time, religious fundamentalism added its voice in regard to evolution and directly attacked scientific authority. Thus, in the late twentieth century, science found itself—to the surprise of its practitioners—under threat. Since scientists now rely on massive government funding, these issues have much more than merely academic significance, for scientists find not only their authority challenged, but also the governmental patronage crucial for their work jeopardized. In short, they find themselves unexpectedly in an unstable social and political position—one strikingly similar to that of the religious fundamentalists. One response has been largely the same—a hardening of position and a crusading mentality, one that overextends science's applicability and attacks those groups and ideas perceived as adversaries or competitors.

Significantly, the loudest expressions of strong scientism occur precisely in the context of scientific opposition, usually aimed at a popular level, to fundamentalist attacks on the teaching of evolution and the closely related issue of the age of the Earth and universe. Thus, the science–religion controversy of our own day reveals itself as nothing so noble or interesting as the interaction of rival philosophical systems, but rather as a struggle between two crusading fundamentalisms powered by concerns about social status and public authority. (This observation is not intended to minimize the necessary efforts of concerned scientists to defend well-established scientific conclusions, or those of educators to protect the proper education of a scientifically literate populace. The critique here is aimed at the shrill voices at both ends of the spectrum that pontificate about topics far beyond their sphere of competence.) Who will teach the next generation, what will they teach, and what will be the fate of whichever side emerges as the loser?

Unfortunately, it is the public good that is currently the biggest loser. The current situation besmirches and weakens two powerful and crucial forces—science and religion—that should contribute jointly to the public good. I am particularly struck by the arrogance of both groups, both of whom presume to represent realms of thought that should be instilling humility, and a recognition of human limitations before the incomprehensible grandeur of the universe and the unfathomable mystery of existence. Instead, both groups compete to give trivial, simplistic, dismissive answers to complex and perhaps unanswerable questions, and in so doing not only preempt more considered and sophisticated thoughts and meditations, but also alienate and wound the more flexible and open-minded public. Now if the public were merely alienated from fundamentalisms of all sorts, there would be little to complain about—perhaps something to celebrate. But the fact is that both sides presume to speak for much larger groups that they do *not* legitimately represent. Christian fundamentalists do not speak for all Christians, nor for the entire mass of nearly 2,000 years of sophisticated Christian theology and philosophy worked out by some of the most acute thinkers who ever lived. Scientistic fundamentalists do not speak for the entire enterprise of scientific inquiry or for the whole population of scientists, most of whom would prefer to be simply left alone to do their important work without being caught up in political and philosophical issues that most see as outside their interests and beyond their realm of competence. The busy general public cannot easily distinguish between these loud minorities and the larger communities they pretend or appear to represent. Nor is the public assisted at all by the oversimplifications and self-serving melodramatics of the mainstream popular media, much of which is currently constituted to benefit from dramatic and infuriating sound bites rather than from considered, intelligent discourse. The result is then either a taking of sides based on faulty and selective information, or a rejection of or ill-considered prejudice against the larger realms of both science and religion. This situation is clearly to the detriment of the public good.

Fortunately, I do think there is a solution. One of the things that characterizes fundamentalism, whether religious or scientistic, is the demand for certainty and the conviction that its proponents possess it. The antidote is a pinch of doubt. Not so much doubt that we lapse into nihilism or hopeless skepticism, but just enough to allow for *growth*. Doubt provides the crucial space in which

inquiry is born, and the room in which to grow in knowledge and belief. Unfortunately, religious people—and not just fundamentalists—are too often encouraged to view doubt as a kind of weakness. But it is doubt that gives strength to faith—without doubt there is in fact no faith. Only when there is doubt can there be a role for faith, and it is only by faith that either religion or science can operate. Without doubt, science and theology—the twin offspring of human questioning—would come to a standstill.

Thomas Aquinas, when considering how God might act within the natural world, examined how a true miracle (defined as an effect brought about by God acting as a direct cause) might be distinguished from an unusual or surprising but purely natural event. He notes not only that our knowledge of the entirety of natural powers and phenomena in the world is incomplete, but also that there is no way for human beings to see with perfect knowledge the chain of causation that results in a given effect. Even a relatively familiar occurrence that happens regularly from natural causes might in a particular instance have God as the immediate cause, or a special supernatural application of a natural cause.[19] Thus, no matter how great our knowledge of the world, we are always left with a certain residue of uncertainty or doubt about causes, and therefore, deciding whether or not something is miraculous or natural must ultimately require an act of faith *in either direction* (i.e., to assert either its natural or its miraculous character). Here we clearly return to the realm of science and scientific knowledge. It was this same issue of causation that formed the subject of a discussion in 1624 between Galileo and his friend Maffeo Barberini, then recently elected as Pope Urban VIII. Galileo was absolutely certain—had no doubt—that the tides were caused by, and thus evidence of, the Earth's motion. Urban replied that this might be possible, but that the world might be so contrived that a given phenomenon might have more than one cause, and some of those causes might well be beyond human ability to sense or to discover. (It was, by the way, Galileo's apparent mockery in print of the pope's position that in large part turned his former admirer against him such that Urban VIII insisted on Galileo's prosecution.)

The limits of human knowledge that should ensure the existence of doubt pertain equally to both science and theology. Can we determine true and complete causes of phenomena or only the apparent ones? Are what we conclude to be causes always true causes or only correlations? Do our senses give us reliably true knowledge of the external world? Is the universe regular

and uniform? None of these questions that stand at the heart of scientific investigation can be answered with certainty; each requires an act of affirmative faith that allows the scientific enterprise to function. This observation is *by no means* intended to undermine the scientific enterprise or to question its validity or value, but only to highlight the essential role played by faith (which presupposes doubt) not by certainty. The foundations that scientific work takes for granted—and must take for granted—are in fact insoluble issues that trouble philosophers greatly. These issues do not go away or become any less faith statements when scientistic oversimplifiers wave off the concerns of philosophers. This is the wonderfully sloppy, seat-of-our-pants thing we call science—neither it nor its practitioners gain any more authority by making bold assertions of its unique access to truth or its supposed certainty. The recognition of doubt and faith does not weaken science but liberates it from the straitjacket of an imperialistic or hegemonic position that it cannot legitimately maintain. And the same is to be said of religion, where faith—no matter how strong—ought never to be confused with certainty.

In conclusion, I recall that Aristotle claimed that scientific inquiry (which he divided into the natural, the mathematical, and the theological) began when human beings had enough leisure time to look out at the world and *wonder*.[20] Wonder is itself a kind of doubt—a doubt that challenges our senses and our minds, and spurs us onward toward that thing that both good science and good religion have loved and fostered, namely education in the pursuit of knowledge, understanding, and the well-lived life. A better understanding of history will serve to smash the origin myth at the core of scientism, strip away its rhetorical and fictional bases, and reveal the often surprisingly cooperative dynamic that has very frequently characterized the pursuit of both science and religion. A better understanding of philosophy will reveal the astonishing gaffes and nonsequiturs made routinely by the modern-day prophets of scientism, and their implicit reliance on insupportable, outdated narratives. A better understanding of the richness of theology and biblical interpretation will undercut both the simplistic claims of religious fundamentalism and the scientistic replies made to it. Equally important is a better recognition of motivations—including self-motivations—that are born of social and political insecurity, and as such, are not essentially related to the proper goal of both science and religion, namely, the difficult, thorny journey toward an ever-better understanding of ourselves, the universe, and existence itself.

Notes

1 As two examples of many, Peter Atkins asserts the "universal competence" of science to resolve all questions, Atkins (1995, p. 97), and likewise, Hawking and Mlodinow (2010, p. 5) declare triumphantly that "philosophy is dead" and that "science" (whatever that might mean) is its victorious successor in the "quest for knowledge" (whatever that might mean). It would be valuable to trace the historical dependence of such dismissive views upon 1920s' logical positivism, a topic beyond the scope of this chapter.

2 For example, see the various essays in Numbers (2009), *Galileo Goes to Jail and Other Myths about Science and Religion*, esp. p. 1.

3 For a careful study of the complexity of the situation (which has been dubbed the "complexity thesis"), see Brooke (1991), *Science and Religion: Some Historical Perspectives*. Historical analysis of more specific episodes can be found in the essays collected in Lindberg and Numbers (1986), *God and Nature: Historical Essays on the Encounter Between Christianity and Science*.

4 For a useful examination of this point, see Cantor (2010). My point here is not to deny the existence of more serious conflicts, but only to underline the various degrees of meaning possible within the term "conflict."

5 Draper (1874); on Draper's adaptation of Comte, see his biography, Donald Fleming, *John William Draper and the Religion of Science* (Philadelphia: University of Pennsylvania Press, 1950), pp. 49, 55, and 58–59.

6 The striking similarity of Spencer's all-encompassing notion of biological, social, and intellectual development based on laws such as those of thermodynamics as enunciated in *First Principles of a New System of Philosophy* (1862) with Draper's ideas has not, to my knowledge, previously been pointed out by historians. Further investigation of such a link may prove enlightening.

7 Apparently, Draper forgot that he himself was an immigrant, or perhaps reckoned that Anglo-Saxons do not count as such.

8 It is worth noting that in this book, his treatment of Anglicans is scarcely better than that of Catholics; see Draper (1865, p. 289).

9 It is of course difficult (and not particularly useful) to prove *mens rea* in retrospect, although certain manipulations, innuendos, and juxtapositions cannot but make one slightly suspicious; for example, his selective and truncated quotation out of context of a passage in Augustine's *City of God* to evidence something diametrically opposed to what the African doctor really intended. See White (1876, pp. 99–100) and compare with *City of God*, book 22, chap. 24, which White footnotes.

10 For a fascinating example of how aspects of the science–religion narrative of Draper and White have developed into "common knowledge" that is repeated uncritically by several of the major exponents of scientism, contrary historical evidence notwithstanding, see Lessl (1999).
11 Even Draper's anti-Catholic ranting finds a remarkable modern-day counterpart in the stammerings of Richard Dawkins' off-target "response" to John Paul II's comments about evolution; see Dawkins (1997). Compare his tone with that of other contributors to that volume of the journal.
12 For Steno, see Cutler (2003).
13 For the various developments and deployments of the "Galileo mythology" see Finocchiaro (2005), *Retrying Galileo, 1633–1992*.
14 On Bruno, see Draper (1874), pp. 178–181; compare with Shackelford (2009).
15 On the uncritical use by Sagan and others of the Draper–White narratives, and more generally for the "prophetic" mantle assumed by public exponents of scientism, see Giberson and Artigas (2007), esp. 137–144.
16 This very point is mentioned by Draper (1865), pp. 274–275; see also Bruce (1987); Kohlstedt (1976); and Kohler (1990).
17 For extremely useful overviews, see Marsden (1991) and Numbers (1992).
18 One could, *mutatis mutandis*, usefully extend this analysis to Muslim and Jewish fundamentalisms, as reactions against Westernization, globalization, and assimilation.
19 Thomas Aquinas, *Summa Theologica* Ia, 105, 6–8; 110, 4; and *Summa Contra Gentiles* III: 99:2, 9; 101–102.
20 Aristotle, *Metaphysics* 982b13–20 and 1026a18–19; *Physics* 198a30–32.

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3

Naturalism in Epistemology

Bastiaan C. van Fraassen

Naturalism is a coat of many colors. My interest here is in similarities and differences between current forms of Empiricism and Naturalism, concerning epistemology. Issues in metaphysics will all be left aside. Naturalism is often enough presented as a methodological assumption, that the best way to find out things is by empirical means. But since no one doubts this for things that can be found out by empirical means, we understand it at once as signaling that there is nothing else to be found out in any case. That links the methodological dictum to Naturalism understood as the ontological view that everything is physical, material, or within the domain of the natural sciences. Those terms have their own obscurities; it is hard to say what is definitely excluded. But it is typically understood to classify, as needing to be "reduced" or explained away, such items as consciousness, reference, goodness, beauty, purpose, functions, etc.[1]

None of that will be within my topic here; I will focus on traditional problems of epistemology. For my exemplars I will take just a few writers of the past half century or so, from the "1944 School of Naturalism," via Willard van Orman Quine, to contemporaries such as Stephen Leeds, Michael Devitt, and Penelope Maddy.

Naturalism's and Empiricism's common ground

For Naturalism and Empiricism I see three commonalities. I will briefly announce these here, and then explain each at some length. Once we reach the end of the common ground there will be a parting of ways.

First, in epistemology at least, Naturalism, like Empiricism, proclaims itself as a *stance* rather than a thesis or dogma. But it is not the same stance.

Second, contemporary Empiricism and Naturalism agree in their *anti-foundationalism* in epistemology. Both reject the idea that rational belief or knowledge must, or even can, rest on foundations that ensure truth (or even the likelihood of truth) that can be seen a priori to provide such a guarantee.

Third, Naturalism and Empiricism place the admiration of empirical science, as a paradigm of rational inquiry, at center stage. This generally includes a rejection of certain forms of metaphysics but not altogether the same forms. Nor is the centrality of science quite the same centrality.

The two stances are not the same stance. We need to ask how there can be a dialogue between two such stances so alike in some ways yet sharply opposed to each other in other ways.

Naturalism as stance

Philosophical positions can be roughly divided into those identifiable by a dogma or thesis about what there is, or what the world is like, and those not so identifiable. As I have argued elsewhere (2002), whatever Empiricism can be today, it cannot be of the former sort. It is a philosophical *stance*. By a stance I mean a position that consists in attitudes, commitments, and a characteristic approach to philosophical problems, possibly including or presupposing some beliefs as well, but not defined by a thesis or doctrine.

Naturalism is also in important instances presented as a philosophical stance. This can be illustrated with examples from the mid-twentieth century to the present.

Stance (1) 1944 and the turn against theory

The Columbia University–centered group of philosophers, somewhat mockingly called "the 1944 School of Naturalism," published a manifesto, *Naturalism and the Human Spirit* (Krikorian 1944; cf. Kim 2003, pp. 86–87). The contributors you may recognize include John Dewey, Sidney Hook, Ernest Nagel, and John Herman Randall.

In their essays we see two themes emphasized. The first is an explicit rejection of doctrine in favor of commitment, attitude, approach, and method. Thus John Herman Randall refers to Naturalism as "an attitude and temper," and says that it is "essentially a philosophic method and a program" (as cited in Kim 2003, p. 86). Sidney Hook calls Naturalism a "commitment to a procedure, not to a theory of metaphysics" (as cited in Kim 2003, p. 86). The second is that the paradigm example inspiring their philosophical approach comes from the sciences: the same procedures that characterize scientific inquiry are to be the core of a naturalistic philosophical approach. As Murphy in a review of Krikorian's (1944) *Naturalism and the Human Spirit* describes it:

> Starting from the acknowledged achievements of scientific inquiry so far, the "naturalists" intend to show that these same methods, or others essentially "continuous" with them, are adequate also to those aspects and dimensions of "the human spirit" which in the past have often been held on philosophical grounds to transcend the methods and aims of science. (Murphy 1945, p. 405)

And Murphy quotes Hook as saying that in regard to religious beliefs the naturalist only asks "to be given an opportunity to examine the evidence and to evaluate it by the same general canons which have led to the great triumphs of science." (Murphy 1945, p. 405)

Sidney Hook exemplifies both themes in his definition of Naturalism as "the wholehearted acceptance of scientific *method* as the only reliable way of reaching truths about the world of nature, society, and man" (as cited in Kim 2003, p. 87; my italics).

Thus we can see, in this mid-century manifesto, both the insistence that Naturalism is—to use my own term—a *stance*, and the special place that the sciences occupy for this stance. In Hook's words, but equally evident elsewhere in this volume, science provides a paradigm for rational methodologyto be taken as guide for what philosophy can be. The similarities in these two respects to the Logical Positivism and Logical Empiricism, which were at that time beginning to exert their special influence on American philosophy, spring to mind. Yet there are clues to how the differences would persist and in some respects intensify, in the style and the topics of concern of these writers, both far from what characterized those empiricist movements.

Stance (2) Penelope Maddy's version

A half century later we find Penelope Maddy's sustained advocacy for a Naturalism tailored for the present but continuing these pervasive themes of the 1944 School. Maddy's article "Naturalism: Friends and Foes" begins with the meta-philosophical view locating her Naturalism: "Naturalism, as I understand it, is not a doctrine, but an approach; not a set of answers, but a way of addressing questions. As such it can hardly be described in a list of theses" (2001, p. 37), thus characterizing it quite explicitly as a *philosophical stance*.

The agreement with the Empiricist stance I have advocated may not go much farther, but the similarities and parallels certainly do. Maddy sees her position as the current instance of a tradition identifiable through this common approach, while feeling free to reject much of what the earlier advocates (*proto-naturalists*, as she calls them) considered as essential.

Maddy goes on to display her Naturalism as a recurring revolt against transcendentalism, in a broad sense—against "two-level" philosophical views, citing Kant, Carnap, and me as the main examples. There is a similarity again in that Empiricism certainly stands out as a persistent revolt against realist metaphysics, against what Kant called "transcendental realism." The difference—but again, an interestingly symmetric one—is that in ontology, Maddy's Naturalism is best likened to transcendental realism (and she does sometimes speak of naturalistic metaphysics!), while she ranks Constructive Empiricism among the foes on the transcendentalist side.

These parallels could, despite the differences, inspire a certain natural sympathy. We are arraigned against forms of traditional metaphysics that purport to be a pursuit of truth by the same means as, but beyond, the empirical sciences. Yet we may already be detecting a tension in Maddy's position that, from my point of view, is irresolvable. Maddy dissolves or displaces a goodly array of traditional philosophical problems by concentrating our attention on what, in Moorean fashion, she can take as rock bottom common sense. This is the common sense of the educated layperson in our culture today, who "is a member of the scientific community [and] regards the methods of science as her own, as the best methods we have for finding out what the world is like" (Maddy 2001, p. 39).

This description does not by itself select philosophers, obviously; it applies to the Naturalistic philosopher only as one instance among the other intellectually nonalienated. Yet this passage began with the subject term "The naturalistic philosopher," and it continues with "until some new method is clearly proposed and defended, she is unimpressed by philosophical systems that place a second level of analysis above that of science" (Maddy 2001, p. 39). This is the Naturalistic *philosopher*, not simply a member of the scientific community.

The tension between the two identities, that of a member of the scientific community (as conceived by a Naturalist philosopher) and a Naturalistic philosopher proper, grows as we see Maddy turning to specific philosophical problems in her book *Second Philosophy: A Naturalistic Method* (2007).

I do not want to use the name "Second Philosopher" here. Use of this epithet in our discussion here would too easily invite a confusion of the character portrayed with the portrayer, and allow for too much slippage between the natural attitude and the philosophical orientation in which she reflects on that attitude. Taking a cue from her phrase "a Native of the contemporary scientific world view" (Maddy 2001, p. 48), let us call anyone who fits her description a *Naturalistic Native*—or where the context precludes ambiguity, just *Native*.

It is the Naturalistic philosopher who describes and defines the Native. Paraphrasing Umberto Eco, we could say that the Naturalistic philosopher holds up this Native as the "model reader" of the texts that the sciences offer the public (see, e.g., Eco 1996). We all know how to play the part of the model reader, whether of those texts or of historical novels, science fiction tales, or ideological manifestos. So even if we are not in the same role, we can understand what it is to play such a role.

But is this Native a second *philosopher*, or just any native of the scientific worldview? This is where that tension becomes apparent when we follow Maddy's engagement with philosophical issues. Obviously she does more here than describe a certain subclass of the world's population. She selects and identifies with this selection in a way quite different from her attitude toward the comparable elements of the artistic or religious or political or philosophical communities in which any of us may have grown up. Indeed, she portrays the Native as scientifically detached from all that, with its interest limited to the anthropology, psychology, and sociology of those areas. The Native "is equally

at home in anthropology, astronomy, biology, botany, chemistry, linguistics, neuroscience, physics, physiology, psychology, sociology" (Maddy 2007, p. 2).

A Renaissance woman, we'd almost say, but then the passage continues, "Her interest in other subjects, at least as far as we see her here, is limited to her pursuit of their anthropology, psychology, sociology, and so on" (Maddy 2007, p. 2).

The description that the philosopher gives of the Native depends crucially on the distinction between what is science—what are its methods, mentioned saliently earlier, and what is the content of scientific theories (so far) that currently counts as common knowledge—and what is not. Identifying that distinction is well known as the "demarcation problem," and Maddy has no more taste for it than do any of us these days. She agrees with Arthur Finethat "it is probably hopeless to search for necessary and sufficient conditions that separate science from the rest" but continues at once to face the obvious critique: "isn't Naturalism the view that scientific methods are the only legitimate source of evidence . . .; doesn't it take a viable demarcation even to state the position?" (Maddy 2001, p. 48).

In shunting this objection aside, Maddy reveals what seems to me her main philosophical tactic whenever it comes to a truly worrisome question for her position. She *retreats* dialectically from the level of Naturalistic philosopher to that of the ordinary Naturalist Native:

> My naturalist's methodology isn't "trust only science!"; her methodology just *is* a certain range of methods, which happen to be those we commonly regard as scientific. When asked why she believes in atoms, she says, "because of the experiments of Perrin" and such-like, not "because science says there are atoms and I believe the methods of science." So my naturalist applies no necessary and sufficient conditions; as a native of the contemporary scientific world view, she simply proceeds by the methods that strike her as justified. (Maddy 2001, p. 48)

She is quite right here about the Native, the Naturalist's model reader of today's science textbooks. But the random, arbitrarily selected Native is no philosopher, and cannot be confronted with philosophical questions, unlike the naturalistic philosopher who has to answer her peers. At this point the term "naturalist" has entered her vocabulary in studied ambiguity between "Naturalistic Native" and "Naturalistic philosopher."

What precisely is the rhetorical tactic put in play here? The Native's intellectual position, in general not even rising to the level of conscious attention, consists in identifying herself, or rather in maintaining her self-identification, as a member of this community, with its tradition and tacit agreements about what counts and what doesn't, who counts as a peer, and what counts as common knowledge. For the paradigmatic Naturalistic Native this is not the result of a deliberate choice, no alternatives being faced or considered or even brought to awareness, except perhaps those already classified as beyond the pale—astrology, creationism, augury, or haruspicy—and hence not present as "live." But as soon as this character confronts traditional philosophical positions, hence acts as philosopher, all of that does rise to the level of consciousness, while remaining somehow outside the scope of any critical scrutiny. Rhetoric aside, we will need to examine those reactions to traditional philosophy at a later point.

Anti-foundationalism: Epistemic life without security

We come now to the second feature that Empiricism and Naturalism have in common: anti-foundationalism in epistemology. This commonality is not surprising: both are heirs to the realization of the failures of traditional epistemology.

In our time, this anti-foundationalism was (re-)announced in a veritable clarion call by Willard van Orman Quine, who liked to cite Otto Neurath's (1932) striking metaphor of mariners at sea:[2] "Neurath has likened science to a boat which, if we are to rebuild it, we must rebuild plank by plank while staying afloat in it" (Quine 1960, p. 3) and "The Humean condition is the human condition" (Quine 1969, p. 72).

These quotes address the two following steps in foundationalist epistemology:

1. There is a secure basis for rational opinion, belief, and knowledge,
2. There is secure ampliative generation of justified, reliable opinion, starting from that basis.

The first step has seen such slogans as "nothing is probable unless something is certain" (C. I. Lewis); the second refers to Hume's critique and the traditional problem of Induction.

Quine targeted both in his critique, and announced the demise of both. I want to spell out this demise that happened not at the hands of Quine, nor due to the advent of naturalized epistemology. On the contrary, it is part of Naturalism's heritage from an earlier Empiricist avant-garde. Specifically, the thesis that rational opinion has or requires a foundation in certainty was subjected to a thorough and definitive critique by Hans Reichenbach in his *Experience and Prediction*, and in his debate with C. I. Lewis at the 1951 American Philosophical Association (Reichenbach 1938, 1952; Lewis 1952). But I shall concentrate here on the second, which is, even now, less uncontroversially to be discarded.

Induction

Is there a Problem of Induction? Hume stated it, didn't he? And many philosophers after him tried to solve it, to justify Induction, some of them announcing real success? But in order for there to be a problem about justifying something, that thing has to exist.

Let's just say that *induction*, written with a little "i," is something we all do: it includes any move to an opinion that goes beyond what we know, or have as evidence. Obviously we do that all the time. That includes hasty generalizations and superstitious credulousness, as well as responsible, careful drawing of conclusions. But what there was supposed to be a problem about I will call *Induction* spelled with a capital "I": a putative method that is

- genuinely ampliative, but
- humanly learnable,
- objective, and
- rationally compelling, while
- leading reliably to true conclusions.

Induction was certainly one of those many philosophical creatures baptized before it was born. In fact, if we insist on criteria of success, this one did not ever actually see the light of day.

What exactly did Hume call into question? The great scientific hero of the time, Isaac Newton, claimed to have established his System of the World by the Method of Induction, something that could not have been done by his rival Descartes's Method of Hypotheses. We have to suppose that Hume was

addressing this famous claim by his famous compatriot. Now, Newton did not say what Induction was. As spelled with a little "'i," Newton certainly engaged in induction; but the ways in which we go beyond our evidence in the opinions and beliefs we form range from the totally precipitate to the milquetoast chary. There is indeed a question about how to find a good balance between the two, just as there is for investing, courtship, diplomacy, and all the other practical arts. But that is not what Hume set about doing, and he was not concerned with straightening out our little "i" inductive moves. The idea Hume calls into question is that there really is a *method* for doing that in such a way as to get things right, *a method in the proper sense of the word*, a sort of general recipe that we finite beings can follow, and justifiably rely on.

Reichenbach's pragmatic turn, and its failure

To my mind the best chapter in this history of the Problem of Induction is Reichenbach's. First of all he diagnosed the British empiricists' debacle: they had accepted a criterion of adequacy imposed by their rationalist opponents, who asked for certainty (Reichenbach 1948). By such a criterion it would follow at the very least that, to be justified, an inductive method would have to be shown to lead to conclusions that can be said, with certainty, to be reliable, whenever applicable, and where the conditions of applicability are directly accessible to us. As Reichenbach pointed out, that is overreaching: the world might be such as to confute any method at all for predictions. That we cannot rule out beforehand. So he proposed a weaker criterion that would, if satisfied, be the best we could have, *and* would be good enough: "that the method should lead demonstrably to the truth, if any method could—that is, under conditions in which any method at all that could succeed" (as cited by van Fraassen 2008, p. 355).

Not only that, Reichenbach advanced the notorious straight rule, the very numerical induction rule that everyone had despised, as the basis of all good induction. And his justification was this: if in a series of instances there is a limiting relative frequency for a certain property, then the straight rule provides a series of estimates for prediction of what will come next that converge to that frequency. If there is no such limiting relative frequency then no method at all can concoct a reliable scheme for prediction.[3]

So wasn't this success? And wasn't it the very success that was wanted? Certainly not for this straight rule, baldly stated. If a weather forecaster always predicted rain for the next day, following that simple rule, he would, after the first thousand days or so, become extremely boring and uninformative. For the probability of rain he would announce for tomorrow would differ from today's by at most 1/1000, even if meanwhile the once-blue heavens had become dark with threatening clouds, or winter had turned to spring.

We would want his predictions to be sensitive to conditions that single out *sub-sequences* in the long series of days: summer days, or days following ten days of rain, or Federal Holidays, for example. To make this challenge realistic, these conditions must be ones that we and he could check on before the announcement. It is possible to make that requirement quite precise: the challenge is allowed to apply to all and only the finitely or recursively definable sub-sequences of future days, with the definition drawing only on what is settled beforehand.

But *demonstrably*, no rule can meet that standard, not even if we add "if any rule could." For any scenario at all there is a rule that does well in that way, but there is demonstrably no rule that does well under all scenarios. It was Reichenbach's own student Hilary Putnam who first introduced the argument to prove this; later it was extended and made precise by statisticians.[4]

So precisely what Reichenbach wanted to have met as criterion, although seemingly so modest, when refined by what interests us in practice, was something demonstrably impossible.

Quine was right when he concluded that "the Humean condition is the human condition" (1969, p. 72).

Anti-foundationalism reigns!

The reaction, common to Naturalists and Empiricists, is not skepticism! It is a resolute decision to adopt a starting point, without demanding a guarantee of reliability, let alone certainty, as an epistemic basis or epistemic policy for belief change.

Starting with G. E. Moore's "Proof of the External World," we have claimed the *right*, while remaining philosophers, to set doubts aside. In our epistemic life, there is no information-less prior, or rationally justified prior; we start

from where we are, from our own current opinion, always with an eye to improvement. As Quine writes, "There is no point of cosmic exile," and more specifically: "In studying and criticizing our cognitive procedures, we should use whatever powers and procedures we antecedently have and accept. There is no starting 'from scratch'" (Quine 1960, p. 275).

We are mariners at sea; we rely, of necessity, on the very ship whose rotten planks we tear out and replace. We rely, of necessity, on what this very ship affords that we are trying constantly to change, repair, improve. There is no secure prior to which we can withdraw, we have to start whatever we do from where we are.

For a striking example of this reaction in a contemporary Naturalist, consider Stephen Leeds.[5] Let's begin with a passage that manifestly expresses his Naturalist starting point, his intellectual home ground:

> Our methodology . . . is in a sense parochial: there could be people whose standards of simplicity or explanatory adequacy differ from our own (20th century, Western). From this, it is sometimes taken to follow that our methodology, as one among many, has a small chance of selecting true theories. But if we are interested in the chance that *this* methodology, described in all its particularity—i.e., *with our actual standards* of simplicity and explanatory adequacy—will select true theories, then it is irrelevant whether it is one among many. (Leeds 1994, p. 202; my italics)

I would submit that the "careful Realist," whose position Leeds articulates here, can make a relevant, pertinent case only if he speaks in the first person, to express his commitment to his own established position, that is, to express a stance. If he does so speak, I do think that he is in a reasonably, in fact rationally unassailable position. He has the right to speak from that point of view, and can do so, as it seems to me any philosopher has the right to do, until and unless he is toppled from that pedestal by acknowledged insuperable difficulties.

The support that Leeds details in third-person terms actually hides, and not very deeply, that essential first-person commitment. He emphasizes that the sort of Realist he is "will deny that we choose our methodology at random. Rather, he will tell a story according to which the world in some sense has guided us to the right standards of explanation and simplicity" (Leeds 1994, p. 204) This is a point about the *coherence* of the position: the commitment

to *this* methodology coheres with his beliefs about what the world is like, in respects affecting the possible success and failure of such a methodology:

> A careful Realist will present his story as a corollary to what he takes himself already to know about the world, and in particular, a consequence of the fact—remarkable, but solidly integral to the Realist's world picture—that some, although by no means all, of the most fundamental laws of Nature involve processes which appear also on the macroscopic level. . . . It is for this reason that, a Realist can argue, a creature who can find his way around the macroscopic world must inevitably be equipped with the "right" standards of simplicity and explanation. (Leeds 1994, p. 204)

That is right *precisely*, *but only,* for the Realist who accepts his science as true. There is no general theory *independent of what this careful Realist holds as true about the world* that will establish anything like that. It sounds objectively impersonal, but in fact continues the personal expression of his creed.

We can see the stance at work, quite clearly and quite effectively, in Leeds's writings about language, where he continues within the "Received View" when needed, but once again expresses a specific epistemic position within which all that is to be understood. Leeds is arguing here for a deflationist rather than correspondence notion of truth, and the way he locates that advocacy clearly reveals a committed starting point:

> For [my deflationist], the entire discussion of truth takes place against a background theory which our semantical discussions do not call into question: roughly the non-semantic part of a garden-variety Realist's theory about the world; it is against this background, in which the existence of people and ships and atoms is taken for granted, that we give our definitions of [. . .] relations between language and the world.' (Leeds 1995, p. 10)

This expresses Leeds's commitment, it expresses what is, as he says, "taken for granted," the framework within which he lives and moves and has his being.

This is not an objection but a clarification. A philosopher taking this position does not claim to have a basis shared with his opposition that would establish his position as correct, but rather the right to speak from his own conviction. He has taken his stand, and he has a right to do that. This is proper to a philosophical stance, provided of course it is acknowledged as a stance.

The parting of the ways: Naturalism takes off

To reject the presupposition of epistemic security is precisely to accept Neurath's metaphor, that there is ultimately no safe harbor, that we are like mariners who are at sea already, and must maintain and repair and improve our ship while at sea. This is a common starting point for both the Naturalist and the Empiricist in epistemology. Yet, with that same rationale, they take different positions. How we relate to the realization that, as Quine put it, "the Humean condition is the human condition," is quite different. The starting point is common, but there are forks in the road from there on. How does this happen?

A diagnosis of the forking path

The Naturalist lives on Neurath's ship too, but with such sanguine and irenic confidence that expressions of fear and trembling, or anguish for our epistemic responsibility, sound incongruous.

It is a truism, Neurath's truism, that we must start from where we are. What we *can* take as our starting point is not up to us, but the attitude toward our starting point is. So how could this attitude be different?

The first part of the explanation must be that, despite the truism's seeming implication that we have no choice (where we are is where we find ourselves; we are thrown into a world not of our making), there is, in fact, some choice involved after all. For, actually, the truism notwithstanding, if we ask where we are just now, we are confronted by a plethora of answers from our neighbors and peers, as well as by uncertainties and ambiguities in what we find ourselves tending to answer. I become aware of my mariner's fate: does where I am now, epistemically, include all that I learned in college? In high school? At my mother's knee? Actually, what was it that I learned, as opposed to "learned"? The word "learn" is factive, while "accept unthinkingly and docilely when told" is not!

So there is a selection involved in what we will *take to be* our actual starting point. But there is a much more important difference that lies precisely *in the attitude taken toward the starting point.*

Whatever it is that we take for granted, pro tem, at the outset, we still have two options about how to take it. First, we can take it as the basis we willingly

affirm as the rock to build on, our personal foundation, so to say. That is certainly one way to follow suit with respect to science, or whatever else we accorded that status, taking it as the received knowledge, or securely founded opinion, about what is the case. This we can indeed do without falling into the trap of dogmatic foundationalism. But alternatively, we can take it as our natural first target for analysis and reflection, for critique and interpretation, as precisely what we need to subject to critical reflection.

This is where we see the crux of this parting of the ways. As I see it, the Naturalist opts for the first option, while the Empiricist sees it as the philosopher's task to opt for the second. To make this plausible, let me try to articulate what I see as the Naturalist orientation, the one that I take to exhibit the first rather than the second form, illustrated by passages from some among the inheritors of Neurath's and Moore's laudable and courageous step. I mean certain latter-day Naturalists who take the *currently accepted scientific view* as part of our commonsensical knowledge: Willard van Orman Quine, Stephen Leeds, Michael Devitt, and Penelope Maddy.

The "fundamental naturalistic impulse"

While Moore wasn't much concerned with the sciences, these latter-day followers certainly are. Thus Quine characterizes his own position in a theory-adjusted echo of Moore by saying that he "begins his reasoning within the inherited world theory as a going concern" (Quine 1975, p. 72), that is, "from the point of view of our own science, which is the only point of view I can offer" (1981, p. 181), and as involving "the recognition that it is within science itself . . . that reality is to be identified and described" (1981, p. 21).

These passages are all cited by Penelope Maddy (2001, pp. 42–43) to illustrate what she classifies as the "fundamental naturalistic impulse." In a further example Quine adds what sounds like a surprising touch of nineteenth-century Idealism:

> Naturalism looks only to natural science, however fallible, for an account of what there is and what there is does. Science ventures its tentative answers in man-made concepts, perforce, couched in man-made language, but we can ask no better. . . . To ask what reality is *really* like, however, apart from human categories, is self-stultifying. (Quine 1992, p. 9)

What is startling in all this? The 1944 School of Naturalism looked to science for a paradigm of *approach* and *method*. What Quine looks to science for must presumably include method, but highlights here instead a received *content* of belief, as the voluntarily accepted starting point for all inquiry.

This starting point is indeed embraced (if not evidently in these passages) with expressions of attitudes contrary to dogmatism, of the right to stake claim to one's own orientation while avowing insight into one's own fallibility. That is right, but since the emphasis characterizing the earlier Naturalism was not beliefs but approach, not theories but method, not content but commitment, we can only be startled to find that after all, what is taken as basis is a content for belief.

We can continue this series of expressions of the attitude that marks the Naturalist philosopher's starting point, noting that the realism invoked is realism in the traditional sense of a thesis about what there is

> with respect to our *prima facie* commitment to [arguments supporting scientific realism]. . . . I know of no reason to think that any of this needs to be validated. (Leeds 2007, pp. 3–4)
>
> The defense of realism depends on distinguishing it from other doctrines and on choosing the right place to start the argument. And the defense of that choice depends on naturalism. (Devitt 1999, p. 90)
>
> [the view that there] is only one way of knowing, the empirical way that is the basis of science (whatever that way may be). (Devitt 2002, p. 31)

These philosophers are implicitly, but I think quite obviously, claiming the right to carry on philosophy in just this way, without needing to offer any special justification. They claim the right to start in the place where they are, epistemically speaking. I have no quarrel with this claim as such: *they do have the right. Let a thousand stances bloom!*

What does this "impulse" imply concerning what Naturalism is?

But now see how this thought is developed there. Suppose we begin our philosophical tasks always with common knowledge taken for granted, and take the accepted scientific theories of our days as common knowledge.

Nothing is known that is not understood; I imagine that too is a truism. That means then that science tells us the true, and *well-understood* story

about whatever it has brought into its own domain. It means, on the one hand, that anything less than belief in the contents of the current sciences is just skepticism. On the other hand, it has this curious effect: it implies that questions of interpretation have no place at all with respect to science. They can have a place only with respect to whatever is seen to lie outside that home turf. Indeed, we seem to have arrived at precisely the picture of the Naturalist Native that we saw earlier painted by Penelope Maddy.

Resistance to interpretation

When we see the Naturalistic philosopher characterizing and endorsing a particular intellectual posture, it involves not just resting in one's inherited beliefs, the content of the currently accepted scientific theories, but resting with confidence in one's understanding of those theories, and of what it is that constitutes scientific practice and activity that produces those theories.

"Understand" is also factive, in a sense: a claim to understand is a claim to "have it right" against implicitly or explicitly posed alternative interpretations. Or so we see it from the outside. For the native speaker, who lives in the language, the words are transparent. For the Naturalist Native, who lives in the language of science, the scientific terms and theoretical statements are transparent; no interpretation is needed or involved in their understanding. Once again, that is not how it looks to the philosopher outside this schema. From the outside it seems that an interpretation *is* involved: an implicit view of *what science*—this so valuable, important, widespread cultural phenomenon—*is*.

The interpretation involved on the part of the Naturalist is precisely the identification of that Native as the paradigm participant in the scientific enterprise. This brings with it a concordant view of the scientific enterprise, of its aim and structure. That Naturalist interpretation contrasts with competing answers to the parallel questions:

- What is an empirical scientist—more broadly, participant in the scientific enterprise—according to the Transcendentalist of today?
- What is an empirical scientist—more broadly, participant in the scientific enterprise—according to the Pragmatist of today?
- What is an empirical scientist—more broadly, participant in the scientific enterprise—according to the Empiricist of today?

The common core of these questions lies at the heart of the scientific realism debates of the 1980s, and of their current Nachlass.

Leeds: The scientific realism debates

Already in the passages I quoted earlier, we do not have to read deeply between the lines to see Leeds's philosophical conception of science. It suffices to recall "But if we are interested in the chance that *this* methodology . . . will select true theories," or to note the confident assertion that "a creature who can find his way around the macroscopic world must inevitably be equipped with the 'right' standards of simplicity and explanation" (Leeds 1994, p. 204).

A very traditional view in philosophy of science is that it is aims such as simplicity and explanation that drive the engine of science in order to produce true theories. For Leeds this pursuit of simplicity and explanation is a method for arriving at *belief*.

The one place where this view finds immediate application is the scientific realism debate, of which one strand is very saliently related to the failure of the idea of Induction. I mean of course such replacements for Induction as forms of, for example, Inference to the Best Explanation (IBE), which realists typically claim motorizes scientific inquiry. If that is so, they can, therefore, claim that the philosopher has a right, or more than a right, to use that as a vehicle for metaphysical inquiry as well. The claim of a right to such forms of inference with broad application, *without any need to justify them*, is clearly expressed by Stephen Leeds. Here is the complete relevant passage:

> With respect to our *prima facie* commitment to IBE—our willingness to use it, not only in cases involving observables, but also in cases in which we don't yet know whether the inferred entities or properties, if they exist, are observable or not, and finally in cases where they are known to be unobservable—I know of no reason to think that any of this needs to be validated. (Leeds 2007, pp. 3–4)

This is a laudable and courageous step, and must be seen as such even by someone not taking it. It is a step that goes beyond full and unqualified belief in the truth of currently accepted scientific theories, for it involves adherence to patterns of inference also outside of the empirical sciences.

But the most crucial point here is that it is *a step*, a choice, a choice of epistemic attitude not forced on us.[6]

The one thing that would not sit well with this dismissal of any challenge to justify an epistemic attitude would be *denial* in the sense of psychopathology: sustained oblivion of the choice involved. The good scientific Naturalist, I would say, must be one who is fully aware of the specter of a third-person view on his situation, in which that *choice* appears in a horizon of philosophical alternatives, and hence, is a choice for which responsibility is inescapable.

The Naturalistic Native, whom I will not identify with any naturalistic philosopher, does not meet this criterion, since interpretation of its home turf is just not on. Others can take such a view of its home turf ; these others are not Native there, if Maddy's picture of the two cultures is right.

Maddy: The Naturalistic native (continued)

Are Naturalistic philosophers, as opposed to the Naturalist Native, good scientific Naturalists, by that criterion?

As I see it, the fundamental Naturalistic impulse is not so much to take science for granted as to take for granted *a particular but unacknowledged philosophical view of what science is*. If that is precisely the case, the stance is not taken consciously and explicitly, but seen as unavoidable or inherent in what it is to be scientific at all. When Leeds says, for example, that there is no reason to think that reliance on IBE needs to be validated, it seems quite clearly to be based on a conviction that IBE is at the heart of scientific practice. But this conviction is part of a view, an *interpretation*, of science offered by philosophers in answer to the question *of what science is*. The neglect or refusal to see it that way is part of a strong resistance to interpretation that is actually more characteristic of Naturalism than any of its more public stances.

If this requisite sense of choice and responsibility were added as a leavening to current Naturalism, what would that do? Let's look at this again in the light of the views currently so ably and inventively defended by Penelope Maddy.

The task of actually laying out the presupposed interpretation of science is not so much lacking as deliberately eschewed in Maddy's presentation of Naturalism. Everywhere she claims the right, as philosopher, to retreat to the position of what she has presented as the Native, when a challenge comes up. When it comes to an answer to the question: What is science?" she simply quotes one of a much-quoted scientist's philosophical remarks: "Science [is] a method of finding things out. This method is based on the

principle that observation is the judge of whether something is so or not" (Feynman 1998, p. 15).

Any empiricist will say *Amen!* to this. But what does Feynman write, if read through Maddy's eyes? In her eyes this means, for example, that the observed outcomes of Perrin's experiments establish that atoms are real, and that not to believe this is to believe that science did not meet its own criteria of success in this case. But she does not make her interpretation explicit. That would require discussion of what the internal criteria of success in the empirical sciences are, or how they are related to the principle that observation is the judge, or to the status of theories involved in the assimilation of those experimental results. *That she owes us no such discussion is part of her philosophical stance.* Nevertheless, not giving it does not preclude her from advancing at every point not simply what currently accepted science generally says, but her interpretation of what it does.

The Naturalistic Native is, in my eyes, not simply someone who is scientific, but someone who has not faced interpretative questions about science. Dismissing such questions when they arise, the Native is credulous in a way that the practicing scientist most assuredly is not. What of the Naturalistic philosopher then? How is it possible for her, who is not just any sort of Native, to eschew exploration of interpretative alternatives, or even to recognize the need or relevance thereof?

Fundamentalism explained

The analogy that springs to the eye is to scriptural fundamentalism. It is not weird beliefs that characterize fundamentalists as such. There are fundamentalists in different religions—Christian, Muslim, Jewish, Hindu—and in ideologies—Leninist, Maoist. They are not singled out as such by the beliefs, of which they don't have many in common, nor by weirdness, which is after all in the eye of the beholder.

Rather, the common feature is this: there is for them no question to be considered about what the "Scriptures" mean, or what is the status of their content. The "Scriptures" mean what they say, all of "us" (in that cultural context) know ("know"?) what that is. Indeed, to the fundamentalist it is not just that the question of interpretation does not arise, the very idea is anathema, a heresy, a snare, and a delusion.

We can generalize on this, to diagnose fundamentalism wherever it occurs. On the fundamentalist's home ground there is a received story, let's call it the *Text*, and a received context of discussion, whose parameters are not to be altered. Anathema are precisely two activities of interpretation:

- of our relation to the *Text,*
- of the *Text*'s relation to the world.

Can we see this resistance to interpretation, the insistent assumption of total transparency, at work in the philosophy of science? To detect it, we must find it in the silences rather than in the explicit pronouncements of the participants.

For example, as we saw, Maddy cites Feynman's assertion that observation is the judge of whether something is so or not. But she sees no need to investigate the relation between *observation* and the final answer as to what is so or not. In just the same way the fundamentalist will insist that the judge of whether something is so or not is *sola scriptura*, without seeing the need to investigate the relation of the scriptural text (taken as a historical entity) to the conclusions drawn from it. *It means what it says*, and we, the cultural in-crowd, the only relevant community, all know what that is.

Similarly, the scriptural fundamentalist will understand the words in the Scriptures, regardless of where and when they were written, precisely as words uttered today in current discourse. When the book of Joshua says that the sun stood still, it means that the sun was moving and stopped moving. When the chemist Dumas wrote, in 1836, "Never in chemistry must we go beyond the realm of experiment" (as cited in Gardner 1979, p. 19), he meant just what a logical positivist or operationalist in the twentieth century would mean by that, and hence expressed a positivist philosophical prejudice.

The Native's uncomprehending stare

The Naturalist Native displays a disturbingly uncomprehending stare at the history of philosophy. Thus Maddy writes

1. about Kant:

   the Native "simply comes away unpersuaded" (Maddy 2007, p. 4); "is deaf and blind to the Kantian transcendental project in the first place" (Maddy 2007, p. 4);

2. about Carnap:

   "This sort of debate flies over [its] head. . . . What they are squabbling about escapes [it] in the first place" (Maddy 2007, pp. 80–81);
3. about Reichenbach:

   the Native remains "unimpressed by philosophical systems that place a second level of analysis above that of science" (Maddy 2001, p. 39);
4. about van Fraassen:

   "What we have here is yet another two-level view" (Maddy 2007, p. 308);
5. about Quine, who seems to have wavered in the Native's eyes:

   the Native "simply proceeds according to [its] own methods, unimpressed by proposed alternatives" (Maddy 2001, p. 55);
6. about Putnam:

   "None of these projects holds any appeal for [it], leaving [it] with no motivation to undertake these . . . inquiries" (Maddy 2007, p. 308);
7. about Richard Boyd's scientific realism:

   the Native "feels no need for this extra stamp of the foot" (Maddy 2007, p. 310).

How does this sit with what is done in foundational studies of physics? I am thinking of the various interpretations of quantum mechanics as answers to the question, how could the world possibly be as this theory says it is? Arthur Fine, expressing his position (NOA: the Natural Ontological Attitude) decries "the totally bizarre question of how the world could possibly be as the theory says."[7] Undoubtedly the Naturalist Native's reaction as well! But these interpretations of quantum mechanics were center stage in the philosophy of physics during much of the second half of the twentieth century. Theories are formulated; their formulation is investigated in the context of the alternatives that are open: for example, quantum mechanics is understood better now that we have seen Bohmian mechanics and the GRW theory. We could see all three, and compare them, discuss agreements and possible disparities in the empirical predictions, try to imagine at least thought experiments in which their differences would become manifest. We could much more clearly, because of the displayed contrasts, address the question what the world could possibly be like if it were as quantum mechanics says it is. What could I call this except a "level of analysis above that of science"?

The sliding back and forth between Naturalistic philosopher and Naturalistic Native, which served Maddy well in some of her defenses, is one she needs to perform in both directions, as the need arises, and so becomes a vacillation that renders her version of Naturalism unstable.

Interpretation

The important difference I see between such a position as Leeds's or Maddy's Naturalism on the one hand and Empiricism on the other, is with respect to interpretation. Both accept that the epistemic agent can only start from where he or she is, epistemically: with the presumed knowledge and accumulated opinion and belief at the point in question. Neither would take the agent to have a completely uncritical attitude toward this starting point taken as a whole: Neurath's mariners are after all repairing their ship at sea.

What we do not see in the more simplistic picture, however, is precisely how or in what way we can reflect on our own starting point and look at it with a critical eye, without for a moment disavowing it as (as yet, and still) our own. When it comes to what is earmarked as currently accepted science, the attitude of the Naturalist Native is that it is to be retained in full belief unless and until that earmark is removed by the relevant scientific community to which she or he defers, and whose pronouncements he or she takes as univocal.

One part of this is unexceptionable: we do not remove the earmark of accepted science on our own. It takes the authority of the scientific community to rule on what is and what is not currently accepted science, notwithstanding any vagueness in this ruling or in the constitution of that community. But does the "full belief" part prevent even comparisons with alternatives, or an assessment of science's own history, except as obviously mistaken, unless . . . *unless what*? All we can have is Whig history,[8] and a Whig assessment of anything comparable to what we have, *unless* it is possible to have as part of one's current epistemic stance a "bracketing" of what is currently accepted, even by oneself.

Such "bracketing" is a prerequisite for any interpretative approach to one's subject matter. The sailors on Neurath's ship cannot step back from it, in the sense of stepping back onto some other platform, such as a dry dock, from which to assess its problems. But this point misleads us if we forget that these sailors are persons, not the bare particulars of an impoverished descriptive

epistemology. They are able to evaluate their ship, and to plan different strategies and tactics for its repair and improvement, precisely because they can "bracket" some of the beliefs they started with, in which there was little doubt on any point of safety. If they could not, we would have to expect their discussions to take some such form as this:

A. Our safety does not require that we put on lifejackets before a storm comes up.
B. But what if a storm comes up very quickly, leaving no time to put on a life jacket after the storm strikes?
A. That will not happen, given that our safety does not require that we put on lifejackets before a storm comes up.

A's reply is a simple logical deduction from his current beliefs. His response is mechanical: facing a question, he offers the answer implied by what he believed prior to the questioning. But this inability to "step back" and evaluate his prior beliefs in the light of that question, which is at the same time a challenge, reveals him as not intellectually endowed to sail on Neurath's ship.

So I offer the Naturalist the dilemma: (a) to be in a position to share a point of view from which the sciences can be discussed, bracketing our beliefs about to what extent they are true, or (b) to be in no position even to survive as a Naturalistic Native in times of trouble. With Naturalism we can agree that we are on Neurath's ship, and can only ever go on from where we are, but we can add, and invite agreement, that an interpretative stance is open to us and indeed, is *needed* to understand our own situation properly.

Enter scientific realism

But now, consider what sorts of questions should—and if not resisted, will—arise in that "bracketed" context. They will most certainly include questions as to how to understand this scientific activity, of the criteria of success apparently applied in intra-community assessment of the work in that area, and therefore of its aim. Two rival proposals will quickly emerge, for they have been on the table in scientists' and philosophers' discussions for many centuries now. I mean of course the contrary views of the aim: is it truth or empirical adequacy? The aim of scientific activity is here meant to be identified in terms of what it has as its criterion of success in practice, and the question

is whether that aim is to achieve theories that are *true throughout*, or theories (whether or not entirely true) that meet the *empirical* criteria of success. Either view is logically compatible with full and unqualified belief in the science in question. The difference is only that on the first view, such full and unqualified belief just amounts to the opinion that this science has achieved its proper aim, while on the empiricist view such belief is to some extent supererogatory and involves belief that the theory has virtues beyond those required by the aim pursued.

At this point we can certainly see some differences among contemporary Naturalists. They do all seem to enter bodily into the realist/antirealist debate, but not in the same way. Is it possible for a Naturalist to couple a full and unqualified belief in a science with an attitude in which that belief is "bracketed," and to explore diverse rival interpretations of the science, of its content, its practices, its values and norms? Does Naturalism allow for this?

Penelope Maddy does not. [9] But in this respect neither Leeds nor Devitt seem to follow suit.

What is scientific realism for the Naturalist?

Maddy insists that the scientific realism debate can be disregarded as soon as one agrees to fully accept the currently accepted science. When Maddy's Naturalist Native turns to this debate, it must of course try to find the questions there that it can understand, just as it did when listening to Kant, Carnap, Reichenbach, and so on. Science is the Native's home ground, and questions of interpretation with respect to its home ground do not arise at all. So Maddy writes,

> What's at issue is whether or not the conclusions drawn by ordinary science methods should be regarded as definitive. So, for example, the scientific realist might argue that the best explanation for the success of the scientific enterprise is the assumption that the entities it describes really do exist, while the instrumentalist denies that this is so.
>
> [. . .] for present purposes, it's enough to note that this sort of debate flies over the head of the Second Philosopher. From her point of view, one either accepts her evidence or explains in her terms why it is inadequate, something

> neither the realist nor the instrumentalist purports to do. What they are squabbling about escapes her in the first place. (Maddy 2007, pp. 80–81)

The uncomprehending stare! As a result, the Naturalist Native sees just one possible intelligible motivation for the empiricist, and one possible factual question that could admit of evidence one way or another:

1. Motive: We should not believe more than what the evidence establishes.
2. Question: Are unobservable entities, such as atoms, real?

The two are then connected, in this putative empiricist's mind, by the apparent conviction that our evidence could establish that a theory that postulates unobservable entities is empirically adequate, but not that it is true.

This misconstrues both the empiricist position and the historical debate over the atomic theory.

Just note to begin: a claim of empirical adequacy for a theory *also* goes far beyond what our evidence could establish! For it entails that there are no phenomena anywhere in the universe or its history that contradict the theory. In that respect, truth and empirical adequacy are on a par. So how could this possibly be what is at issue between the empiricist and the realist in the scientific realism debates?

It seems that for Maddy's Native, or her Second Philosopher for that matter, the puzzle is how to find an intelligible (to her!) question that could possibly be at issue, and the only thus intelligible questions are scientific. Isn't this strangely reminiscent of some other famous episodes of philosophical puzzling over philosophical issues? Recall Sir Alfred Ayer's version of the uncomprehending stare directed to moral philosophy in the darkest days of Logical Positivism: "in so far as statements of value are significant, they are ordinary 'scientific' statements, and in so far as they are not scientific, they are not in the literal sense significant" (Ayer 1946, pp. 102–103).

So when Maddy looks into the scientific realism debates, the only question she can find that makes sense to the Naturalist Native is the factual question whether unobservable entities such as atoms or molecules exist. That is a question she takes to have been addressed, and answered, by scientists themselves. So what's the issue? Skepticism about whether scientists know their job! And yet, curiously, no party in the debate ever expressed such skepticism.

Looking into atomic theory

The development of the modern atomic theory in the nineteenth century was a scientifically much contested affair, but the experiments of Jean Perrin in 1905 ended the contest. I have discussed the Perrin episode (as recounted in Perrin 1910), and its handling in philosophical debate, at length elsewhere (van Fraassen 2009). Until recently, the story of Perrin appeared recurrently in the form of a bit of philosophical lore, as the story of the discovery that atoms and molecules are real. Characteristic of the way this lore was called into service for Naturalism is the presumption that *precisely what the scientific advance was* is crystal clear, transparent, and not in any way subject to interpretation.

Once this way of telling the story is accepted, the only question a philosopher may still address is how we can understand Perrin's work as legitimating the conclusion drawn from it, that is, the reality of atoms and molecules. This question was addressed by Wesley Salmon, Clark Glymour, and Peter Achinstein, with its presuppositions intact. Maddy does not depart from this standard line when she repeatedly returns to the Perrin episode. But the presupposed bit of conventional wisdom is an interpretation of this historical scientific episode. The interpretation is readily supported by quotes from Perrin and others. Such "text-proofing" (to use the Bible scholars' term) is a weak reed.

Now it seems to me that, *malgré eux*, the discussions by Salmon, Glymour, and Achinstein brought to light precisely the implications and puzzles that should lead us to an entirely different view of Perrin's achievement. If that is so, if there is a feasible interpretation at odds with the standard lore, then that shows that the standard lore was itself an interpretation as well. What passed for a literal reading of a transparent text was actually an unavowed interpretation.

Wesley Salmon (1978, pp. 698–699; 1984, p. 221; 1989, pp. 124–126) begins by noting that Perrin's measurements, in conjunction with his hypothesis that the Brownian particles mimic the dance of the molecules, establish a value for Avogadro's number. In the context of relevant background theory, as Perrin also noted, this is one of a number of different forms of measurement to establish Avogadro's number, and these measurements have concordant results, yielding the same value for that number. This Salmon saw as the clue to a compelling scientific realist argument.

From his teacher Hans Reichenbach Salmon takes the concept of "common cause" explanation, and its use in an inference "back" from effects to causes: if very similar effects have been produced by very different procedures, mutually independent, then it can be concluded that these effects all result from a common cause. In his famous paper "Why Ask 'Why'?" he first introduces the concept as follows:

> If two or more events of certain types occur at different places, but occur at the same time more frequently than is to be expected if they occurred independently, then this apparent coincidence is to be explained in terms of a common causal antecedent. If, for example, all of the electric lights in a particular area go out simultaneously, we do not believe that they just happened by chance to bum out at the same time. We attribute the coincidence to a common cause such as a blown fuse. (Salmon 1978, p. 691)

Experiments on various phenomena—including Brownian motion, alpha particle decay, X-ray diffraction, blackbody radiation, and electrochemical phenomena—all yield approximately the same value for Avogadro's number. That, Salmon claims, invites precisely the pattern of common cause reasoning. Describing the earlier noted concordance in measurement results, Salmon presents his common cause argument for scientific realism:

> The fundamental fact to which I wish to call attention is that the value of Avogadro's number ascertained from the analysis of Brownian motion agrees, within the limits of experimental error, with the value obtained by electrolytic measurement. Without a common causal antecedent, such agreement would constitute a remarkable coincidence. (Salmon 1978, p. 698)

In fact, Salmon has stretched the concept of common cause to the breaking point here. For that concept, which is technically explained in terms of probabilities of occurrence, pertains to relations of correlated simultaneous physical events to the same preceding event in their common history. Those measurement procedures are indeed physical occurrences, but their agreement is not a case of statistical correlation, nor is it due to a common causal antecedent—on the contrary, care is taken that those procedures are physically and causally independent of each other.

But, *lesson number one*, Salmon has put his finger on a crucial node in scientific practice: the concordance of the outcomes of physical procedures that

are classified, relative to theory, as measurements of the same quantity. That is an empirical criterion applied in the evaluation of theory: *concordance*.

Clark Glymour diagnosed the significance of Perrin's experiments differently, by presenting Perrin's reasoning as following his bootstrapping method:

> For example, some of Jean Perrin's tests of equations of the kinetic theory are exactly of the kind illustrated. Perrin had, for instance, to use one of the equations to be tested to determine a value for a constant (Avogadro's number) it contained. (Glymour 1975, p. 409, fn. 12)

As shown in further detail in Glymour's *Theory and Evidence* (1980, pp. 226–263), the development of the atomic theory in the nineteenth century bears out the bootstrapping view of evidential support. According to this view of how experimental results intertwine with hypotheses in a theory to confirm those hypotheses, measurement is characterized as yielding its results relative to a theory, but empirically significant. The bootstrap method involves a crucial demand for concordance of the sort noted earlier, while placing theoretical dependence center stage, and adding insurance against trivial or vacuous ways of satisfying that criterion. This theory-dependence of what counts as measurement is the second lesson.

Nevertheless, as Peter Achinstein pointed out, that Perrin's measurements can be understood in Glymour's way does not suffice to show how Perrin, or the scientific community, legitimately concluded that atoms are real (Achinstein 2001, pp. 252–253). That is so, but Glymour's point has a different, if implicit, significance. It is characteristic of the bootstrap method, as explication of how scientific theories earn their credentials, that greater support can accrue to logically stronger theories (cf. van Fraassen 1983). That is a puzzling point at first blush: if theory T2 is logically stronger than T1, that is, if T2 implies T1, then T2 *cannot* be more probable than T1, on any body of evidence. How can we solve this puzzle? Glymour's framework makes it very clear: the logically weaker theory may not offer the conceptual resources to design telling experiments that *could* offer support. And so we see in the development of the nineteenth century molecular-kinetic theory the amazing increase in confidence in that theory as more and more hypotheses are added to it, to increase its logical strength! For only thus do we arrive at the design of procedures that will count as measurements of the theoretical quantities involved—count relative to theory, that is! And Perrin took the last step in

this sequence of additions when he postulated that the observable Brownian motion mimics the unobservable molecular motion.

Finally Achinstein himself displays a different sort of causal reasoning to provide his own diagnosis (2001, pp. 254–258). His criticism of both Salmon's and Glymour's accounts is that they do not succeed in legitimizing the inference of the reality of molecules from Perrin's results. His own "legitimation" thereof posits certain premises involved in Perrin's reasoning, re-construed as explicitly probabilist. Surprisingly, the premises he lists include a prior probability of at least 0.5 for the atomic hypothesis, and the conclusion is only that it is "greater." If this was what established the reality of molecules, that increment in probability was the proverbial last straw to break skepticism's back! That does not seem plausible, so I would suggest the opposite conclusion: Achinstein is right that Perrin's achievement was *not* to raise the probability of the reality of molecules to the threshold-of-belief point, it was a different sort of achievement!

Putting these three conclusions together we arrive at this: the atomic theory introduced by Dalton early in the nineteenth century, and successively strengthened with additional hypotheses by Avogadro, Boltzmann, Maxwell, and, eventually Perrin, became strong enough to make *empirical grounding* for all its theoretical quantities possible. Empirical grounding is a requirement upon physical theory, within scientific practice, and it consists in this: that there should be procedures that would count as measurement for the theory, for the theoretical quantities, procedures that could, in principle, have outcomes contrary to the theory, but which do in fact have concordant outcomes to fix the values of those quantities. That is a mouthful; I have explored this demand elsewhere and will not elaborate further here (van Fraassen 2012 and forthcoming). This demand is closely related to Glymour's bootstrapping concept, and as Achinstein pointed out for that, compliance does not suffice to show that the theory is true. But it is the empirical criterion that must be met to satisfy the norms in force in scientific practice. There is no grist for the scientific realist's mill here; on the other hand, to grant this should offend no realist scruples either.

Conclusion

There are varieties of Naturalism, and not all Naturalists are to be shorn over the same comb. If we take Quine's "Epistemology Naturalized" as the

seminal paper for post–mid-twentieth century Naturalism, however, Penelope Maddy's dismissal of the "second level of analysis above that of science" should be taken as representative. Equally, it is as true heir of Quine that her "second philosopher" is "a Native of the contemporary scientific world view" who lives there as unselfconsciously as a fish in water or a bird in air.

This is in strong contrast to what I take to be at the heart of the Empiricist tradition, which involves, as a main philosophical task, achieving an understanding of the sciences as providing us with empirical knowledge. That is a task of *interpretation*, in which the empirical, conventional, and postulational elements in a theory are distinguished, and the practice of science is understood as an empirically driven, norm-governed activity. Quite the opposite of any attitude that deems interpretation, or analysis from a detached epistemological point of view, anathema.

Notes

1 Thanks to Angela Mendelovici for clarifying this in an early seminar discussion of the material.

2 Quine mentions Neurath's boat already in his 1950 article. The image became a mainstay of later writings and appears as an epigram at the start of *Word and Object* (Quine 1960).

3 Just to elaborate on the first part: the sequence of predictions will be well calibrated if the announced probabilities match the actual frequencies. That is not the success the straight rule can automatically claim. We must not confuse that with its obvious virtue: that since the straight rule's announced probability for the next item is always the actual frequency so far, that will match what has been found up to that point—and these probabilities will converge to the relative frequency in the entire series even if it is infinite.

4 Putnam was offering a critique of Carnap rather than of Reichenbach. For details on this critique and its continuation and completion by Putnam, Gaifman, Oakes, Dawid, and others see van Fraassen, 2000. Note that the argument applies to any computable forecast system, as defined by Dawid et al., and applies mutatis mutandis to any significant subclass we might want to consider (such as those basing their calculations on "acceptable" scientific theories plus data, etc.).

5 I classify Stephen Leeds as a Naturalist if only because of his able defense of a "Naturalistic Instrumentalism" in the philosophy of language (Leeds 1978). It has generally been taken as obvious that scientific realism requires, for its intelligibility, adherence to a correspondence theory of truth, or at least to the

idea that some notion of correspondence, subject of such a theory, is crucial to the discussion of what is and what is not true in science. But two prominent realists, David Lewis (2001) and Stephen Leeds (1994, 1995, 2007), argued strongly against this impression, insisting that the realist is barking up the wrong tree when thinking that the position requires anything going much beyond the Tarskian equivalences also beloved of the deflationist. I agree, and add that the same goes for the constructive empiricist. It is fascinating to reflect on (a) how comfortably Leeds can develop his views on scientific realism, given that as a convinced naturalist he takes his departure from its characteristic epistemic position, and (b) how in turn this Naturalism drives him inevitably to recast scientific realism as a *stance*, or I should say, as a *stance*-part of the naturalist stance.

6 Leeds at least is very aware of its being a choice of attitude: for example, he dismisses problems that have been pointed out with respect to IBE with the remark: "I pass over the Dutch book argument against having any non-Bayesian inductive policy. This would worry me if I were forced to bet with the devil; otherwise I cannot see that it proves anything at all" (Leeds 2007, p. 5, fn. 5).

7 Fine wrote this in correspondence, and gave me permission to quote it; it is very close to what he wrote in his 2001 article, (p. 118): "I favor a more open attitude toward science, one not committed to reconfiguring scientific practice to suit the needs of pre-set philosophical agendas—the attitude I call NOA. One irony of van Fraassen's global approach is that it merges with more or less traditional metaphysical projects when it asks, as van Fraassen does, after the 'content' of a theory or 'how the world could possibly be the way this theory says it is.'"

8 The term comes from Herbert Butterfield's influential book *The Whig Interpretation of History* (1931). Whig historiography evaluates the past as a progression toward the present, with errors, failures, and successes so classified by the historian's (community's) present values.

9 Note that Maddy (2007, pp. 308–311) contains a sustained argument to the effect that the sort of argument I gave here does not succeed in bringing the Naturalist to the table. I will leave the verdict to the reader.

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4

Philosophy and Scientism: What Cognitive Neuroscience Can, and What It Cannot, Explain

P. M. S. Hacker

Scientism

By "scientism" I understand attempts to extend the natural sciences beyond their proper sphere of explanatory competence, and the use of the methods of the natural sciences to explain phenomena that require other forms of explanation. The result of scientism is bogus explanation and intellectual deception.

The scientific achievements of the last four centuries transformed our understanding of the world. The natural sciences rid us of theologism—the extension of theology beyond *its* proper sphere of competence—and enabled us to understand the natural world in terms of robust, testable empirical theories and well-confirmed laws of nature. Our scientific understanding of the natural world is among the greatest intellectual achievements of mankind. (I come to praise Caesar, not to bury him!)

Nevertheless, every source of truth must, in the nature of things, also be a source of falsehood. Against falsehood the sciences themselves are well equipped to struggle by further experiment and more complex testable hypotheses. However, a source of truth may also be a source of intellectual illusions and conceptual confusions. Against these the natural sciences are much less well equipped to struggle. For one cannot dispel conceptual confusions by experimental methods. Scientism—the attempt to extend

science beyond its proper domain—is one great source of intellectual illusions and conceptual confusions. These can be eradicated only by the methods of conceptual analysis. Of course, whether such analysis is undertaken by scientists themselves (who are, on the whole, not trained for such tasks) or by philosophers (who ought to be well trained) does not matter—the issue is not a trade union dispute, but a dispute concerning different kinds of intellectual investigations, namely empirical and conceptual.

The scientistic attitude is exemplified in the following remarks—one by an eminent American philosopher and the other by a well-known British scientist. Wilfred Sellars, half a century ago, wrote: "In the dimension of describing and explaining the world, science is the measure of all things, of what is that it is, and of what is not that it is not" (1963, p. 173). Much more recently, at the turn of the millennium, Richard Dawkins wrote: "Science is the only way we know to understand the real world" (2000). Science, it is thought, gives us the best description of the world, and the methods of natural science give us the best methods of discovering truths about the world.

Realization of the explanatory limits of the natural sciences in the empirical domain dawned in the nineteenth century—the century of rising historicist self-consciousness.[1] For it became evident to many of the German hermeneutical thinkers in the early nineteenth century (such as Schleiermacher) and to German historicist thinkers of the late nineteenth century (such as Rickerts and Dilthey) that the methods of investigation into history, culture, and society are not those of the physical sciences. No amount of physics, chemistry, or biology can explain why Hannibal did not attack Rome itself after the battle of Cannae, why increasing wealth is accompanied by falling birthrates, let alone why Raphael painted the figure of Democritus in *The School of Athens* with boots on. But we do know the answers to these questions. Similarly, no amount of natural science can explain what the mind is, how the mind is related to the body, or—given that I have a mind, a body, and a soul—what it is that has all these things? But we do know the answers to these questions. In short, the natural sciences are the best way to explain the phenomena of physics, chemistry, and biology, but they are no way to explain the phenomena of history, political economy, culture, and society, let alone to answer the conceptual problems of philosophy. Nor do we turn to science to explain our humdrum daily activities—why you and I are here today, or why

you came by car rather than walked, or why you bought a widget at the shop. These lessons concerning the explanatory limits of the natural sciences have still not been adequately assimilated.

Scientists in different scientific disciplines have displayed the *hubris* of scientism. Their excesses are typically neither silly, nor easy to pinpoint. The scientism implicit in the explanatory claims made by competent scientists venturing into domains beyond the reach of natural science can be constrained and curtailed only by detailed and comprehensive argument—not by dogmatic rejection of the unfamiliar or unsavory. To show that a putative scientific explanation of a given phenomena is a form of scientism is not to show that the explanation is false. Indeed, to show that it is false would rightly be a spur to scientists to improve their endeavors and come up with a correct explanation. Rather it is to show that the putative explanation is, in one way or another, incoherent, that it is logically or conceptually out of order, that the form of explanation offered is inappropriate to the questions being investigated, or that the very question is unintelligible. *This* requires, above all, a *philosophical* investigation that will show that the bounds of sense are being transgressed.

The twenty-first century promises to be the century of neuroscience. It is constantly in the headlines of the mass media, and its putative results are a persistent subject of journalistic enthusiasm. Its true advances in treating diseases of the brain, in neuropharmacology or in neurosurgery are barely heard about in the hubbub surrounding cognitive neuroscience. For cognitive neuroscientists, equipped with brain scanners and intoxicated by fMRI, claim to have made the most amazing discoveries. They claim to have shown that the mind is the brain, or at least that the unconscious mind is the brain, that we have no free will, or at least that the window of opportunity for free action is a mere 150 milliseconds between the time we become aware that the brain has decided to act and the time at which the muscles contract to execute the intended act. They claim that the brain thinks, believes, decides, and that it interprets the information it receives; that the brain knows things, and stores what it knows—its memories—in the strengthened synaptic connections of its neurons in the hippocampus. As the neuromania grows, some eminent neuroscientists (e.g., Ramachandran) explain aesthetic appreciation with reference to innate neural responses to the hour-glass shapes of exaggerated female forms.

I shall not bother you with further folly. What I shall try to do in this chapter is to show what kinds of conceptual errors are evident in the writings of some leading neuroscientists. I shall show you that cognitive neuroscientists are commonly conceptually confused in subtle *but remediable* ways, that the questions they sometimes ask make no sense, that some of the experiments they conduct are incoherent, that the results of the experiments they conduct sometimes fail to show what they claim to have shown.[2]

My aim is not to denigrate neuroscience—rather it is to eradicate conceptual confusion from neuroscience. I believe that there is a very good chance that further developments in neuroscience in the coming decades will enable us to alleviate the scourge of Alzheimer's and senile dementia, to cure Parkinson's, and to treat schizophrenia. What cognitive neuroscience will not do is solve the problem of freedom of the will (it has been solved—we are free agents responsible for our actions), uncover the mysteries of consciousness (they are only pseudo-mysteries), or explain the relation between the mind and the body (there is no *relation*, since the mind is no *relatum*). These are philosophical, conceptual questions, not empirical ones. They can no more be solved by neuroscience than mathematical theorems can be proved by experiments in physics.

Brains, minds, and human beings

It is a striking fact in the history of ideas that we tend to model our understanding of ourselves on the model of our machines. Plato compared the soul to a two-horse chariot and driver—the horses representing the appetites and passions, and the charioteer reason. Descartes compared animals with the mechanical automata of his day, and argued that they were nonconscious, unthinking, biological automata. He thought that the human body too is such a machine, but it is one that is controlled by the mind. He compared the mind or soul with the controller of the flow of water into the wonderfully complex fountains of his times, for, he argued, it determines the flow of animal spirits (neural impulses) via the pineal gland along the nerves to the muscles. When I was a boy, the brain was compared to the central telephone exchange and the mind to the telephone operator at the exchange. Today the brain is compared

to a computer, and the mind is held to stand to the brain as the software of a computer to its hardware. The power of these similes is remarkable, and the ensuing confusions nontrivial.

Richard Dawkins has remarked that "each one of us is a machine, like an airline only much more complicated" (1986, p. 31). Colin Blakemore agreed: "We are machines, but machines so wonderfully designed that no one should count it an insult to be called such a machine" (1988, p. 270). Of course, it *is* an insult to be called a machine, since machines are not persons, they have no minds, they are under no obligations, and they have no rights. They cannot act for reasons (although there are reasons why they do what they do), they cannot feel emotions for reasons (since they cannot feel emotions at all), and they cannot reason. What they can do, since this is what we have built them for, is produce the results of calculations and computations without anyone or anything actually calculating or computing. We are not biological machines. Let me remind you of some salient conceptual differences. Machines are designed, animals and humans are not. Animals and humans have a good, and various complex conditions are conducive to their welfare. Machines have neither a good nor welfare, although some conditions may be good *for* a machine and others bad for it. But what is good for a machine (e.g., to be regularly oiled, or kept out of the rain) is *preventative*, that is, it prevents deterioration of the machine in wind and weather, or through overuse and neglect. But what is good for an animal or human is *also augmentative*—that is, conducive to an increase in its welfare and well-being. By contrast, what augments the performance of a machine does not increase its welfare, but its performance—machines have no welfare. Machines have controls, animals and humans do not—even though they may be controlled in various ways. The purpose of machines is the purpose for which they were designed. Animal species do not exist for a purpose. Animals and humans alike have purposes of their own, which are whatever ends they pursue—but machines do not have ends of their own. Animals, and humans, are alive; they have a life cycle consisting of characteristic phases. Machines are neither alive nor dead, and having no life, they have no life cycle. Animals and humans can be healthy, may prosper and flourish—or they may sicken, suffer poor health and illness. Machines can be none of these—they can only be in good or poor working order, break and break down—and unlike animals and humans, they do not

die. They are neither conscious nor unconscious—they are just machines! We need not worry, as science fiction writers and journalists do, that machines will take over the world—we need only worry that governments, with ever more sophisticated machines, may destroy civil liberties and lead us into a dystopic *Brave New World*.

We are, indeed, creatures with a mind. Inanimate objects and machines do not have a mind. What of other animals? Crabs and molluscs? Fish and fowl? Lions and elephants? Well, what sort of question is this—do nonhuman animals have minds? Certainly it is not akin to such questions as "Do any animals have a brain/body weight ratio larger than *n*?" or "Do animals suffer from Parkinson's disease?" Rather, it is more like "Do animals have voices?" That is, in order to answer such a question, we must first clarify for ourselves—who have both minds and voices—what the grounds and circumstances are that warrant ascribing a mind or a voice to a living being. Having a voice is straightforward enough. It is connected with the ability to sing, to accompany music with song, or to declaim, hence to control one's voice in speech, modulate it at will, raise and lower it, and so forth. But having a mind is a more complicated matter. The struggles of different cultures to articulate what is distinctive about *animate* life (breath, hot blood, *thumos*, *psuche*, *ruach*, *neshama*) became linked with equally difficult endeavors to articulate what is distinctive about human *mental* life (spirit, ghost, soul, mind). Philosophical reflection on human nature and human powers bifurcated very early in the history of our subject.

Aristotle conceived of the mind—the *rational psuche*—as the array of powers characteristic of rational beings, that is, the powers of the intellect and the rational will. To have a rational *psuche* is to have the power to think and act for reasons. Only if one has a rational *psuche* can one apprehend the truths of reason, such as the truths of mathematics and logic, or apprehend the truths of morality (i.e., the character of the virtues and the differences between right and wrong conduct). If we characterize the mind in analogous terms, we shall further realize that only creatures with a mind have the potentiality for self-consciousness, that is, for reflection on themselves, their deeds, feelings and thoughts, their attitudes, motivations and goals. Furthermore, only if one has a mind can one also have an autobiography or history—that is, a sense of one's past and of one's social identity. And it takes but little further reflection to

realize that *all* these powers are dependent upon mastery of a language. They are *essentially* powers of language-using creatures.

The alternative conception, given philosophical shape above all by Plato and Augustine, and in the early modern era by Descartes, is to conceive of the mind as a distinct entity or substance, connected to, but in principle detachable from the body. Descartes characterized the essence of the mind as thought or consciousness, and, surprisingly, denied that nonhuman animals are conscious beings. Rather, he thought, they are biological automata, lacking not only reason, but also consciousness and thought. Accordingly, for a human being to have a mind "intermingled" with the body, as Descartes put it, is to think, perceive, feel emotions and desires, and to cause one's body to move in different ways in pursuit of what one desires. Descartes was mistaken about his restriction of consciousness to humans, and his reasons (which I have not mentioned) were poor. However, his conception of the mind displaced the Aristotelian and medieval one, and dominated early modern and modern thought.

One of the distinctive features of Cartesian, dualist, thought is that psychological attributes are ascribed not to the body but to the mind. After all, Cartesians would argue, what would the body, which is, after all, merely extended matter in highly organized form, do with a mind? This seems as absurd as crediting an automaton with a mind—what could a piece of complicated clockwork *do* with a mind? No!—psychological attributes *are properties of the mind*. It is the mind, not the body, that is conscious or unconscious, that thinks and reasons, that feels sensations and perceives, that knows and remembers, that forms intentions and executes them. Or so it seemed.

The development of neuroscience in the eighteenth and nineteenth centuries occurred in the shadow of Descartes. Most of the great neuroscientists prior to the Second World War, including the Newton of neuroscience, Sir Charles Sherrington, were avowed Cartesian dualists. In mid-twentieth century, partly due to the psychological behaviorism that flourished in the interwar years, and partly due to the philosophical assault on dualism mounted by Wittgenstein and Ryle, the sciences of the brain turned away from dualism. Vaguely aware that there were objections to a duality of mind and body, they turned to a degenerate form of monism. The mind, they argued, is not an immaterial substance—it is a material part of the material substance that is the animal. *The*

mind is the brain, and mental attributes that we commonly ascribe to human beings and animals, as when we say that they perceive, want, feel pain or anger, expect or think something to be so, are ultimately to be explained by reference to the brain's feeling pain (in the body-image that is in it), the brain's perceiving, thinking, believing, wanting and deciding. What postwar neuroscientists in effect did was to retain the basic *structure* of the Cartesian conception of the mind, but replace the ethereal nonmatter of the mind with the gray glutinous matter of the brain. That is, the Cartesian conceptions (or misconceptions) of the relation between mind and body, between mental attributes and bodily behavior, between will and action, *were left intact*—although it was now the brain in the driving seat, rather than the immaterial mind. Let me illustrate this intellectual aberration with a few examples.

The Nobel Prize winner Francis Crick argued that "what you see is not what is *really* there; it is what your brain believes is there. . . . Your brain makes the best possible interpretation it can according to its previous experience and the limited and ambiguous information provided by your eyes . . . the brain combines the information . . . and settles on the most plausible interpretation of all these clues taken together . . ." (1995, pp. 30, 32fn.)—so *seeing* is explained by the brain's believing, interpreting, relying on previous experience, and making use of information! Another Nobel laureate, Gerald Edelman, argued that the brain categorizes, discriminates, and "recursively relates semantic to phonological sequences and then generates syntactic correspondences . . . by treating rules developing in memory as objects for conceptual manipulation" (1994, p. 130). Colin Blakemore, an eminent British neuroscientist and sometime head of the Medical Research Council, asserted that "the brain gets its knowledge by a process analogous to the inductive reasoning of the classical scientific method. Neurons present arguments to the brain based on specific features they detect, arguments on which the brain constructs its hypothesis of perception" (1977, p. 91). So the brain knows things, reasons inductively, and constructs hypotheses. Antonio Damasio holds that our brains decide, and Benjamin Libet concurs that the brain makes decisions and initiates action before the human being is even aware of it (Damasio 1996, p. 173; Libet 1985, p. 536).

What is, in effect, being done is to ascribe psychological attributes to the brain in order to explain the exercise, or faults in the exercise, of human

psychological powers and abilities. This is a widespread incoherence, rife in cognitive neuroscience. Why is this incoherent? Let us go back a stage. I said that it was characteristic of the Cartesian conception of the mind that mental attributes were ascribed to the mind (and indeed, that seemed a much preferable alternative to ascribing them to the body which, we are inclined to think in our dualist moments, the mind has). But that was already a dire error. It is not my mind that thinks, it is I. It is not my mind that makes up its mind—minds don't have a mind to make up. I make up my mind. And it is not my mind that has a pain in its body image, it is I who have a pain—and it is in my hand or foot, not in a body-image in my mind or in my brain.[3] To call something to mind is to remember—and it is people who remember things, not brains, and to have a thought cross one's mind is for something to occur to one, not for something to occur to one's mind. To be in two minds is to be undecided, and it is not one's mind that is undecided, it is oneself. To have half a mind to do something is to be tempted to do it—and it is not one's mind that is tempted. And so on. All ascription of psychological attributes to the mind is either metaphorical or a mere *façon de parler*. It is human beings, not some apparently ethereal immaterial substance allegedly attached to human bodies, who are the subject of mental attributes.

To ascribe the same set of psychological or mental attributes to the brain, rather than to the mind is not to remedy, but only to aggravate, the conceptual confusion. It is not false, but *incoherent*, to suppose that the brain is the subject of psychological attributes. There is no such thing as a brain's thinking, believing, knowing, and remembering, let alone deciding and intending. A fortiori, there can be no such thing as *explaining* why a human being malfunctions in various strange ways by reference to his brain's knowing or not knowing something. Why is this so? Why should neuroscientists not extend language in this manner? Let me explain.

We ascribe psychological attributes to a being on the ground of its behavior. We say that an animal is in pain if it displays characteristic pain behavior. We say of it that it wants something if it displays conative behavior, tries to get something. We say that it is pleased if it displays pleasure when it gets what it wants, as when a dog wags its tail in delight, or a cat purrs. Pain, desire, and pleasure are *not*, of course, mere behavior. But they are ascribed to others on the grounds of behavior. Such behavioral manifestations of the mental are

criteria, that is, *logically* good evidence, for the mental. One can be in pain and not show it, and one can exhibit pain-behavior without being in pain. But one cannot grasp the *concept* of pain and yet fail to recognize that pain-behavior is a ground for pain-ascription.

We say of human beings that they are thinking when they are evidently engaged in cogitative tasks—playing chess, doing a calculation, arguing a case. We say of humans that they intend doing something if we see them making appropriate plans and preparations, or when they announce their intentions; and we say of others that they hope or fear, love or hate, approve or disapprove, when they display such feelings in what they do and say in the circumstances of life. In all these cases, it is the creature as a whole that is the subject of psychological attributes, since it is the creature as a whole, not a part of it, such as the brain, that displays psychological attributes in behavior. Brains, by contrast, don't *behave*. They do not smile or weep, cry out or groan. There is no such thing as the brain making up its mind, since the brain does not have a mind—it is human beings who have minds. There is no such thing as a brain thinking or reasoning—there are no reflective or unreflective, thoughtful or thoughtless brains, only human beings, who may or may not reflect on what they are doing, have done or are going to do, who may behave considerately or inconsiderately, and may think before they act, or act spontaneously. These are conceptual truths. It is not because it is too difficult for brains to know or remember, think or believe, hope or fear—it is because it makes no sense to ascribe such predicates to brains. The error is one that Max Bennett and I have elsewhere termed a *mereological fallacy* (Bennett and Hacker 2003, chapter 3). Mereology is the logic of parts and wholes. The mereological fallacy in neuroscience is the incoherence of ascribing to a part of a creature properties that can intelligibly be ascribed only to the creature as a whole. A similar mereological fallacy applies to machines: airplanes cannot fly without engines—but it is still the airplane that flies, not its engines; antique bracket clocks would not keep time but for their fusées, but it is the clock, not the fusée, that keeps time. So too, animals (including human beings) without a brain could do none of the things they characteristically do—they would be dead. But it is the living creature as a whole that has and exercises psychological powers—not their brain. It is true that we could not walk without a brain, but we walk *with our legs*, not *with our brain*. We could not think and reason without a brain—but it is still we who think and reason, not our brain.

So, the mind is certainly not the brain—although we would have no mind had we no brain. What then is the mind, and how is it related to the body? The mind is not a thing of any kind. And precisely because it is not a kind of thing, it is not *related* to the body in any way, since it is not a *relatum*. Since the mind is not a kind of entity, the question "What is the mind?" is misleading. It is as misleading as the question "What is a sake?", that a child might ask when the mother says "Do it for my sake, dear." So what is the right question to ask in order to dispel our confusion? It is this: *What has to be true of a creature for it to be said to have a mind?* And the answer is clear: it has to be a creature with a distinctive range of abilities exhibited in its behavior (or that would be so exhibited were the creature not incapacitated). What kind of abilities? Not merely consciousness! Most animate beings, from fish to fowl, from lizards to lions, go through a waking and sleeping cycle, that is, are conscious or unconscious. Is that a reason for saying that they have a mind, can reason and think, have an inner life of reflection and imagination, grasp the difference between good and evil? All developed beings have perceptual powers, and so may become and then be conscious of something that catches and holds their attention. But that surely does not mean that we should extend the idiom of mind to fish and frogs, or even cats and dogs. We can already say everything we wish to say by characterizing them as sentient beings, that can feel pain, that want things and pursue goals, that can perceive their environment and become conscious of such features of it as catch and hold their attention. Animality is a necessary condition of having a mind, but not a sufficient one.

Many animals have the ability to recognize themselves in a mirror. Scientists have become very excited about this, assuming, quite wrongly, that to recognize oneself in a mirror is to be aware of oneself, and that to be aware of oneself is to enjoy a form of self-consciousness. They concluded, equally wrongly, that apes, elephants, dolphins, and crows are not merely conscious, but also self-conscious creatures. And certainly, if an animal has the capacity for self-consciousness, then it has a mind. But this is confused: to be able to recognize oneself in a mirror is not to be able to recognize one's self in a mirror. The ability to recognize one's reflection is not a form of self-consciousness. It is an ability that does not imply any conception of oneself whatsoever, let alone a conception of what or who one is. An animal can, after all, look at itself without a mirror. It does not "mistake itself" for another animal. And it does

not mistake its foot or paw for the foot or paw of another member of its kind. But that does not make it partially self-conscious. The fact that it can recognize more of its somatic features, for example, its face, in a mirror is not an increase in self-consciousness—merely an increase in intelligent responsiveness to what is visible.

So how are we to draw the line between animals that can be said to have a mind and those that cannot? Should we follow the Cartesians and the moderns and settle for consciousness? Or should we follow the Aristotelians and restrict ourselves to those beings in possession of the powers of intellect and rational will? I suggest that with two provisos, we should follow Aristotle. To have a mind is to have the distinctive powers of beings that can *reason*, and think, feel and act *for reasons*—and that are therefore *free agents*, *responsible for their deeds*. The two provisos are, first, that we should further recognize that such powers are distinctive only of developed language–using creatures, and, second, that mastery of the concepts expressed in a developed language suffuses the rest of our animal powers, of perception, affection, and action, since we cannot but subsume what we perceive, feel, and do under these concepts.

If having a mind is best conceived as possessing and exercising a range of language-dependent abilities of intellect and rational will, then it is obvious that the thought that the mind is the brain is incoherent. It is like saying that the horsepower of a car is the engine under its bonnet. Neither abilities nor their exercise in the stream of life are objects of any kind, let alone brains. Human beings have a mind—that is, they have a wide range of distinctive abilities of intellect and rational will. They would not have those abilities but for their brains—but it is they that have the abilities, not their brains. It is people, not brains, that can reason and think, know and believe things, want things and try to get them, act intentionally, and bear the responsibility for their actions. A human being, a person, is not a brain in a skull.

Misplaced experiments and misguided conclusions

The misunderstandings we are dealing with reach deep. So far I have discussed attempts by neuroscientists to resolve conceptual questions that can be solved only by means of careful conceptual analysis, not by scientific experimentation and empirical theorizing. This, I have emphasized, is a form of scientism. Now

I should like to turn to *conceptual confusions within empirical neuroscientific research.* Sometimes such confusions do not vitiate the research, but only the conclusions drawn from it. In some cases, what looks like an explanation is actually not one at all. In other cases, the misunderstandings render the research worthless, since the question being asked makes no sense. And sometimes the misconceived questions lead again to scientism—for example, to show by empirical means that we lack free will. Let me give you some examples.

Eric Kandel received a Nobel Prize for his work on memory. The research was remarkable. Kandel worked on a primitive marine creature known as *Aplysia*—a sea slug. Its brain has a mere 20,000 neurons, and the cells and axons are very much larger than ours. By touching its gills, a withdrawal reflex is activated. After a number of trials, the gill retraction becomes faster. Microscopic scrutiny of the neurons and axons of the sea slug showed changes at the synapses that were duly analyzed with wonderful precision. The conclusion drawn from this brilliant research was that memories are stored at synaptic connections. What seems questionable here is whether this research has anything whatsoever to do with memory. Kandel held that memory is *any change in the behavior of an organism consequent upon experience*. But that is a mistake, for it would mean that if an animal limps after being wounded, then it is remembering something, or that if one's reaction to having a puff of air blown into one's eyes leads to a faster blinking reaction, it is a manifestation of memory. That would be absurd. Memory is *knowledge retained*. A creature remembers something if it previously came to know it, knows it now, and knows it now *because* it knew it previously. Sea slugs are far too primitive a form of life for the idea of knowledge, let alone memory, to gain any grip. What was being tested was a form of reflex behavior—which is not a kind of remembering. Of course, this criticism does not imply that remembering does not involve strengthening synaptic connections in some parts of the cortex. Perhaps it does. But even if it does, that does not mean memories could intelligibly be said to be *stored* at synaptic connections. One can store knowledge in books, on filing cards, and on computers—but not in brains, and certainly not at synapses. To know something is to have a complex and diffuse ability, and since to remember is to retain that ability, it makes no sense to speak of *storing* an acquired ability to say, do, or think things, at a synaptic connection. Nor does it make sense to store *a memory*, that is, what we remember, namely, that things are or were

so, *in the brain*. What would it mean to say that one stores the fact *that the Second World War ended in 1945* in the brain? That we can remember the things we learn is doubtless due to changes in the brain, including changes at synapses—about which we know next to nothing. But it does not follow that we *store* what we remember in the brain.

Antonio Damasio argues that "when you and I see an object outside ourselves, we form comparable images in our respective brains" (1999, p. 320). Gerald Edelman (another Nobel laureate) holds that "as human beings, we experience primary consciousness as a 'picture' or a 'mental image' of ongoing categorized events" (1994, p. 119) and Francis Crick stated that "we can see how the visual parts of the brain take the picture (the visual field) apart, but we do not yet know how the brain puts it all together to provide our highly organized view of the world—that is, what we see" (1995, p. 22). Kandel and his colleague Robert Wurtz presented what they conceived to be a sore problem: "How is information carried by separate pathways brought together into a coherent visual image? . . . How does the brain construct a perceived world from sensory information and how does it bring it into consciousness?" (1995, p. 492). But this too is confused. We do not see images, unless we are looking at pictures in an art gallery or watching a film. We see objects and their properties, events, processes, and activities. We see the multicolored world around us, not images in our brains. There are no images in our brains, and if there were, we would not be able to see them, since we cannot see our brain. What is true is that the neural responses of the retinaengender neural impulses in the optic nerves that are duly transmitted to various parts of the "visual" striate cortex, different parts of which are sensitive to different features in the visual field, such as color, line orientation, and motion. We do not yet know how the excitation of the neurons in the columns of the "visual" striate cortex is linked to neural activities in other parts of the brain the upshot of which is that we see, and acquire knowledge of, the visual scene before us. But that is *misdescribed* as "the brain taking apart the visual field," and the consequent problem is *mischaracterized* by the question "How does the brain construct the perceived world from sensory information?" For the brain does no such thing. What it does is to *enable the animal to perceive* the world. It constructs nothing—neither an image nor a perceived world. It does not "put together information," either in the common or garden sense of "information" or in the

information-theoretic sense. The brain is not an information processor as is a computer, the sensory input to the brain is not information, and the "output" or upshot of the normal brain activity is not information, but the exercise of visual abilities, that is, *we see*, and may *thereby* acquire visual information. The brain and the perceptual organs endow an animal with the ability to see. The activity of the perceptual organ—in this case the eye—and the neural activity of the brain consequent upon it, is what is empirically requisite for the animal to exercise its sense of sight, which is indeed a source of knowledge and information.

Let me turn to a different array of problems discussed by contemporary cognitive neuroscientists—problems pertaining to voluntary and intentional action. It has been suggested by contemporary cognitive neuroscientists, both in Britain and the United States, that far from our being free agents, responsible for our deeds and free to lead our lives within the framework of social possibilities, it is our brain that decides things for us. The experiments, conducted by Benjamin Libet in the United States, were as follows. Subjects, with their brain activity being recorded, were told to move their index finger if they wanted to. If they moved it, they were asked to note on an electronic clock measuring milliseconds exactly when they felt the decision to move their finger, felt the desire or urge to move it, or felt the intention to do so. The time at which they reported such feelings turned out to be 350 ms after the motor center in the brain showed increased activity, and 150 ms before the muscles in the finger contracted. From this it was concluded that the brain decides to do things before the agent becomes aware of the decision. This in turn was supposed to show that the only room for free will was the 150 ms window of opportunity in which the person could stop the brain from acting.

This is a form of scientism, since it seeks to resolve a conceptual question—whether the will is free—by means of scientific experimentation. Moreover, it is confused. First, there is no such thing as *feeling* a decision or intention, and although one can feel desires, the desires one can feel are appetites, such as hunger and thirst, not wanting to move a finger. Second, there is no such thing as the brain's deciding anything. It is human beings who decide to act or refrain from acting. Third, there is no such thing as one's brain informing one that it has decided to move one's finger. Fourth, suppose one does decide to move one's finger and moves it. Can one pinpoint making this decision to

the millisecond? The notion of the time of a decision no more lends itself to such minute determination than the notion of the time of a smile or the time of a thought. Fifth, it is a mistake to suppose that a voluntary movement is a movement preceded by an act of volition or decision. After all, every word I am now uttering is uttered voluntarily and intentionally, but the words I utter are not preceded by separate acts of deciding to utter them. Sixth, it is equally a mistake to suppose that when one moves one's finger voluntarily, the movement of one's finger is *caused* by one's antecedent decision, volition, or felt urge to move it. Note that an urge can cause a bodily movement, but then the movement is *involuntary*—as when the urge to sneeze, cough, or vomit causes one to sneeze, cough, or vomit. So the presuppositions of the experiments involve an incoherent conception of voluntary action. Moreover, the range of experimentation is absurdly narrow. What happens to the motor centers of the brain when one decides now to move one's finger in 30 seconds? Or when one decides now to go for a walk tomorrow morning? Finally, determination of whether human beings are free and responsible agents is a conceptual matter that demands clarification of what *counts* as acting of one's own free will. And at least one thing is clear—to act freely is not to be caused to act by the occurrence of a volition or by a decision or an intention.

I will provide only one further example to demonstrate how conceptual confusion can lead to a misconceived description of a problematic and puzzling phenomenon to masquerade as an explanation of the phenomenon. Patients with severe epilepsy can sometimes be relieved of their symptoms by undergoing hemispherectomy, which involves cutting through the corpus callosum connecting the two hemispheres of the brain. The left hemisphere is dominant for language and speech, the right is largely causally responsible for visual motor tasks. Patients who had undergone hemispherectomy were exposed to a pair of pictures, for example, of a snow-covered house and drive on the left hand side of their visual field, thus affecting their right hemisphere, and of a chicken's claw on the right side of their visual field, thus affecting their left hemisphere. They were then asked to pick out two further pictures from a strip of eight that would severally match each of the pair of pictures before them. Patients then pointed with their left hand to a shovel to clear the snow-covered drive, and with their right hand to a chicken's head to match the chicken's claw. But when asked why they selected the shovel, instead of saying that it was needed to clear the drive,

they confabulated, saying that it was needed to clean out the chicken house. Gazzaniga and LeDoux explained this curious dissociative and confabulating phenomenon as follows: "The left hemisphere Interpreter has no knowledge of the snow scene seen by the right hemisphere," and it accordingly makes up a story to explain why the left hand, controlled by the right hemisphere, pointed at the shovel. From this and other experiments, Gazzaniga concluded that "mind left dealt with the world differently than mind right."

But this is a muddle. The hemispheres of the brain cannot be said to have minds. They cannot be said to see, to know, *or be ignorant* of anything whatsoever. They cannot be said to recognize anything, or to make judgments. So this explanation is literally unintelligible. The correct *description* of the phenomenon is that severing the corpus callosum deprives human beings of their capacity to exercise normally coordinated functions of seeing what is before them, matching objects to one another, and explaining the reasons for the matching. That is a consequence of the disconnection of neural fibers that are causally implicated in the exercise of their relevant capacities. In short, the transmission of neural signals across the corpus callosum is a necessary condition for a person to be able to carry out the recognitional, matching, and explaining tasks. But, of course, *we knew that from the experiment alone*. The explanation offered, concerning the one hemisphere seeing but being unable to inform the other hemisphere, and the other hemisphere making up stories to explain what the contralateral hemisphere is doing with its hand, does no more than redescribe the phenomenon in misplaced anthropomorphic terms.

Concluding remarks

The examples I have discussed are representative of a widespread style of thinking in cognitive neuroscience. What I believe they show is that successful cognitive neuroscience needs to pay far more attention to conceptual clarity than is customary. Poking the gills of sea slugs is not as such research on memory at all—which should have been evident by more careful reflection on the concept of memory. Equally, more attention has to be dedicated to the design of experiments. Wiggling one's finger is hardly a fruitful field for determination of brain activity associated with voluntary and intentional behavior. Research

on the neural correlates of consciousness had better be preceded by far more attention to the purely conceptual question of *what consciousness is*, that is, what it is to be conscious as opposed to unconscious or nonconscious, and what it is to be conscious *of* something, as well as what *sorts of things one can be conscious of*. Unless one is clear about that, as few neuroscientists are, serious research cannot even begin. And if one is clear about that, it may well be that the very idea of a neural correlate of consciousness is chimerical, since the phenomena of consciousness are too heterogeneous and categorially diverse to have *a* correlate.

Neuroscience may aspire to explain what neural structures endow human beings with their distinctive powers and what neuronal activities accompany the exercise of such powers. More importantly, we may reasonably hope that neuroscientists will discover what forms of neural malfunctioning are responsible for the various appalling and incapacitating diseases of the mind, and how we may strive to cure or treat them. These are noble goals. What neuroscience cannot do is resolve conceptual questions—such as "What is the mind?" or "Are we free?". Nor should it aspire to explain free human behavior—a task that belongs to hermeneutics, and methods of explanation that are essentially teleological, intentional, and social. Finally, it is surely absurd to try to explain morality and moral experience, or aesthetic value and aesthetic experience in neural terms. For the most that could be discovered are the conditions of the possibility of moral understanding and aesthetic experience. The differences between right and wrong, or between great works of art, good or weak works of art, kitsch, and chicanery are not to be found by studying the brain, but by reflecting on society and the good of man, by studying art and its history.[4]

Notes

1 Anticipated, to be sure, by Giambattista Vico's *New Science* (1725).

2 Together with my colleague Max Bennett, I have tried to do this in detail in two books: *Philosophical Foundations of Neuroscience* (2003) and *History of Cognitive Neuroscience* (2008).

3 Descartes held that pain is felt in the body-image in the mind. Cognitive neuroscientists and some philosophers, such as John Searle, hold that pain is felt

in the body-image in the brain. Both are mistaken. The so called body-image in the brain is no more than a one-to-one mapping of sensitive points on the surface of the body with corresponding cells in the post-central gyrus. It is not a map, but a mapping, and it is not body-image, since it is not an image. Moreover, one does not feel pain in the post-central gyrus, but wherever one is hurt.

4 I am grateful to my colleague Max Bennett for his guidance throughout the years we wrote together. Without it I should not have been able to engage in philosophical reflection upon cognitive neuroscience.

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5

The Implausibility of Physical Determinism

Richard Swinburne

Physical determinism is the doctrine that everything that happens (every event[1]) is fully caused by some earlier physical event in accord with some law of nature. By an earlier event "fully" causing a later event, I mean that the earlier event necessitates the later event, not merely makes it probable that it will occur; in philosophical terminology, the earlier event is a sufficient condition of the occurrence of the later event. If physical determinism were true, then every movement we make would have been predetermined by events long before we were born, in accord with physical laws in such a way that our intentions and beliefs have no influence on them.

But what is it for an event to be a "physical" event? This term may be understood in different ways, but I suggest that the most useful way to understand it is as a public event, one equally accessible to all investigators. A physical event is then contrasted with a "mental event" as one to which one person (the person whose event it is) has privileged access. More precisely, I suggest that we call an event a mental event if and only if whatever ways other people have of learning about whether it occurs, its subject necessarily has an additional way (by experiencing it) of discovering this. By this criterion, pains, itches, mental images, patterns of color in our visual field, and other sensations are all mental events. This is because whatever way you have of discovering whether I have a pain or not, I could also use. If you can find out to some extent whether I have a pain by observing my behavior, I could also observe my behavior (by watching a film of it); and if you could find out to some extent whether I have a pain by studying the patterns of my brain events, I could also study these patterns. But clearly I have a further way of learning

about my pain, by experiencing it, which is not available to you. By contrast, a physical event is one about which anyone can learn just as well as can anyone else at the same place and time. That there is a glass on this desk, that there are many people in this hall, are events whose occurrence we are all equally able to discover. It follows straightforwardly from this definition that no mental event is the same event as any physical event. Among physical events are brain events; that a certain neuron is firing (i.e., transmitting an electrochemical impulse) to a neighboring neuron is also something that we are all equally able to discover (at least, after having had a neuroscientific education).

According to the physical determinist, it is not only physical events but also mental events that are fully caused by physical events. And note that it is not merely sensations that are mental events, but also intentions (what we are trying to achieve), beliefs, thoughts (occurrent thoughts that "cross our minds"), and desires (to which we may or may not yield, to do actions of various kinds). Not all of these events are ones of which we are currently conscious, but what makes them mental events is that their owner can become conscious of them and others cannot. When we are actually conscious of a mental event, it is what I will call a conscious event. By an agent's "intention," I mean an intention *in* what the agent is doing (or trying to do), guiding (or trying to guide) his or her present movements (as opposed to an "intention" for future action—I shall not discuss intentions of this latter kind). My intention in moving my hand is to pick up the glass; my intention in coming to Provo was to give a lecture. And so on. It seems to us that our intentions (often together with our beliefs) make a difference to what we do.[2] My intention to come to Provo together with my belief that a particular aeroplane was going to Salt Lake City and that I would be met there and taken to Provo by car caused me to get on a particular aeroplane.[3] There are, however, also what are called "basic actions" caused—it seems to us—by our intentions alone. I just move my hand; I don't need a belief about how to move my hand—I just do it because I have the intention of doing it.

I have privileged access to my intentions and beliefs. As my hand moves, it may seem to you that I am moving it intentionally (i.e., the movement of my hand is caused by my intention to make it). But of course sometimes our nerves get out of control and hands move without our intending it; yet I can know better than you whether I am intending it to move. And clearly the same

nonbasic action such as getting on the aeroplane may be caused by different combinations of intention and belief—for example, getting on the aeroplane might have been caused by my intention to go to Beijing and my (false) belief that that particular aeroplane was going to Beijing. But I can know better than you which combination of intention and belief I have. Hence intentions and beliefs are mental events. While intentions (often together with beliefs) are the main influence on our bodies causing our public behavior, they are, in their turn, also—it seems to us—influenced by our desires and sensations; these latter sometimes affect our bodies directly—a sensation may cause me to scream, and a thought may cause me to blush. Our mental events, of course, affect our bodies by affecting our brains; so if my intention causes my hand to move, it does so by causing a brain event, which in turn causes the hand movement. So there is the normal picture: we have sensations, intentions, beliefs, thoughts, and desires—mental events to which we have privileged access, and these affect our bodies and, in particular, cause the bodily movements that constitute our public behavior.

The physical determinist, however, is committed to epiphenomenalism. This is the theory that although brain events cause mental events (including intentions), mental events never cause physical events. What happens when, for example, someone forms an intention to move his hand is that some brain event has caused that intention, and the same brain event causes the motion of his hand by a different route (i.e., not via their intention); that leads that person to believe that his intention caused the motion of the hand—but that belief, according to epiphenomenalism, is false. On that view, the only causes of brain events are other brain events. Such is the doctrine of physical determinism.

Now you will know that it is widely believed that quantum theory, one of the two great physical theories of the twentieth century, rules out physical determinism. It is a consequence of quantum theory that the position and momentum of a particle cannot be measured simultaneously to a joint accuracy of greater than $h/4\pi$, where h is Planck's constant ($\Delta p \cdot \Delta x \geq h/4\pi$). Since the future position of a particle is a function of its present position and momentum, there is therefore a limit to the accuracy with which the future position and momentum of the particle can be predicted. This result, the Heisenberg Uncertainty Principle, follows from the basic structure of quantum theory and is well confirmed independently. The unpredictability that affects

particle motion affects all other small-scale phenomena. Most physicists hold that the data explained by quantum theory could not be explained nearly as well by any deterministic theory, and so hold that on the small scale, nature is not merely not totally predictable, but not totally determined. There is, for example, only a physical probability (e.g., of ½ or 2/3) that a photon emitted from a source in the direction of a screen with two slits will pass through one slit rather than the other slit; or a physical probability of ½ that an atom of carbon-14 will decay within 5,600 years. Normally these small-scale physical probabilities average out on the larger scale; it is enormously probable that a proportion very close to ½ of the atoms in a large lump of carbon-14 atoms will decay within 5,600 years. A lump of carbon-14 is therefore what I may call an averaging system; the undetermined variations of the times at which individual atoms decay get averaged out, so that any individual variation makes virtually no difference to what happens on the large scale and so the large-scale behavior of the lump is almost deterministic. And so of course almost all large-scale systems are almost deterministic—which is why we can make successful predictions about the behavior of such systems. But there can be systems in which small-scale undetermined variations have large-scale effects; one could, for example, construct an atomic bomb such that whether it exploded depended on whether some atom in a block of atoms decayed within an hour or not. Such a system I will call a multiplying system.

For these reasons, it might seem that science constitutes no threat to our normal belief that our beliefs and intentions cause our behavior; for they could make a difference to what happens in our brains and so to the bodily movements we make within the range over which quantum theory does not dictate what will happen. Note however that if that was our only reason for believing that our intentions cause our bodily movements, our belief would depend on physicists not discovering some probable, more fundamental deterministic explanation of the data explained by the laws of quantum theory. In the 1950s, David Bohm proposed a "hidden variables" deterministic explanation that would explain why, for example, approximately 50% of carbon-14 atoms decay within 5,600 years. While it has seemed to most physicists that Bohm's particular theory was too "contrived" (complex) to be likely to be true, it might seem quite possible that one day some other physicist might provide some simpler deterministic theory that would yield testable predictions of the

behavior of subatomic particles. But of course at any given moment of time we must believe the theory best established at that time—and that today is quantum theory; that has the consequence that no small-scale events are fully determined.

Yet even given quantum theory, it is open to question whether the brain is an averaging system or a multiplying system. For only in the latter case could some small-scale brain event (e.g., one neuron transmitting enough of some chemical to the next neuron to cause it to fire) cause some bodily movement that would not otherwise have occurred. As far as neuroscientists are concerned, this is an unresolved issue, but they incline to the view that small-scale brain events would seldom, if ever, make any difference to which bodily movement we make. Anyway, they claim that other recent experimental work on whether intentions cause brain events gives strong support to epiphenomenalism, and, therefore also to the view that, even if there were a significant amount of indeterminism in the operation of the brain so that it was not a deterministic machine, our intentions would make no difference to what happens there, and therefore, no difference to our bodily movements. If there is not a sufficient cause of some brain event or sequence of brain events, then—they claim—mere chance determines what happens. So, they claim, even if physical determinism is false, its crucial consequence of importance for humans, epiphenomenalism, is true.

The recent neuroscientific work on whether intentions cause brain events (and thereby bodily movements) began with an original experiment in the 1980s by Benjamin Libet.[4] Participants in Libet's experiment sat at desks with a very fast clock in front of them. He attached to their skulls an apparatus that would measure any build-up of electrical potential at crucial points in their skulls. He then asked them to move their hands at a moment of their free choice at some time during the subsequent 20 seconds, and to note the time on the clock at which time they formed the intention to move their hands. Libet then discovered that there was always a build-up of electric potential 500 milliseconds before the time at which, according to each subject, they formed the intention to move their hand, which intention was followed 200 milliseconds later by the hand movement. What this showed, according to many neuroscientists, is that some brain event (which caused the build-up of the potential) occurring before the formation of the intention also caused a

sequence of brain events that both caused that intention and separately caused the brain event that caused the hand movement without the intention causing anything. So, it was claimed by many neuroscientists, the experiment showed that intentions don't cause the brain events that cause bodily movements.

But of course the experiment showed no such thing because it is perfectly compatible with the experimental results to suppose that the brain event that caused the build-up of the electric potential caused the subject to form an intention, and that the intention itself caused another brain event that caused the hand movement. However, these experiments might seem to have opened the way to the possibility of more sophisticated experiments showing what this particular experiment failed to show. For, it might be suggested, could not the neuroscientist discover the train of brain events from the original brain event that caused the electric potential to the motion of the hand, and then show that the hand would still move, whether or not the subject formed an intention to move it? That would surely show that the intention did not cause any later brain event that caused the hand movement, and so establish epiphenomenalism, would it not? Indeed in the past 20 years new techniques have been invented whereby neuroscientists can tell us which parts of the brain are most active when we are forming this or that intention. So could not such work finally establish epiphenomenalism?

Now I do not wish to deny that sometimes we make bodily movements very quickly in response to some sensory input, and only afterward do we attribute to ourselves some intention in making them (i.e., we rationalize our actions). But what is at stake is whether this is always the case—whether, for example, when you decided to attend a lecture and believed that the lecture would take place at a certain place, it was that intention and that belief that caused the motions of your legs which brought you there. For if epiphenomenalism were universally true, your intentions and beliefs would have had no effect whatsoever on your brain and so none on your bodily movements.

I shall now argue that no experimental results of the kind that I have described could possibly be generalized so as to establish universal epiphenomenalism. But for this purpose I need to establish a certain result, which I will then apply to our subject matter. So follow me during what might seem a digression. To establish a scientific theory, a scientist needs to show that it predicts certain events not predicted by rival theories, and that these events occurred. How

does a scientist know that certain events occurred? Either the scientist is currently observing them herself, or remembers having observed them, or has received reports from others that they have observed them. These three sources—experience, memory, and testimony—provide the evidence that the events predicted by a theory occurred; or rather, since all of these sources may mislead, it is *apparent* experience, memory, and testimony which provide the evidence that the events predicted by the theory occurred.

It is a fundamental epistemic principle which I call the Principle of Credulity, that what we seem to (i.e., apparently) experience is probably so—barring counterevidence; this applies to what we seem to observe in the public world, what we seem to experience as conscious events, and the logical consequences of inferences that we seem to "see." It follows from that also that what we seem to (i.e., apparently) remember having experienced, we probably did experience—barring counterevidence. And it is a second fundamental epistemic principle which I call the Principle of Testimony that what people seem to be (i.e., apparently are) telling us that they experienced, they probably did experience, again barring counterevidence.[5] Beliefs acquired by apparent experience, memory, and testimony are probably true—in the absence of counterevidence. Science relies on the applicability of these principles to determine what constitutes evidence. A scientist takes his (apparent) observations, experiences of conscious events, and calculations as probably correct, at least when he has looked carefully and checked. Almost all scientific knowledge relies on (apparent) memory (e.g., of the results of experiments or calculations only written up the following day). And for all science, we all rely most of the time on the (apparent) testimony (written and spoken) of observers to have had certain experiences (normally in the form of observations) and of theoreticians to have done certain calculations.

Beliefs acquired by apparent experience, memory, and testimony are however open to counterevidence or defeaters; that is, one can have evidence that, despite the apparent experiences (or whatever), the resulting beliefs are not to be trusted. One kind of defeater is an undermining defeater. If someone has an apparent experience of an event x that make it probable that an event y occurred, an undermining defeater is any evidence making it probable that x did not occur or is not good evidence for y. An undermining defeater to a belief in y doesn't show that y didn't occur, merely that our supposed evidence x is not

good evidence for supposing that *y* did occur. The evidence constituting the defeater must however itself be provided by apparent experience, memory, or testimony. In relying on evidence for the occurrence of some event provided by apparent experience, memory, or testimony, we assume that the occurrence of the event caused the apparent experience, testimony, or memory; and if we have evidence that it didn't, then that constitutes an undermining defeater for the belief that the event occurred. If for example I acquire a belief that my telephone is ringing because I apparently experience hearing my telephone ring, and then someone points out to me that the noise is coming from the television set where someone is depicted as hearing a telephone ring, that constitutes an undermining defeater for my belief resulting from my apparent experience. It doesn't show that my telephone was not ringing, but it does show that the noise was not evidence that it was, because the noise had a different cause.

A similar assumption of the existence of causal chains, although longer ones than for experience and ones involving different kinds of events, undergirds our beliefs in the deliverances of apparent memory and testimony. I trust my apparent memory of an event because I assume that that apparent memory was caused by a past apparent experience of the event recalled, and that the experience was caused by the event itself. Thus, in trusting my apparent memory that I visited London when I was five years old, I assume that the apparent memory was caused by my apparent experience of being in London when I was five years old, itself caused by my being in London then. Hence the generally accepted causal theory of memory. Any evidence that the apparent memory was planted in me by a hypnotist or a brain surgeon constitutes an undermining defeater for that apparent memory belief. So too does any evidence that my apparent memory was caused by my reading a fictional story written by my father in which I visited London.

Similarly in believing someone's apparent testimony to be experiencing or having experienced some event I assume that they say what they do because they are apparently experiencing or apparently remember having experienced that event and have the intention of telling me the truth about it; that is, their apparent experience or memory and their intention causes them to say what they do, "causes" in the sense of being a necessary part of the total cause. In the case of a past event I believe that their apparent memory was caused by

an apparent past experience of the event, the latter being caused by the event itself. So if I get evidence that the words coming out of some person's mouth were not caused by any intention of his (e.g., that the words were caused by a neurophysiologist stimulating that person's neurons to cause his mouth to make the sounds, or—as in fluent aphasia or Tourette's disease—were caused by a neural malfunction), that evidence constitutes an undermining defeater to believe in the truth of what that person seemed to be saying. In all of these cases, the counterevidence (in the form of an undermining defeater) must itself come (directly or indirectly) from apparent experience, memory, or testimony.[6]

In summary, then, science relies on an epistemic assumption (EA) that

1. a justified belief in a scientific theory requires a justified belief that the theory makes true predictions;
2. a justified belief that a theory makes true predictions is provided by and only by the evidence of apparent experience, memory, and testimony that the theory predicts certain events and that these events occurred;[7]
3. such justification is undermined by evidence that any apparent experience was not caused by the event apparently experienced, any apparent memory was not caused by an apparent experience of the event apparently remembered, or any apparent testimony was not caused by the testifier's intention to report his apparent experience or memory.

I hope that the few examples by which I have illustrated its application show the centrality of EA in our noetic framework. The fundamental criterion (FC) behind EA is that justified belief that some event occurred requires the assumption that that event is (privilegedly) accessible to or causes effects (privilegedly) accessible to the believer.[8]

Now I noted earlier that what a neuroscientist would need to do in order to show that intentions do not cause brain events is to produce a train of events from before the time when the subject formed the intention to make that movement, which would lead to that movement both when a subject formed the intention to make that movement and when he did not; that would show that the intention made no difference to whether the movement occurred. But in order to get such an experimental result, the neuroscientist would need to know just when subjects formed their intentions and when they didn't. How is he to know this? Presumably because subjects tell him when they form

intentions. But why should he believe them? Well clearly he believes them because he thinks that they have the intention of telling him the truth and the belief that they formed the intention when they said they did. I have however just pointed out that we believe what people tell us because we believe that they are saying what they do because their intentions and beliefs cause them to say what they do. But if universal epiphenomenalism were true, then the words that come out of subjects' mouths would not come out of their mouths because they intended that they should or because they had any beliefs—they would come out of their mouths solely because of some prior brain event. The situation would be just like that in fluid aphasia or Tourette's disease when words come out of a subject's mouth, but we know that the words are formed by some brain process that is not the result of any intention on the subject's part to utter those words. It is a necessary condition for a justified belief in what people tell us, that we believe that what they say is caused by their intention to tell us what they believe. So the situation is that epiphenomenalism could only be shown to be operative in a particular kind of situation, on the basis of results which could only be trusted on the assumption that epiphenomenalism is false for some other kind of situation (e.g., in respect of the words which come out of the subjects' mouths when they tell us about their intentions). So there is absolutely no possibility of universal epiphenomenalism being established by this route. Neither Libet's experiments nor any experiments of any similar kind could possibly establish that epiphenomenalism is universally true.

You might think that universal epiphenomenalism could be established in a different kind of way by showing that physical determinism governed all physical events without needing to discover whether and when any conscious events occur. I drew attention earlier to the possibility that quantum theory might one day be superseded by a successful determinist theory. Such a theory would have the consequence that every physical event (and that would include every brain event) had as a direct (i.e., immediately precedent), necessary, and sufficient condition of its occurrence another physical event. If that were shown, then of course no mental event could make any difference to what happened in the brain. But a similar problem arises with the justification of that all-embracing physical theory. In order to be justified in holding the theory we would need to know that it makes successful predictions. In this case we might discover that various events purportedly predicted by the theory did in

fact occur without assuming that any mental events cause physical events. For someone's observation of a physical event might cause a trace in their brain which in turn caused words to come out of his or her mouth reporting the occurrence without the causal chain proceeding through any mental event. Subjects would not report that they observed an event (an observation which would itself be a mental event) but only that the event happened. In a rather stretched sense of "testimony," they would be testifying to the occurrence of the event; and so FC would not have been violated. But in order for the occurrence of certain events to be evidence supporting a deterministic physical theory, we would need to have a justified belief that the theory predicted those events. To work out what a complicated theory predicts involves a long process of calculation. No individual scientist can hold such a calculation in his head; he will need to write down (or type into his computer) the stages of the calculation. And he will need to believe that he writes down each line of the calculation because he "sees" (i.e., has a convinced thought) that each line is entailed by the previous line. That is, he assumes that some mental event of his—a conscious belief—is causing (via some brain event) his hand to type what it does. And to assume that is again to assume that mental events cause brain events. And so again he can only justifiably believe that all physical events are caused by and only by physical events by making the assumption that sometimes they aren't. And it wouldn't make any difference if the scientist could do all his calculations mentally; for he would need to believe that the last line of his proof was caused by his beliefs about earlier stages of the proof. And we know that earlier experiences are recalled later because they have laid down "memory traces" in the brain—and so to rely on memory involves relying on conscious experiences causing brain events. And anyway every scientist rightly relies on other scientists to confirm his calculations; to do so is to rely on their testimony to their mental events, which again involves making the assumption that mental events cause physical events. Evidence for physical determinism can only be trusted on the assumption that physical determinism is false.

My argument so far does not prove that physical determinism or its consequence, epiphenomenalism, are false; but it does show that they couldn't be shown to be true. I must now take the argument a little further. To have a certain intention to do an act is to try to do that act; to try to do an act is to do what you believe will bring about the performance of that action.

So inevitably when we are acting we must believe that our intentions do affect our bodily movements; if we didn't believe that, we couldn't perform any intentional actions at all. And it seems to us very strongly for that reason that we do affect our bodily movements by our intentions, as when we try hard to do a difficult action, we inevitably believe strongly that we are exerting causal influence. I suggested earlier that it is a basic principle of epistemology, which I called the Principle of Credulity, that probably things are the way they seem to be—in the absence of counterevidence. If that's right, then (since mental events can only cause bodily movements by causing brain events) probably mental events do cause brain events—in the absence of counterevidence. What I claim to have shown is that there could not be counterevidence (i.e., defeaters) to universal epiphenomenalism. So probably in the respect that brain events are sometimes caused by mental events, the brain is not an ordinary physical system. And that should not be surprising because it is obvious that the brain is not an ordinary physical system in the different respect that brain events, unlike (as far as we can see) other physical events, often cause mental events, cause us to have pains, thoughts, and feelings, and so on. And since the brain is unlike any other physical system in this respect, it shouldn't be too surprising if it was unlike any other physical system in the respect that there is causation also in the reverse direction. And so even if quantum theory were superseded by some other theory of physics, that new theory of physics would also have to acknowledge that the brain is not governed entirely by deterministic physical laws.

So physical determinism is implausible. It remains probable that we do what we do because of our intentions and beliefs, that is because we mean to do it. It is still however possible, as far as anything which I have argued here shows, that our intentions are themselves fully caused by brain events and so psychophysical determinism (in contrast to physical determinism) is true. But a scientific theory consisting of deterministic laws relating mental events to brain events would be a theory of incredible complexity. For brain events consist of measurable quantities of a few properties (ultimately the mass, charge, spin, etc., of fundamental particles); whereas there are as many different intentions, beliefs, etc., as there are sentences in any language and they are not related to each other on any common measurable scale—my belief

that today is Thursday does not have twice as much of any quantity from that possessed by any belief there are many people in this room. While intentions, beliefs, etc., differ from each other in strength (some intentions are stronger than others; that is, we try harder to do some actions than other actions), there cannot be a public scale on which the strengths of such private events can be measured. So a psychophysical theory would need so many separate laws for each kind of belief, intention, etc., relating unmeasurable quantities, that no human scientist could ever construct it, let alone acquire enough experimental results to test it.

That in the very broadest outline are my reasons for supposing that these could not be a well-established deterministic psychophysical theory; and so by the principle of credulity we should again believe that things are in this respect also as they seem to us—that it is sometimes up to us what we decide to do, independently of any influences of nature or nurture to which we are subject. But for a more detailed argument to show the impossibility of constructing a deterministic psychophysical theory, I must refer you to my new book.[9] What I have shown in this chapter is that there could not be a well-justified theory of physical determinism. For to establish such a theory we would need evidence of a kind that could only be obtained on the assumption that that theory is false.

Notes

1 I understand "event" in the sense that both continuing states of affairs (e.g., my having some belief) and changes of state (e.g., my acquiring a new belief) are events.

2 In order not to have to defend in a short chapter a much-disputed philosophical theory, I am going to assume that when a person makes some movement intentionally, their intention causes that movement. This is not, I believe, strictly correct; it is the person (who is in philosophical terminology a "substance," or a thing), not some state of that person (in philosophical terminology often called an "event"), their intention that causes the movement. However, although this is an important point, nothing in the issue being discussed in the present chapter turns on it; so I shall assume that it is a person's intentions that often cause brain events and thereby cause their bodily movements.

3 See note 1.

4 For Libet's original experimental work, see Libet (2004). For a short account of Libet's experiments and subsequent experimental work on the neural mechanisms underlying intentional actions, see Haggard (2011).
5 For further justification of our use of the Principles of Credulity and Testimony, see Swinburne (2013), pp. 42–44 and pp. 54–58.
6 Part I of Robert Audi's textbook *Epistemology* (1998), entitled "Sources of Justification, Knowledge, and Truth," has chapters on "Perception," "Memory," "Consciousness," "Reason," and "Testimony." What I have called "experience" covers what he calls "perception," "consciousness," and "reason." Audi defends the causal nature of perception, memory, consciousness, and (in effect) testimony. Thus, "perception is a kind of causal relation" (his p. 28), "causal connections to the past are essential to genuine remembering"(p. 56), "the process by which introspection leads to introspective beliefs . . . is . . . causal" (p. 81), and "with testimonally grounded knowledge, as with memorial knowledge, there must be a certain kind of unbroken chain from the belief constituting that knowledge to a source of the knowledge in some other mode" (p. 137).
7 Clauses (1) and (2) of EA are subject to the qualification "unless the believer has a justified belief in a higher-level theory of which the belief referred to (in (1) or (2)) is a consequence." But these clauses make no difference to the cogency of the later arguments against the possibility of having a justified belief in any theory of which epiphenomenalism is a consequence because the same arguments would show the impossibility of having a justified belief in any such higher-level theory.
8 FC is subject to a qualification similar to that applicable to EA described in note 6, and—for the same reason as that given there—makes no difference to the cogency of later arguments.
9 Chapter 7 of Swinburne (2013).

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6

Scientism and the Humanities

Roger Scruton

In his well-known Rede lecture of 1959, scientist and novelist C. P. Snow argued that intellectual life had divided in to two, and that the two "cultures," as he called them, of science and the humanities were becoming increasingly opaque to each other. Snow also argued that the future of mankind depended upon bringing the two cultures together, so that science would once again be informed by an imaginative understanding of the human condition, and educated people supplied with real factual knowledge. It all seemed unexceptional and innocent, especially to Snow, who was an unexceptional and innocent person, an establishment figure who finished life as Lord Snow of Leicester, and who occupied blissful thrones in universities and advisory committees on both sides of the Atlantic. In 1962, however, literary critic F. R. Leavis published, in the London *Spectator*, an attack on Snow's lecture that was of extraordinary ferocity, contemptuously dismissing Snow as a talentless nonentity, and ridiculing the idea that science could be considered to be either a whole culture or a meaningful part of one.

Although the ensuing controversy generated more heat than light, it remains important for us today, for two reasons. It raised the question of the relation between the sciences and the humanities in a new way, and it made the idea of culture central to that question. In the German universities of the late nineteenth-century, there had been a public *Methodenstreit*—a contest of methods—between the sciences (the *Naturwissenschaften*) and the humanities (the *Geisteswissenschaften*), both claiming to be forms of objective knowledge. The influence here was the Kantian philosopher Wilhelm Dilthey and his theory of *Verstehen*, by which he meant the kind of understanding that grants

knowledge and membership of the human world, and which cannot be reduced to the forms of explanation found in the natural sciences. In the background was an earlier contest—the *Streit der Fakultäten*—that had been the subject of a famous discourse by Kant. But those metaphysical contests had passed over the horizon, and in any case become irrelevant when the German universities committed suicide during the 1930s. Unwittingly Snow had touched on what was to become for people of my generation the central questions faced by the modern university—the question of method in the humanities, and that of the nature and value of culture.

As the universities expanded, and as the hard sciences began to retreat to the margins of an educational system increasingly reluctant to demand too much of its students, the humanities moved to the center of the curriculum. First among them was English, a subject that established its place as a university degree in Britain only during the twentieth century, and largely as a result of the failed attempt by I. A. Richards to treat the study of literature as a branch of empirical psychology. History of art followed close on the heels of English, bringing with it the Hegelian forms of periodization that had been developed in the German universities. The rise of philosophy (still considered a branch of the "moral sciences" during my undergraduate days in Cambridge) laid the foundations for the continuing expansion of the curriculum into areas as diverse as classical civilization, film studies, and creative writing. The simultaneous growth of the social sciences to include not only anthropology (coupled to archaeology in the Cambridge of my youth) but also sociology, economics, political science, and the theory of education, meant that many of the new areas of study fell uneasily between arts and sciences and required extensive borrowings from both. Media studies, for instance: was it a branch of sociology, or a subsection of literary criticism? The habit very quickly arose during the 1960s and 1970s of throwing together clusters of disciplines from the social sciences and the humanities, in order to generate "studies" that would appeal to the increasingly unqualified intake of students, by conveying a spurious (and usually highly politicized) image of "relevance."

This fragmentation of the curriculum in the humanities adds a certain poignancy to the "two cultures" debate. Snow and Leavis wrote at a time when the university curriculum could be meaningfully divided into two, rather than indefinitely many, areas of study. In our present circumstances the impression

arises that, outside the hard sciences, just about anything goes, and that the humanities have neither a method nor a received body of knowledge, it being up to the professor to decide what to teach in his class. Occasional attempts to establish a canon of great books are quickly and easily overthrown, while the journals fill with articles devoted to fashionable nonsense of the kind exposed by Jean Bricmont and Alan Sokal (Sokal & Bricmont 1998).

An additional problem has been created by the growth of postgraduate schools in the humanities and social sciences. University departments and the people who teach in them are increasingly assessed—both for status and for funding—on their output of "research." The use of this word to describe what might formerly have gone under the name of "scholarship," naturally suggests an affinity between the humanities and the sciences, implying that both are engaged in *discovering* things, whether facts or theories, to be added in the same way to the store of human knowledge. Pressed to justify their existence, therefore, the humanities begin to look to the sciences to provide them with "research methods," and the promise of "results." To suggest that their principal concern is, as Leavis argued, the transmission of "culture" is to condemn them to second-class status. If all the humanities have to offer is "culture," then they can hardly have the same claim on the public purse as the sciences, which constantly add to the store of knowledge. Culture has no method, while research proceeds by conjecture and evidence. Moreover, while culture means the past, research means the future.

I have sketched a few of the factors that have contributed to today's "crisis in the humanities." Putting them together we can see why we are now a long way from the "two cultures" debate. Leavis defended the transmission of culture as an autonomous and precious part of the academic curriculum, and one that is not to be confused in any way with the growth of scientific knowledge. He did not speak of a method in the humanities, still less of "research." He was singling out certain subjects, and English in particular, as having an exemplary role in what we might call the spiritual development of the student. The true humane discipline, for Leavis, involved exploring cultural products as vessels of the human spirit, and absorbing from them the store of moral knowledge that they contain. It is fair to say that Leavis did not, in defending this position, give due weight to the many hard disciplines that might be involved—disciplines like languages, historiography, the study of ancient texts, and foreign cultures,

through which to attain not only a culture of one's own, but also the kind of critical distance that would serve best to cast a clear light on it. Nevertheless, he was surely right to suggest that both the process and the result of this study have little to do with the practices involved in the natural sciences.

However, once the defense of the humanities is made to rest on the "culture" that they transmit, all competition with the sciences is at an end. This is for two reasons: first, because culture, being without any obvious "cash value," cannot attract the funds that universities would need for the pursuit of it; second because, by defending the humanities in this way, you open them to subversion. Immediately it will be said that, by "culture," you mean only the particular culture of a particular society, rather than some universal value open to all mankind. The business of the academy, the critic will say, is not to perpetuate cultures but to hold them up to critical examination, to expose as ideology and manipulation what people have been content to receive as beauty and truth. In other words, you place the topic of the humanities within the field of social science, and then, as likely as not, the Marxist theory of ideology, or some feminist, poststructuralist or Foucauldian descendent of it, will be summoned in proof of the view that the precious achievements of our culture owe their status merely to the power that speaks through them, and that they are of no intrinsic worth. In this way the whole idea of culture as an autonomous sphere of moral knowledge, and one that requires learning, scholarship, and immersion to enhance and retain, is cast to the winds. The university, instead of transmitting culture, exists to deconstruct it, to remove its "aura," and to leave the student after 3 years of anxious dissipation, with the view that anything goes and nothing matters. That, it seems to me, is where we are today.

Or nearly so. Increasingly, we discover attempts to rectify the difficulty faced by humane studies by assimilating their subject matter to one or other of the sciences. Take the history of art, for instance. Generations of students have been drawn to this subject, in the hope of acquiring a knowledge of the masterpieces of the past. The field of study had developed in nineteenth-century German universities, under the influence of Burkhardt, Wölfflin, and others, to become a paradigm of objective study in the humanities. The Hegelian theory of the *Zeitgeist*, put to astute use by Wölfflin, divided everything into neatly circumscribed periods—Renaissance, Baroque, Rococo, neoclassical, and so

on—and the "comparative" method, in which images were shown side by side and their differences assigned to the distinguishing mental frameworks of their creators, proved endlessly fertile in critical judgments. Look at the works of Wittkower, Panofsky, Gombrich, and the other products of this school of thought, many of whom fled to safety from the Nazi destruction of the German universities, and you will surely conclude that there has never been a more creative and worthwhile addition to the curriculum in modern times. Yet the scholars are not satisfied. Is there any more "research" to be done on the art of Michelangelo, or the architecture of Palladio? Is there anything to be added to the study of the Gothic cathedral after Ruskin, von Simson, Pevsner, and Sedlmayer? And how do we confront the complaint that this whole subject seems to be focused on a narrow range of dead white European males, who spoke clearly for *their* times, but who have no great relevance to ours? All in all the subject of art history has been condemned by its own success to a corner of the academy, there to be starved of funds and graduate students—unless, that is, it can be endowed with some new field of "research."

Similar problems have bedeviled musicology and literary studies, and in each case the temptation has arisen to look for some branch of the natural sciences that could be applied to their subject matter, so as to rescue it from its methodless sterility. Two sciences in particular seem to fit the bill: evolutionary psychology and neuroscience. Both are sciences of the mind, and since culture is a mental arena, both sciences ought to be capable of making sense of it. The first treats mental states as adaptations and explains them in terms of the reproductive advantage that they confer on our genes; the second treats them as aspects of the nervous system, and explains them in terms of their cognitive function.

Over the past 20 years, therefore, we have witnessed a steady invasion of the humanities by evolutionary psychology and neuroscience. These invasions provide us with a useful illustration of the distinction between scientific and scientistic ways of thinking. The scientific thinker has a clear question, a body of data, and a theory that can be tested against it. The scientistic thinker borrows the apparatus of science, not to explain the phenomenon before him, but to create the *appearance* of a scientific question, the appearance of data, and the appearance of a method that will arrive at an answer. Structuralism in literary criticism, as exemplified by Barthes in *S/Z*, was scientistic in this sense, raising

questions that had only the appearance of science, and addressing them with theories that could not be refuted since they failed to make predictions (Barthes 1974). Barthes's flamboyant analysis of *La Peau de chagrin* by Balzac, casting about the technicalities of Saussurian linguistics, created a certain stir in its day, and was immediately taken up by literary critics hungry for a "method" that would deliver results. The results never came, and that particular episode is now more or less forgotten. Of more relevance to us today is the new "science" of "neuroaesthetics," introduced and championed by V. S. Ramachandran and Semir Zeki, which has its own journal and a growing pile of publications devoted to its results. Indeed, it is to neuroaesthetics that the art historian John Onians has turned, in his own attempt to recast his discipline as the science of "neuroarthistory" (Onians 2007).

Philosophers and critics have, over the centuries, asked themselves questions about the meaning of art, why it is so special, and why it affects us as it does. Their speculations have been subtle, difficult, and alert at every point to the human significance of the subject—what it means *to us*, who interpret the works of artists and take them to heart. This human significance is a cultural phenomenon, the kind of thing that the humanities emerged in order to study. And Ramachandran's first move is to present it already dressed in the science that he proposes to apply to it:

> The purpose of art, surely, is not merely to depict or represent reality—for that can be accomplished very easily with a camera—but to enhance, transcend, or indeed even to distort reality. . . . What the artist tries to do (either consciously or unconsciously) is to not only capture the essence of something but also to amplify it in order to more powerfully activate the same neural mechanisms that would be activated by the original object. (Ramachandran & Hirstein 1999, p. 16)

Having reduced the effect of art to one of perceptual distortion in this way, and dazzled the reader with a reference to "neural mechanisms," Ramachandran is able to summon a psychological theory—the theory of "peak shift"—to give a general explanation of "what art really is." The ensuing mish-mash of abridged and misapplied theories has been explored and exploded by John Hyman in an article to which I can add nothing.[1] What I wish to point out is the way in which science *intrudes* into Ramachandran's description of the subject. Instead of a careful and circumspect attempt to define a problem, there is a perfunctory

description of a few artistic phenomena, an unwarranted reference to a preferred explanation ("neural mechanisms"), and an anticipation of the result of applying it. This is the sure sign of scientism—that the science precedes the question, and is used to redefine it as a question that the science can solve. However, the difficulty of understanding art arises precisely because questions about the nature and meaning of art are not asking for an explanation of something, but for a description.

Why should there be such questions, and why is it that they lie, if they lie, beyond the reach of the empirical sciences? The simple answer is that they are questions that deal with the "spirit," with *Geist*, and therefore with phenomena that lie outside the purview of experimental methods. But it is not an answer that would satisfy people today. Putting it that way is likely to prompt a wry, skeptical smile. The "spirit" vanished with Kant's demolition of the Cartesian theory of the subject. Or if it didn't vanish then, how could it have survived the advances in cognitive science, in genetics and evolutionary psychology that have abolished the illusions through which religion governed our world? All that Ramachandran, Zeki and co. are doing, it might be said, is to replace the vague language in which the dispute between science and the *Geisteswissenschaften* was originally formulated with something more in keeping with our modern view of what we are.

The problem is that there is no agreed "modern view of what we are." As a conscious subject I have a point of view on the world. The world *seems* a certain way to me, and this "seeming" defines my unique perspective. Every self-conscious being has such a perspective, since that is what it means to be a subject, rather than an object. When I give a scientific account of the world, however, I am describing objects only. I am describing the way things are, and the causal laws that explain them. This description is given from no particular perspective. It does not contain words like "here," "now" and "I"; while it is meant to explain the way things seem, it does so by giving a theory of how they are. In short, the subject is in principle unobservable to science, not because it exists in another realm but because it is not part of the empirical world. It lies on the edge of things, like a horizon, and could never be grasped "from the other side," the side of subjectivity itself. Is it a real part of the real world? In one sense not. For if I look for it in the world of objects, I shall never find it. But without my nature as a subject nothing for me is real. If I am to care for my

world, then I must first care for this thing, without which I have no world—the perspective from which my world is seen. That is the message of art, or at least of the art that matters. And that is one reason why those humanities that have art and culture as their theme will never be reducible to natural sciences.

We understand others through the attitudes that Martin Buber summarized as relations between *Ich* and *Du*, but which would perhaps better be described as relations between I and I. We see each other I to I, and from this all judgment, all responsibility, all shame, pride, and fulfillment arise. This momentous fact about the human condition might be summarized in the word bequeathed to us by the Roman Law, and taken up by Boethius and Aquinas: we are persons, and personality is of our essence. Hence there are concepts that play an organizing role in our experience but which belong to no scientific theory because they divide the world into the wrong kinds of kind—concepts like those of ornament, melody, duty, freedom, which divide up the world in a way that no natural science could countenance. Science tells us a lot about the ordered sequences of pitched sounds, but it tells us nothing about melodies. A melody is not an acoustical object but a musical one. And musical objects belong to the purely intentional realm: they are sounds *heard under a musical description*. And that means they are sounds as *we self-conscious beings* hear them, under concepts that have no place in the science of sounds. The concept of the person is like the concept of a melody. It features in our way of perceiving and relating to each other; but it does not "carry over" into the science of what we are. This does not mean that there are no persons, but only that a scientific theory of persons will classify them with other things—for example, with apes or mammals—and will not be a scientific theory of every kind of person. (For example, it will not be a theory of corporate persons, of angels, or of God.)

In other words the kind to which we fundamentally belong is defined through a concept that does not feature in the science of our nature. Science sees us as objects rather than subjects, and its descriptions of our responses are not descriptions of what we feel. When we refer to the soul, we do not refer to some Cartesian substance, floating in the inner nowhere. We refer to the organizing principle of first-person awareness: the capacity for self-attribution, self-knowledge, and intersubjective response, which distinguishes ours from every other species, and which makes the life of a person into a thing worthwhile. That is what Aristotle and Aquinas meant by describing the

soul as the "form," and the body as the "matter" of the human being. All that I have added to their account is to define the "form" in terms of the organization exhibited by the "first-person singular"—in other words, a person.

Our behavior toward each other is mediated by the belief in freedom, in selfhood, in the knowledge that I am I and you are you and that each of us is a center of free and responsible thought and action. Out of these beliefs arises the whole world of interpersonal responses, and it is from the relations established between us that our own self-conception derives. It would seem to follow that we have an existential need to clarify the concepts of the self, of free choice, of responsibility and the rest, if we are to have a clear conception of what we are, and that no amount of neuroscience is going to help us to clarify those concepts. We live on the surface, and what matters to us are not the invisible nervous systems that explain how people work, but the visible appearances to which we respond when we respond to them as people. It is these appearances that we interpret, and upon our interpretation we build responses that must in turn be interpreted by those to whom they are directed. That is why culture is a distinct realm of human enquiry, which cannot be replaced by a natural science.

This returns me to the history of art and the study of pictures. It is fairly obvious that Titian's famous painting of the Venus of Urbino consists of a canvas, on which are distributed pigments. We could describe this distribution using geometrical coordinates in two-dimensional space, and so "pixelize" Titian's picture in a digital formula that enables a machine to reproduce it. This formula makes no mention of the woman, her servant, or the eyes that challenge and the hand that hides, and which caused Mark Twain to denounce this picture as obscene. Yet it contains all the information necessary to produce an image in which those things are seen by someone who has the capacity to understand pictures. We could imagine animals who were adept at recognizing the distribution of pixels, and could selectively respond to every difference between patterns of pigments that we see as pictures, but who did not see pictures. And of course, we are familiar with the digital programs that record, transmit, and present pixelated images, in machines that see nothing at all.

The normal response to that kind of example is to say that pictorial images are emergent features of the things that possess them. The picture of the young lady of Urbino is not something over and above the colored patches in which

we see it, but it is not reducible to them either: it is a feature of the painting that emerges, for those with the imaginative powers required to perceive it, but which can be produced simply by producing the right distribution of colored patches. Someone might be an expert in producing copies of the Titian, even though he is blind to its subject matter, and sees it only as a distribution of pigments on a canvas.

It is certain that there is much to be said about Titian's painting in terms of the disposition of pigments on a two-dimensional matrix. But it will not amount to an interpretation of the painting and will tell us nothing about its significance or value. For it will not mention the most important fact about the painting, which is what it is *about*. The word "about" is notorious: it is the very same word that causes all those difficulties about mental states and their analysis that were held, at one stage, to present an immovable obstacle to any simple physical analysis. Pictures have intentionality, just like beliefs and desires. And they can be compared in respect of their intentional content, not only with other paintings, but also with works of literature and works of music. There is an interesting question whether Titian's painting is to be understood as the expression of a domestic and nuptial sexuality, or whether the young lady is to be seen more as a courtesan than a wife. There is an interesting comparison to be made here with a painting that explicitly refers to Titian, the famous Olympia of Manet, in which the rough trade of the Boulevard is put in ironical relation to the soft downy embraces of Renaissance Venice.

Interpretation starts here, in comparative judgment, and it is hard to see what neuroscience can contribute to the result. Pictures are understood by finding their meaning and by assessing the place of that meaning in the life of the observer, and what it conveys about the human condition. And you are likely to gain insight into Manet's painting if you set it side by side with Daudet's *Sappho* and Zola's *Nana*. You understand what Manet is saying better if you see Titian's world ironically reflected in the forms and props that surround this hard-bitten courtesan.

Art critics have a discipline, and it is one that involves reasoning and judgment. It is not a science, and what it describes forms no part of the physical world, which does not contain Olympia or anything else that you see in Manet's painting. Yet someone who thought that art criticism is therefore deficient, and ought to be replaced by the study of pigments, is surely missing

the point. There are forms of human understanding that can be neither reduced to science nor enhanced by it.

Here is where the neurothugs step in, to declare that, of course, the science of pixels won't explain pictures, since pictures are in the eye of the beholder. But there is also such a thing as the MRI of the beholder, and this *does* contain the secret of the image in the frame. Since understanding a picture is a matter of seeing it in a certain way—in such a way as to grasp its visual aspect, and the meaning which that aspect has for beings like us—then we should be understanding the neural pathways involved in seeing aspects, and the connections which link those pathways to judgments of meaning.

But what, exactly, would such a study show? Suppose we have achieved a perfect decipherment of the pathways involved in seeing an aspect and in stabilizing it in the mind of the observer. This is not a judgment of criticism, and while it might enable us to predict that the normal observer will, on confronting Titian's picture, see a naked woman lying on a couch and looking at him, it will say nothing in answer to the critic who says—yes, but that is not *all* that there is, and indeed you must see that this woman is not naked at all, but rather unclothed, that her body has the texture and the movement of the clothes she has removed (cf Hollander 1975) and that those eyes do not look at you but look through you, dreaming of someone you are not. Critics don't tell us how we *do*, with normal equipment, see things, but how we *ought* to see them, and their account of the meaning of a picture is also a recommendation, which we obey by making a free choice of our own. All this by way of saying that the neuro*science* remains only a science: it cannot rise to the level of intentional understanding, where meaning is created through our own voluntary acts. Hence we should not be surprised at the dreariness of neuroaesthetics, and its inability to cast light on the nature or meaning of works of art.

Just as there is an understanding of art, which forms the domain of criticism, and which is a rational exercise with its own standards of validity, so there is an understanding of people, which forms the domain of interpersonal relations, and which is a rational exercise obedient to norms of its own. And just as it is an error to think you can replace art criticism with the neuroscience that allegedly explains the phenomena with which it deals, so too is it an error to think that you can replace interpersonal understanding with the neuroscience that allegedly explains our behavior. If you try to do this, you end up describing

the behavior in terms that remove it from the context that gives it sense—you become a reductionist, in other words, someone who fails to see that the most important features of the human condition are emergent features, which inhabit the surface of the world, but are invisible to those whose eyes are fixed on the depths. Human cultures are reflections on and in the surface of life, ways in which we understand the world of persons, and the moral framework within which persons live.

This returns me to the "two cultures" conflict. Leavis was attempting to defend a certain kind of culture—a "high culture" of critical reflection, embodied in art, literature, and (though he does not admit as much) religion. And he believed that the study of high culture was not only of vital importance to us, in our "technologico-Benthamite" world, where all matters of the heart were being steadily marginalized by instrumental ways of thinking. He also believed that this was the true task of the humanities in our university—to perpetuate cultural achievements, in times of adversity. Such a goal would only be undermined by the use of the methods of the empirical sciences. It cannot proceed by collecting evidence or proposing theories. Indeed, it is not a matter of *explanation* at all. Yet it is, for all that, objective, concerned with what Eliot called the "common pursuit of true judgment," and the establishment of authoritative paradigms against which we could judge our ways of falling short.

But this exalted idea of culture has undergone another scientistic assault—this time from Richard Dawkins and his concept of the "meme."[2] Natural selection can account for all the difficult facts presented by human culture, Dawkins suggests, once we see culture as developing according to the same principles as the individual organism. Just as the human organism is "a survival machine" developed by self-replicating genes, so is a culture a machine developed by self-replicating "memes"—mental entities that use the energies of human brains to multiply, in the way that viruses use the energies of cells. Like genes, memes need *Lebensraum*, and their success depends upon finding the ecological niche that enables them to generate more examples of their kind. That niche is the human brain.[3]

A meme is a self-replicating cultural entity that, lodging in the brain of a human being, uses that brain to reproduce itself—in the way that a catchy tune reproduces itself in hums and whistles, so spreading like an epidemic

through a human community, as did *La donna è mobile* the morning after the first performance of *Rigoletto.* Dawkins argues that ideas, beliefs, and attitudes are the conscious forms taken by self-replicating entities, which propagate themselves as diseases propagate themselves, by using the energies of their hosts:

> Just as genes propagate themselves in the gene pool by leaping from body to body via sperm or eggs, so memes propagate themselves in the meme pool by leaping from brain to brain by a process which, in the broad sense of the term, can be called imitation. (Dawkins 1976, p. 192)

Dennett (2006) adds that this process is not necessarily harmful: there are, among parasitical organisms, both symbionts, which coexist harmlessly with their hosts, and mutualists, which positively amplify the host's ability to survive and flourish in its environment.

Clearly, the theory of the "meme" threatens to debunk the whole realm of high culture, by making culture into a thing that survives in the human brain by its own efforts, as it were, and which has no more intrinsic significance than any other network of adaptations. To make the theory remotely plausible, however, Dawkins has to distinguish memes that belong to science from memes that are *merely* "cultural." Scientific memes are subject to effective policing by the brain that harbors them, which accepts ideas and theories only as part of its own truth-directed method. Merely cultural memes are outside the purview of scientific inference and can run riot, causing all kinds of cognitive and emotional disorders. They are subject to no external discipline, such as that contained in the concept of truth, but follow their own reproductive path, indifferent to the aims of the organism that they have invaded. That is how Dawkins views religion.

That idea is appealing at the level of metaphor, but what does it amount to in fact? From the point of view of memetics, absurd ideas have the same start in life as true theories, and assent is a retrospective honor bestowed on reproductive success. The only significant distinction to be made, when accounting for this success, is between memes that enhance the life of their hosts, and memes that either destroy that life or coexist symbiotically with it. It is one of the distinguishing characteristics of human beings, however, that they can distinguish an idea from the reality represented in it, can entertain propositions from which they withhold their assent, and move judge-like in

the realm of ideas, calling each before the bar of rational argument, accepting them and rejecting them regardless of the reproductive cost. And it is not only in science that this attitude of critical reflection is maintained. Matthew Arnold famously described culture as "a pursuit of our total perfection by means of getting to know, on all matters which most concern us, the best which has been thought and said in the world, and, through this knowledge, turning a stream of fresh and free thought upon our stock notions and habits" (*Culture and Anarchy* 1869, p. 6). Like so many people wedded to the nineteenth-century view of science, Dawkins overlooks the nineteenth-century reaction—which said "wait a minute: science is not the only way to pursue knowledge. There is moral knowledge too, which is the province of practical reason; there is emotional knowledge, which is the province of art, literature and music. And just possibly there is transcendental knowledge, which is the province of religion. Why privilege science, just because it sets out to *explain* the world? Why not give weight to the disciplines which *interpret* the world, and so help us to be at home in it?"

That reaction has lost none of its appeal. And it points to a fundamental weakness in "memetics." Even if there are units of memetic information, propagated from brain to brain by some replicating process, it is not they that come before the mind in conscious thinking. Memes stand to ideas as genes stand to organisms: if they exist at all (and no evidence has been given by Dawkins or anyone else for thinking that they do), then their sempiternal and purposeless reproduction is no concern of ours. Ideas, by contrast, form part of the conscious network of critical thinking. We assess them for their truth, their validity, their moral propriety, their elegance, completeness, and charm. We take them up and discard them, sometimes in the course of our search for truth and explanation, sometimes in our search for meaning and value. And both activities are essential to us. Although culture isn't science, it is nevertheless a conscious activity of the critical mind. Culture—both the high culture of art and music and the wider culture embodied in a moral and religious tradition—sorts ideas by their intrinsic qualities, helps us to feel at home in the world and to resonate to its personal significance. The theory of the meme neither denies that truth nor undermines the nineteenth-century view that culture, understood in that way, is as much an activity of the rational mind as science.

The concept of the meme belongs with other subversive concepts—Marx's "ideology," Freud's unconscious, Foucault's "discourse"—in being aimed at discrediting common prejudice. It seeks to expose illusions and to explain away our dreams. But it is itself a dream: a piece of ideology, accepted not for its truth but for the illusory power that it confers on the one who conjures with it. It has produced some striking arguments—not least those given by Dan Dennett in *Breaking the Spell*. But it possesses the very fault for which it purports to be a remedy: it is a spell with which the scientistic mind seeks to conjure away the things that pose a threat to it.

That, it seems to me, is how we should view scientism in general. It involves the use of scientific forms and categories in order to revert to another and prescientific way of thinking. It is a form of magic—a bid to reassemble the complex matter of human life, at the magician's command, in a shape over which he can exert control. It is an attempt to *subdue* what it does not understand. Surely both Snow and Leavis were right in thinking that human beings can do better than that—by the pursuit of genuine scientific explanation on the one hand, and by the study of high culture on the other. It is also why Leavis was right to argue that science is not another "culture," and that the knowledge delivered by the study of culture can never be expressed in scientific terms.

A culture does not comprise works of art only, nor is it directed solely to aesthetic interests. It is the sphere of *intrinsically interesting artifacts*, linked by the faculty of judgment to our aspirations and ideals. We appreciate works of art, arguments, works of history and literature, manners, dress, jokes, and forms of behavior. And all these things are shaped through judgment. But what kind of judgment, and to what does that judgment lead? This is the question that Leavis fudged since, for all his hostility to the "technologico-Benthamite" vision, he stopped short of a religious response to it. He wanted culture to exist and reproduce itself in the no-man's land of secular morality. It is my belief that culture in this sense, which stems from the "I" perspective that is the root of the human condition, points always toward the transcendental—the point on the edge of space and time, which is the subjectivity of the world (see Scruton 2014). And when we lose our sense of that thing, and of its eternal, tranquil watchfulness, all human life is cast into shadow. We approach the point at which even the St Matthew Passion and the Rondini Pietà have nothing more to say to us than a shark in formaldehyde. That is the direction we have taken.

But it is a direction of drift, a refusal of the posture that is inherent in the human condition, in which we strive to see events from outside and as a whole, as they are in the eyes of God.

Notes

1 Hyman (2010).
2 Expounded on in Dawkins (1976).
3 For various attempts to give a memetic theory of culture, see Aunger (2000). The theory of the meme is dismissively criticized by Stove (2006).

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7

Neuroethics

Kenneth F. Schaffner

Introduction: The emergence of "neuroethics" as a field

This chapter is an overview of the relatively new and still emerging field of neuroethics. After summarizing from my personal perspective how the subject developed against the backdrop of the Human Genome Project (HGP)'s subproject on ethical, legal, and social implications of genomics, I focus on three salient topics in neuroethics. My first topic considers what the initial implications of the neurosciences, and also genetics, are for freewill or free agency. This analysis serves as an introduction to a more "practical" topic involving legal implications of our understanding of free agency limitations, and the subject of what has been termed "dangerous brains." My third area of analysis looks at how neuroscience may assist our understanding of psychiatric disorders, especially schizophrenia, in connection with the ethics of research on this disorder's psychiatric and neuroscientific aspects.

HGP developed in the 1980s as molecular biology technology was increasingly able to rapidly sequence the series of nucleotides that constituted organisms', including human, DNA. The historical story is complex, involving university-based entrepreneurs, the National Academy of Sciences, two major federal research agencies (the Department of Energy or DOE and the National Institutes of Health or NIH), and the support of the political establishment. Bob Cook-Deegan's excellent book provides a detailed history of these developments (see Cook-Deegan 1994). After a series of turf wars and competing visions of what biological science should best be, the beginnings of this 3 billion dollar HGP began to roll. The first NIH director of the HGP

was James Watson, the codiscoverer of the double helix structure of DNA. Sensitive to the concerns that had been raised by genetics, including eugenics and its outrageous misapplications by the Nazis, the first act of Watson as director was to create the Ethical, Legal, and Social Implications (ELSI) Joint Working Group of NIH's new Human Genome Institute in 1989. Recognizing that money talks, the ELSI Working Group was allotted 3% of the genome funding, a small percentage in terms of scientific support, but a very large and significant amount for research in the "humanities" area. This source of funding encouraged many bioethicists and individuals interested in law, policy, and the social sciences to turn their attention to those implications of genetics. Over the course of some two decades, many ELSI projects advanced the discussion of generalized ethical implications of the genetic sciences.

Thus, it is understandable that by the early 2000s, other branches of science saw in the ELSI paradigm in genetics a model for extending ethical analysis to *their* subjects. The neurosciences, which was coming off the "Decade of the Brain" that ran from 1990 to 2000 and which produced many major advances, including attention to consciousness and issues of freewill, was an obvious disciplinary area in which to consider developing another ELSI program. If a specific point in time can be claimed to have initiated the more professional formation of the discipline of neuroethics, an excellent candidate is the 2002 meeting in San Francisco sponsored by the Dana Foundation, Stanford University, and the University of California at San Francisco. The conference proceedings were published as a book, which began with this summary statement:

> On May 13–14, 2002 more than 150 neuroscientists, bioethicists, doctors of psychiatry and psychology, philosophers, and professors of law and public policy came together in San Francisco, California to participate in a landmark conference *Neuroethics: Mapping the Field*. The conference organizers define "neuroethics" as the study of the ethical, legal and social questions that arise when scientific findings about the brain are carried into medical practice, legal interpretations and health and social policy. These findings are occurring in fields such as genetics, brain imaging, disease diagnosis and prediction. Neuroethics should examine how doctors, judges and lawyers, insurance executives and policy makers as well as the public will deal with them. (Marcus 2002, p. III)

The field has grown rapidly in the past 10+ years and has produced a number of general books and an exponentially growing mass of diverse articles covering the subject as defined in the quote earlier. Neuroethics contains not only some older ethical issues in a new guise, but also some issues especially specialized to neuroethics. It deals both with practical ethical issues and some advanced abstract philosophical issues.

Early in the development of the field, Adina Roskies proposed an influential distinction between the "ethics of neuroscience" and the "neuroscience of ethics" (Roskies 2002, p. 21). She suggested that the former actually can be divided into two subareas:

1. the ethical issues and considerations that should be raised in the course of designing and executing scientific studies and
2. evaluation of the ethical and social impact that the results of those studies might have, or ought to have, on existing social, ethical, and legal structures. (Roskies 2002, p. 21)

The second major area, the neuroscience of ethics, would investigate the "philosophical notions such as freewill, self-control, personality identity, and intention . . . from the perspective of brain function" (Roskies 2002, p. 22). In the sections of this chapter, both major and subareas named in these distinctions are addressed, but in a more integrated and interactive form. The separation of these two areas of neuroethics was critiqued by Northoff (2009). More recently, an article by Federico, Lombera, and Illes (2011) has also called into question the separability of Roskies' initial distinction, suggesting that the distinction needs to be rethought to establish the ethics of neuroscience as the "superordinate category" (p. 383), with more subtle subdistinctions reflecting various interests and approaches within the developing field.[1] Though thus far there is no NIH-supported specific ELSI project on neuroethics, the area has obtained reasonable funding, especially by the Dana Foundation, but also other agencies such as those thanked in the acknowledgments section (Illes and Sahakian 2011, p. XV).

On the practical side, technology is making it possible to monitor and manipulate the human mind via various neuroimaging and interventionist methods, which will be discussed in more detail later. These may involve a kind of "mind reading" as well as thought and memory controls. Brain enhancements

are also more likely soon. The neurological sciences increasingly allow us to understand "the human mind as part of the material world," as Farah writes introducing her collection of readings in neuroethics (Farah 2010, p. 1). This in turn raises perennial philosophical and even theological questions regarding the nature of the self, and of "freewill" or what some call "free agency" or "moral agency" to which I turn in the next section of this chapter. These difficult and abstract issues do resonate with the practical technological matters just mentioned. A good way to appreciate the current extent of work in the neuroethics field is to examine the comprehensive 2011 *Oxford Handbook of Neuroethics* (Federico et al. 2011) and also to log on to the two following websites: http://www.neuroethicssociety.org/teaching-neuroethics and http://neuroethics.ubc.ca/National_Core_for_Neuroethics/Home.html

"Freewill" and "free agency": Philosophy and neuroscience

An accessible, introductory article on freewill is available to point interested readers to some of the extraordinarily extensive philosophical literature on the topic in O'Connor (2011). O'Connor's article suggests we might think of freewill as involving dimensions of *agency, ownership,* and *deliberation* as key features. Generally we believe that actions follow desires coupled with motives and intentions. If the desires are considered compatible with the (suitably reflective) self, such as the desire to have a hearty breakfast, we take that desire and the subsequent enjoyable eating as "free." This view assumes the absence of a contrary desire to limit food intake due to health or appearance reasons. The presence of potential conflicting desires, and their joint presence in the "self," is the kind of puzzle that led philosopher Harry Frankfurt to propose his influential account of freewill. More on this view later.

As a standard background to this discussion, it will be useful to distinguish among three general views of the way that various forms of determinism—physical, biological/neurological, and genetic determinism—and approaches to determinism or nondeterminism, can be parsed in relation to the issue of freewill. Often this is done using a simple fourfold table that is depicted in

Table 7.1 The forms of determinism displayed in relation to beliefs about the universe and the existence of freewill

		Free Will	
		Free Will Does Not Exist	*Free Will Does Exists*
Determinism	*The Universe is Determined*	Hard Determinism	Compatibilism
	The Universe is Not Determined	Hard Incompatibilism	Libertarianism

Table 7.1, and the interrelations among these concepts and positions should be evident from that table. In some of the discussion later, terms such as "compatibilist" will be used in attributing positions to philosophers and scientists. Reading from Table 7.1, a compatibilist would both believe that the universe is determined, or more specifically that an individual's actions are determined (say by the neurons in the brain), *and* that freewill exists. How this *prima facie* contradiction might be resolved is the task of philosophical (and theological) analysis, some of which follows, such as the account advanced by Frankfurt.

Frankfurt's view draws a distinction between human and animal activity involved in desire satisfaction. The human has the capacity to entertain what Frankfurt calls "second-order desires" of the type that might be invoked by the actor deliberating about the hearty breakfast. When an individual acts so that both first- and second-order desires agree, that individual exhibits "free action." Another way of looking at that agreement is that the action reflects the individual's "true self," and that the second-order desire is one with which the individual *identifies* and is happy to *own*.

In Frankfurt's analysis there is a distinction between desires that are effective and noneffective. For first-order desires, a first-order effective desire is one that has motivated, is motivating, or will in the future motivate the desirer to act. A noneffective desire is one that does *not* result in an action. For second-order desires, there are two types: desires to have a first-order desire simpliciter versus a desire to have an *effective* first-order desire. It is the second sense, for an effective first-order desire, that Frankfurt also calls volition.

To make these distinctions more concrete, consider the case of the psychiatrist and the young anorexic patient that Cohen presents so vividly in his book developing and applying McHugh and Slavney's perspectives model from philosophy of psychiatry. (For the case, see Cohen [2003, chapter 3]; for the perspectives framework, see McHugh and Slavney [1998]; for a similar more philosophical example, see Fischer [1986, pp. 43–44].) The psychiatrist may weakly and hypothetically want to have the desire to be anorexic (he actually has no first-order desire *to be* anorexic) so as to empathetically treat his young patient (a second-order desire), but not wish that such a desire be effective (thus he has no second-order volition). The patient, with a new insight into her disorder, may wish not to starve herself but to eat normally (thus a second-order desire), but may not (yet anyway) be able to put that second-order desire into effect (and thus not have a second-order volition). In Frankfurt's view, this patient would be an "unwilling" anorexic and also lack freedom of the will. "It is in securing the conformity of [her] will to [her] second- order volitions, then, that a person exercises freedom of the will" (Frankfurt 1968, p. 331). Frankfurt also proposes that a strong "identification" with a first-order desire can be sufficient for a freewill.

Frankfurt takes "freedom of action" to be a weaker notion in which one is free to act on a first-order desire, and distinguishes that notion from the stronger "freedom of the will," which requires freedom of action *and* the capacity to have the type of will that one wants to have. Moral responsibility is a bit tricky for Frankfurt, but essentially if the agent has volition to act, even though the agent could not have done otherwise, the agent is responsible for the act. Thus, a *willing* drug addict is responsible for taking a drug, even though addicted.

Finally we should note that Frankfurt is what we termed earlier a compatibilist. He writes:

> My conception of the freedom of the will appears to be neutral with regard to the problem of determinism. It seems conceivable that it should be causally determined that a person is free to want what he wants to want. If this is conceivable, then it might be causally determined that a person enjoys a freewill. (Frankfurt 1968, p. 336)

This general view of Frankfurt's is what some have called a "hierarchical" approach to the freewill problem, and it has been criticized in the philosophical literature. One critique, by Gary Watson, not only raises the question of a

further regress (to a third level of desires), but also develops an alternative view regarding different sources of desires (rather than levels), such as desires deriving from the passions and desires based on reason (and even acculturation) (Watson 1975).

Though the Frankfurt view has generated a large critical literature (see references in O'Connor [2011] and also in Widerker and McKenna [2003]), the view seems to the first approximation to be a plausible one, and one that is consistent with what we know about the role of biology in general and genetics in particular. As such, it seems to be a reasonable one to hold in assessing the issues raised in the present chapter, as well as additional information related to neuroscience and genetics to be introduced later.

Let us now consider some general issues more directly related to the neurosciences and freewill. Discussions about the role of neuroscience and brain research in this area almost always begin with references to the Libet experiments on volition. These experiments, which Libet began nearly 30 years ago (Libet et al. 1983), have been thought to show that the brain unconsciously generates plans for movements that only later (albeit only about a second later) become conscious but are *attributed* to being initiated by a *conscious decision*. Many authors have inferred from such experiments that "freewill" is an illusion, a confabulation that the conscious mind creates for reasons that remain obscure. A recent book-length development of the idea that freewill is an illusion can be found in Wegner (2002).

The Libet experiment asks subjects to initiate a simple voluntary action, such as pressing a button, when they feel like doing so. The subject's brain activity is measured, initially using an EEG but more recently fMRI has been used. The subjects simultaneously are observing a clock, and are asked to record the time when they first "feel the urge" or their conscious intention to press the button. This "urge" moment is referred to as W standing for the subject's "will." The experiment revealed that brain activity in the frontal motor areas began several hundred milliseconds prior to the W event, suggesting that a decision to press the button was really an unconscious one and only *subsequently* became conscious *after* the "decision" had been made in the brain. This suggests that the unconscious brain is the true puppet master, and our conscious selves are but puppets of the unconscious brain.

An extensive literature both defending and critiquing the Libet experiment has developed that cannot be surveyed in this chapter. The field of neuroscience has also moved on. And currently there exist much more sophisticated methods, including the ability to record from single neurons in the brain to test the timing and more specific localization of the origination of "voluntary movements" and conscious decision making. Suffice it to say that though the technology has advanced considerably, the implications of even recent studies are equivocal whether the "conscious mind" is the initiator of such bodily movements. A good summary of some of these recent experiments can be found in Haggard (2011). One very interesting finding comes from the considerably higher spatial and temporal resolution this approach permits and which enables more specific localization of the W process in a brain region known as the midline supplementary motor area or SMA. Interestingly, in monkeys, the SMA contains a map of the body, though in humans that map is not apparent. Relatedly, it is noted that:

> This finding suggests a revision of how we interpret the [Libet] W judgment. It is clearly wrong to think of W as a prior intention, located at the very earliest moment of decision in an extended action chain. Rather, W seems to mark an intention-in-action, quite closely linked to action execution. The experience of conscious intention may correspond to the point at which the brain transforms a prior plan into a motor act through changes in activity of SMA proper. (p. 405)

The relation between brain phenomena, consciousness, and the experiencing self that makes choices is only beginning to become clarified in spite of an enormous growth in the brain and neuroscience research activities and literature. Still, these advances do serve as the backdrop to our next section on "Dangerous brains."

The neuroethics and legal context of "dangerous brains"

In this section I look at several classical and current neuroscience examples of determinism in the law. For though the Libet and related alternative neuroscience experiments are of interest, a different set of empirical results has resulted in considerably more discussion in the recent neuroethics literature.

Here I turn to several cases of individuals who suffered brain damage, either from accidents or from tumor growth and excision, which purportedly changed both their personalities and their disposition toward criminal activities. It is these brain damage cases that serve as the backdrop for a number of books and articles, and numerous meetings of judges and neuroscientists on what some call the "neurological defense" (Restak 1993).

It should be noted, however, that the specific cases to be described later are of interest *because* the brain damage is putatively compromising the decisional and volitional capacity of those individuals. This point has been emphasized in the neuroethics literature by Vincent (2011) in an analysis of what neuroimaging might be able to tell us about such capability, where she suggests that it is a volitional capacity problem that is central in such considerations. Some 20-odd years ago, the way in which volitional capacity might enter into a general analysis of decision-making capacity was investigated in a model of competence that I developed that includes three dimensions of assessment: degree of rationality, aggregated expected balance of harms and benefits, and volitional freedom (Schaffner 1991). I will not have sufficient space in this chapter to apply that analysis, which also includes the role that H. L. A. Hart's account of legal excusing conditions might play in moral and legal culpability, to neuroethical issues. The Hart account was also briefly described and provisionally related to a test for competency developed by Appelbaum and Grisso (Appelbaum and Grisso 2001), known as the MacArthur Competence Assessment Tool (MacCAT) in my 2002 neuroethics article (Schaffner 2002). But generally, it would seem that such a multiaxial or tridimensional account would hold up quite well in the legal context of the cases to be described later.

I should also add, that in addition to the brain damage cases to be described, a related set of findings, those in the genetics area, parallel these brain damage cases and have resulted in actual alterations of criminal sentencing, which I will get to shortly. But I begin with the brain damage classical case of Phineas Gage.[2]

In 1848, at the age of 25, Gage, a railroad worker, suffered a terrible injury when his tamping iron—an iron rod used for beating down the earthy substance in preparing for blasting—shot through his head in an accidental explosion. The iron bar entered his head in his cheek area just below his eyes and exited

the top of his skull. The accident resulted in Gage losing a substantial amount of brain tissue comprising what is termed the ventromedial prefrontal cortex (VMPFC). The VMPFC encompasses the brain area, approximately located behind the eyes and in the middle of the hemispheres in the frontal lobe, and the term is thus sometimes used interchangeably with what is called the orbitomedial prefrontal cortex.

Many accounts of the consequences of this event state that there was a severe personality change due to the brain injury, transforming Gage from "a reliable, well-liked, respected, and organized" to a "garrulous, sexually promiscuous, reckless, unreliable, and irresponsible—essentially a pseudo-psychopathic individual" (Raine & Yang 2006, p. 279). Antonio Damasio, a prominent neuroscientist, who has extensively publicized the Gage story describes the latter Gage as someone who

> no longer showed respect for social convention; ethics in the broad sense of the term, were violated; the decisions he made did not take into account his best interest, and he was given to invent tales "without any foundation except in his fancy," in Harlow's [the doctor treating Gage, S.S.] words. There was no evidence of concern about his future, no sign of forethought. (Damasio 1994, p. 11)

Damasio concludes that "[t]here is no question that Gage's personality change was caused by a circumscribed brain lesion in a specific site" (1994, p. 17).

In addition, Damasio says the area damaged in Gage, which has a significant effect on emotions, is critical for normal decision making. Damasio has also investigated a contemporary individual, a patient named "Elliot" or EVR, who had a brain tumor (a large orbitofrontal meningioma), which was removed surgically, resulting in a loss of brain tissue overlapping with Gage's area of estimated brain injury. Concerning Elliot, whom Damasio calls "A modern Phineas Gage," Damasio concludes that: "It is appropriate to say that his freewill had been compromised and to venture . . . that Gage's freewill had been compromised too" (p. 38).

Gage, Elliot, and several other cases briefly described in the literature on brain damage and compromised freewill suggest that neuroscience testing coupled with neuroimaging results may provide a legal excuse based on a diminished capacity. Also, Jonathan Cohen, a Princeton neuroscientist, and Joshua Greene, his student, and now at Harvard, claimed in 2004 that

neuroscience will reveal that criminal defendants are not morally responsible for their actions and that this revelation will thereby undermine retributivist justifications for criminal punishment. Greene and Cohen in their influential article "For the Law, Neuroscience Changes Nothing and Everything" wrote:

> Neuroscience is unlikely to tell us anything that will challenge the law's stated assumptions. However, we maintain that advances in neuroscience are likely to change the way people think about human action and criminal responsibility by vividly illustrating lessons that some people appreciated long ago. Freewill as we ordinarily understand it is an illusion generated by our cognitive architecture. Retributivist notions of criminal responsibility ultimately depend on this illusion, and, if we are lucky, they will give way to consequentialist ones, thus radically transforming our approach to criminal justice. At this time, the law deals firmly but mercifully with individuals whose behaviour is obviously the product of forces that are ultimately beyond their control. Someday, the law may treat all convicted criminals this way. That is, humanely. (Greene & Cohen 2004, p. 1784)

For me, this is an interesting possible position but not yet convincing and one that is not even close to being generally accepted. This issue is under intense discussion, though to date there is no US court case in which the "neurological defense," if we can call it that, has been a major determinant.

However, this area is being monitored closely as part of an ongoing project of Professor Carter Snead of the Notre Dame Law School in his "Neuroimaging and the Courts: Standards and Illustrative Case Index." This effort is part of the Judicial Seminar on Emerging Issues in Neuroscience—sponsored by the American Association for the Advancement of Science, the Federal Judicial Center, the National Center for State Courts, the American Bar Association Judicial Division, and the Dana Foundation. The currently available "Neuroimaging and the Courts: Standards and Illustrative Case Index" indicates there are 21 pages of summarized cases, as well as a summary of the evidential standards for scientific information. In the cases, there is occasional admission of scan evidence (fMRI, MRI, PET, SPECT, etc.) but only rare effects on the sentencing aspect of court decisions.

Also, it is important to note that in his incisive critique of the Gage, Elliott, and several other brain damage cases, Schleim (2012) argues that a closer analysis of these cases as well as others with comparable brain lesions shows

that while many patients had personal and social problems after their brain damage occurred, these are *not particularly criminal in nature.* Their course of life may well not only depend on their cerebral condition, but also on the social support available in their environment.

That said, there is an interesting Italian case that broadens the science to include genetic dispositions, and in which a sentence was reduced. That Italian case has a US legal case as a backdrop, as well as some 2002 genetics research, and both the case and the background genetic research are worth a brief mention. This first background case is usually referred to as the "Mobley case."

The Mobley case and other related cases are surveyed in an excellent multiauthored book by legal scholar and ethicist Nita Farahany (2009). An article by Denno (2009) in that book reviews the details of a number of legal cases involving evidence from behavioral genetics in criminal cases. One early case is based on a Dutch family with a null mutation (actually a stop codon) in the gene for MAOA (Brunner et al. 1993) (and is thus related to a somewhat later influential article by Caspi and Moffitt on MAOA alleles that appeared in 2002 [Caspi et al. 2002], which is described later). (The MAOA gene codes for monoamine oxidase A which is an enzyme that metabolizes neurotransmitters and thus keeps the amount of neurotransmitter in balance.) The Mobley case has generated much comment in legal and ethical circles and is also reviewed by Denno. David Goldman, a geneticist at the NIH who was asked to be a court consultant and to test Mobley for the null MAOA mutation (he declined), discusses the case in his very recent book; see Goldman (2012) as well as Denno (2009) for details.

The gist of the Mobley case involves a Stephen Mobley who in 1991 murdered a Domino's pizza deliverer. After his 1994 conviction for the murder and in the sentencing phase of his case, his lawyers argued that Mobley, who came from a family with a history of criminal behavior, might have the null MAOA mutation identified by Brunner and that that should be taken account of to mitigate his sentence. Actually the Mobley family pedigree did *not* match that of the Dutch family; the judge refused to order a state-paid genotyping, and Mobley was not tested. He was subsequently executed in 2005. But there have been similar types of appeals to that in the Mobley case, and in an Italian court one has succeeded in obtaining a

sentence reduction (Feresin 2009). Goldman reports Farahany's comment that there have been over 200 attempts to introduce DNA evidence into a trial and that several have been successful (Goldman 2012, p. 31). A number of these cases appeal not just to the 1993 Brunner finding, but also to the 2002 finding by Caspi and Moffitt, alluded to earlier, concerning another mutation in the MAOA gene. In their study, Caspi and Moffitt looked at two alleles of the MAOA gene. The normal type is the long allele, but the contrasting short allele form does not metabolize as well as the long allele and thus potentially unbalances the amount of neurotransmitter present. It turned out that individuals who had the short allele were much more likely to develop antisocial behaviors, including a record of criminal convictions, but only if they had been abused in childhood. Those with the long allele did not display this increased antisocial behaviors, even though they also likely suffered childhood abuse (Caspi et al. 2002).

The Italian case, which used this genetic information in part, is more specifically known as the 2009 Bayout case (Feresin 2009, accessed December 12, 2013). Bayout, an Algerian citizen who had lived in Italy since 1993, admitted in 2007 to stabbing and killing Walter Felipe Novoa Perez on March 10. On the basis of psychiatric testimony, Bayout was sentenced to 9 years, three less than typical in that jurisdiction. The case was appealed, and Pietro Pietrini, a molecular neuroscientist at Italy's University of Pisa, and Giuseppe Sartori, a cognitive neuroscientist at the University of Padova, conducted a series of tests and found abnormalities in brain-imaging scans and in five genes that have been linked to violent behavior—including the gene encoding the neurotransmitter-metabolizing enzyme monoamine oxidase A (MAOA), referring to the Caspi–Moffitt findings mentioned earlier.

In the report to the Italian court, Pietrini and Sartori concluded that Bayout's genes would make him more prone to behaving violently if provoked. "There's increasing evidence that some genes together with a particular environmental insult may predispose people to certain behaviour," says Pietrini (Feresin 2009, accessed December 12, 2013). On the basis of the genetic tests, Judge Reinotti docked a further year off the defendant's sentence, arguing that the defendant's genes "would make him particularly aggressive in stressful situations" (Feresin 2009, accessed December 12, 2013). Giving his verdict, Reinotti said he had found the MAOA evidence particularly compelling.

Thus, there is extensive discussion of a neurological defense, but the only actual case turns on not only neurological but also concomitant genetic evidence, perhaps best described as neurogenethics. It needs to be stressed, however, that the Italian decision is thus far idiosyncratic, but bears watching as a (weak) precedent for other cases in neuroethics/neurogenethics.

Neuroethics and research on developing mental disorders—a focus on the early phases of schizophrenia

I want to begin this section, which looks at another aspect of neuroethics, with some definitions and examples to clarify the terms we will be considering. Our focus here will be on some ethical implications arising in the context of psychiatric disorders, with an emphasis on schizophrenia, and in particular on the earliest forms of psychosis in that mental malady. The first clarification involves the definition of the disorder itself. Schizophrenia, in contrast to some lay uses of the word, does not mean a split personality, is not (obviously anyway) due to the stress of life, is not just holding odd ideas, such as believing in UFOs (Representative Kucinich is not a schizophrenic), and is not just thinking someone is after you (though there are aspects of a more severe type of paranoia that is manifest in some forms of schizophrenia). A well-known account of the illness is Nobel Laureate John Nash's story presented in the biographical film, *A Beautiful Mind*. Also, several recent mass shootings (including the assassination attempt on Representative Giffords, the Batman theater event, and possibly the Newtown massacre) seem to involve schizophrenics in the early stages of their disorder.

The diagnosis of schizophrenia is often difficult, requires several symptoms to be present over fairly long periods of time, and has a high threshold since it can be mimicked by other disorders and certain drugs. The new *Diagnostic and Statistical Manual of Mental Disorders*, 5th edn's (*DSM*-5) summary description is enough to give the gist of the disorder, and is found in this excerpt from the dsm5.org website: "Schizophrenia is characterized by delusions, hallucinations, disorganized speech and behavior, and other symptoms that cause social or occupational dysfunction. For a diagnosis, symptoms must have been present for six months and include at least one month of active symptoms." The website

(dsm5.org) also raises the symptom threshold for schizophrenia, requiring that an individual exhibit at least two of the specified symptoms. In the manual's previous editions, that threshold was one. Additionally, the diagnostic criteria no longer identify [former] subtypes [such as the older catatonic and paranoid forms]. A much more detailed description of the *DSM*-5 account of schizophrenia, and changes from the *DSM-IV-TR* diagnosis, is available at Tandon et al. (2013).

Schizophrenia is one of a number of severe mental illnesses that devastate millions of lives worldwide. Exciting recent developments in studies of psychoses in general, and in schizophrenia in particular, however, are offering hope that such illnesses can be identified and treated early. Early treatment offers the promise of better outcomes for both the affected individuals and their families. Such treatments aim at reaching patients during their first "psychotic break" as well as attempt to identify at-risk individuals during the preillness or "prodrome" period. This latter form of "early intervention" (EI) projects thus also point toward the possibility of true prevention for those at risk of psychoses from genetic or environmental factors or both.

A focus on the prodromal stage of schizophrenia and its historical backdrop has been the subject of several recent articles, including Gross and Huber (2010) and Fusar-Poli et al. (2013). Early work in this area dates back at least into the 1950s as described by Gross and Huber (2010), with the "basic symptom" concept then further pursued in the 1980s by Klosterkötter.[3] These studies can roughly be described as falling into European (some call it the Germanic) tradition. More recent history covering the 1990s through today, including the development of newer instruments and clinical trials are described in some detail in Fusar-Poli et al. (2013).

For this audience, a *brief* summary of the key prodromal developments in the 1990s and some later research will be useful.[4] In addition to the European tradition mentioned previously, what might be termed the Anglo-Australian-American tradition was initially developed by the McGorry group in Melbourne and several other research projects in other countries. In 1991 McGorry and Yung and other colleagues created the Early Psychosis Prevention and Intervention Centre (EPPIC) and the Personal Assistance and Crisis Evaluation (PACE) clinic. Publications from the Melbourne group began to influence the literature several years later, with important articles describing the prodrome

in operationalized terms, including three types of patients at increased risk for psychosis (Yung & McGorry 1996; Yung et al. 1998). Beginning in 1994, the Australian studies also developed the Comprehensive Assessment of At Risk Mental States (CAARMS) instrument. The CAARMS was "specifically designed to prospectively measure the psychopathology of the At Risk Mental State" (Weinberger and Harrison 2011, p. 1766) characterized as that mental state which may represent the prodrome or precursor state to a first psychotic episode.

Also in the early to mid-1990s, Birchwood's group in the United Kingdom began investigations into the prodrome. In the United States, McGlashan and his colleagues at Yale in New Haven, including the late Tandy Miller, established the Prevention Through Risk Identification Management and Education (PRIME) clinic and developed the widely used Scale of Prodromal Symptoms (SOPS). The SOPS is administered as part of a candidate interview termed the Structured Interview for Prodromal Symptoms (SIPS), both of which are instruments to assess the prodromal state (McGlashan et al. 2010), thus complementing the CAARMS. Research in Melbourne, New Haven, and many other centers around the world continued through the 2000s and are vigorously pursued today. Several additional citations to some of these later developments are mentioned further later.

However, the studies needed to determine the safety and efficacy of such early interventions in the prodrome and even first-episode schizophrenia raise a series of significant neuroethical and regulatory questions that if not adequately answered will impede progress in this vital area, as well as potentially harm human subjects and their families. The studies in this area are controversial, in part because a number of them include administration of low doses of new antipsychotic drugs to some participants who are not psychotic. These drugs were developed for severe forms of mental illness and have some significant side effects. In the early 2000s several proposed studies in early psychosis intervention were denied funding by the National Institutes of Mental Health (NIMH), partly because of human subject concerns. In fact, one prodromal clinical trial conducted at Yale University in the early 2000s was investigated by the US Office for Human Research Protections (OHRP) on the basis of a complaint alleging Yale was treating nonill individuals and not

offering any benefits that would offset the risks from the drugs (Schaffner & McGorry 2001). The Yale program was exonerated after the investigation.

Several recent articles in the neuroethics area address the issue of ethical and policy issues in research on mental disorders (see Kim 2013). The general context of these discussions is typically that of obtaining informed consent, but this issue also swiftly moves to considerations of risk and benefit in an area of incomplete psychiatric and neuroscientific knowledge, as well as raises questions of capacity to participate in such investigations. In the remaining part of this chapter, I note a very recent and still ongoing dispute that arose in the revision process of the just published *DSM*-5 (American Psychiatric Association 2013). This somewhat contentious set of ethical and scientific arguments, both within the *DSM*-5 Psychotic Disorders workgroup and from the larger psychiatric community, has resulted in postponing the diagnosis of the prodromal state as a recognized psychiatric diagnosis. In the *DSM*-5 deliberations, the prodromal state was initially termed the "psychosis risk syndrome" (PRS). However, various consideration led to this diagnosis being reconceived and renamed the "attenuated psychosis syndrome" (APS). This reasoning was interesting, and the Psychosis workgroup recently wrote:

> Data revealed that a majority of individuals with this condition [PRS] did not go on to develop a psychotic disorder and that most individuals with this condition had additional relevant clinical needs other than the risk of conversion to psychosis. Consequently, a condition that described current clinical need—attenuated psychosis syndrome (APS)—was considered instead. In contrast to "psychosis risk syndrome," APS describes a currently relevant clinical condition leading to help seeking, with many more clinical outcomes other than conversion to psychosis. (Tsuang et al. 2013, p. 32)

This latter named disorder is currently found in Section III labeled "Emerging Measures and Models," indicating these diagnoses are not yet ready for inclusion in standard diagnoses grouped in Section II of the manual (American Psychiatric Association 2013, pp. 783–786). I will not have the space in this introductory section to provide any comprehensive approach to the neuroethical problems in this area, but will outline some of the main ethical issues and provide pointers to the literature concerning this still-evolving disorder. The best overviews of the science related to the APS issue as

well as the extensive psychiatric literature can be found in the recent articles by Tsuang et al. (2013) and Yung et al. (2012).

The initial proposal of a psychosis risk syndrome was advanced against the backdrop of the early intervention studies briefly noted at the beginning of this section; for a more extensive history of this work, see Fusar-Poli et al. (2013), Schaffner and McGorry (2001), and Gross and Huber (2010). Familiar objections were raised in the psychiatric literature to the PRS (in Yang et al. [2010], Corcoran, First, and Cornblatt [2010]). Corcoran, First, and Cornblatt (2010) cautioned that "there is a potential for high rates of false positives particularly at the community level given the difficulty in discriminating mild symptoms from normal variants and low base rates of the syndrome in the general population" (p. 16). The authors added "there are substantial risks, for both stigma and discrimination, and for unnecessary exposure to antipsychotic medications, which make the high false-positive rate associated with the psychosis risk designation particularly problematic" (Corcoran et al. 2010, p. 16). These issues were looked at again in terms of the benefits of including such a diagnosis, as well as the downsides, in Section II of the developing *DSM*-5. See, for example, an exchange by the Chair of the Psychotic Disorders group, William Carpenter, MD, and a senior member of that committee, Jim van Os, MD (Carpenter & van Os 2011). Other articles were also published, some arguing for including the psychosis risk aspect of the APS, such as Woods et al. (2010). Some insightful suggestions concerning the types of research that will be needed to advance the APS concept can be found in Yung et al. (2012). Relatedly, McGorry and van Os have proposed a more flexible approach to diagnosis termed "staging" for the APS and PRS analogues (McGorry & van Os 2013; also see Fusar-Poliet et al. 2014). The "staging" notion also has some antecedents in the earlier work of Huber and Klosterkötter, and has also been associated with endophenotypic research (Klosterkötter 2008; McGorry et al. 2006);[5] also see point 5 in the "Points to Consider" section later in this chapter.

Ultimately, as indicated earlier, the decision was made to place the diagnosis in Section III and recommend further study. As noted earlier, the Psychotic Disorders workgroup has very recently published an article summarizing their views (Tsuang et al. 2013). Recognizing that substantial advances had been

made in the early detection and intervention area, the group members stated that:

> To consider the appropriateness of adding psychosis risk syndrome to our diagnostic nomenclature, the psychotic disorders work group extensively reviewed all available data, consulted a range of experts, and carefully considered the variety of expert and public comments on the topic. It was clear that reliable methods were available to define a syndrome characterized by sub-threshold psychotic symptoms (in severity or duration) and which was associated with a very significant increase in the risk of development of a full-fledged psychotic disorder (schizophrenia spectrum, psychotic mood disorder, and other psychotic disorders) within the next year. At the same time, the majority of individuals with "attenuated psychotic symptoms" had one or more other current psychiatric comorbid conditions (usually mood or anxiety disorders, substance use disorder; Fusar-Poli 2012) and exhibited a range of psychiatric outcomes other than conversion to psychosis (significant proportions either fully recover or develop some other psychiatric disorder, with a minority developing a psychotic disorder). (Tsuang et al. 2013, p. 31)

These authors added:

> Although the reliability of the diagnosis is well established in academic and research settings, it was found to be less so in community and other clinical settings. Furthermore, the nosological relationship of attenuated psychosis syndrome (APS) to schizotypal personality disorder and other psychiatric conditions was unclear. Further study will hopefully resolve these questions. The work group decided to recommend the inclusion of attenuated psychosis syndrome as a category in the appendix (Section 3) of *DSM*-5 as a condition for further study. (Tsuang et al. 2013, p. 31)

In addition, the field trials run as part of the *DSM*-5 revision process were not able to establish reliability due to too small a sample size (Tsuang et al. 2013).

The final and published clinical criteria for the APS are as follows:

A. At least one of the following symptoms is present in attenuated form, with relatively intact reality testing, and is of sufficient severity or frequency to warrant clinical attention:

   1. Delusions.
   2. Hallucinations.
   3. Disorganized speech.

B. Symptom(s) must have been present at least once per week for the past month.
C. Symptom(s) must have begun or worsened in the past year.
D. Symptom(s) is sufficiently distressing and disabling to the individual to warrant clinical attention. [Interestingly, an earlier draft version of this point D stated that "Symptoms in Criterion A are sufficiently distressing and disabling to the individual and/or legal guardian to lead them to seek help" (Tsuang et al. 2013, p. 32), thus explicitly introducing the terminology of "help seeking" not present in the final version of the APS.]
E. Symptom(s) is not better explained by another mental disorder, including a depressive or bipolar disorder with psychotic features, and is not attributable to the physiological effects of a substance or another medical condition.
F. Criteria for any psychotic disorder have never been met. (American Psychiatric Association 2013, p. 783)

Points to consider

The consensus, then, of virtually all investigators in these areas is that various risks and uncertainties in the APS concept will need additional scientific study. From the perspective of neuroethics, the same issues noted earlier will also require additional ethical, legal, and regulatory (including local institutional review boards or IRBs) examination of integrated informed consent, and risk/benefit issues in any proposed studies. In an article that I coauthored with McGorry some dozen years ago, we outlined five intertwined ethical and study design "points to consider" that warrant further consideration in what is now termed the APS area (Schaffner & McGorry 2001, p. 3). Those points, somewhat modified, updated, and supplemented (Point 6 later) are still relevant, and are listed as follows:

Point 1

Early intervention patients are diverse, clinically and ethically, and in fact may even be more clinically heterogeneous than can be identified at present. Patients who satisfy the current treatment (and clinical trial inclusion) criteria for prodromal studies such as the APS include among themselves a large number (probably ~60%) of false positives who may run risks of labeling and

from drug treatment, a problem that should be resolvable by implementing the appropriate guidelines already in existence that prioritize nondrug interventions (see ("International Clinical Practice Guidelines for Early Psychosis" 2005) (Fusar-Poli et al. 2013). Obtaining appropriate informed consent (see several other points later) is also relevant here. And regarding the labeling aspect, as has recently been noted, "An additional concern is that there is still the danger that people meeting the criteria will be incorrectly conceptualized as being on the psychosis spectrum and that an irreversible lifelong underlying 'process' has started" (Fusar-Poli et al. 2013). Incorporating minors in early intervention research raises special ethical, legal, and regulatory problems, but excluding minors in this area is unwise given ages of incidence and at least by US regulations probably untenable. Though the false positive issue, especially in community settings, is problematic, it should be noted that the existing research centers using the APS criteria have tended to obtain conversion rates to psychosis of about 20% in the first year and 36% in 3 years. This is better than various medical condition predictions, such as those for diabetes (Tabak et al. 2012).[6]

Point 2

Interventional studies employing selective serotonin reuptake inhibitors (SSRIs), atypical neuroleptics, cognitive therapies, and other modalities such as omega-3 fatty acids are being conducted. All of these options as well as other reasonable drug and behavioral interventions need to be *disclosed to potential patients as treatment alternatives*, along with the guidelines previously cited ("International Clinical Practice Guidelines for Early Psychosis" 2005).

Point 3

The conditions under which various randomized, open label, and blinded trials should be implemented in the APS and kindred PRS areas needs further public discussion, and the uncertainty about the best designs may need to be communicated to prospective patients (this is revisited in more detail in the next and also in the final point). A more precise characterization of epidemiologically based naturalistic studies and a clearer methodological defense of their ability to avoid bias and achieve standard levels of power and

significance would be helpful to make sure that such studies conform to good scientific designs.

Point 4

Some psychiatrists and clinical psychologists view a double-blind, placebo-controlled clinical trial in this area as being premature in its scientific rigor, whereas others believe this is where the field should be at this time. Others seem to be in favor of continued open-randomized trials of atypical low-dose neuroleptics (though see the points earlier), but there are some investigators who favor even less directive interventional studies at present, recommending the more epidemiological types of studies. As described earlier, McGorry, van Os, and others have proposed a nuanced staging approach (McGorry and van Os 2013; McGorry et al. 2006). This involves a more pluralistic approach. This concept is not just focused on the APS concept, nor solely on the prodrome, but includes a broader risk syndrome including patients at risk for persistent mood and anxiety disorders (van der Gaag et al. 2013). Such a staging approach may also provide more "personalized" psychiatric care. Whichever type of trial and informative materials is offered, *the subjects need to be acquainted with an easily understandable account of the relevant features of any trial's methodology and its pros and cons*. Subjects need to be told that there are *trade-offs* between a personally structured plan-of-care and scientific rigor to avoid the risks of therapeutic misconception, particularly since in the APS the qualifying patients are implicitly characterized as "help-seeking"—see the bracketed comment regarding Criterion D in the APS diagnosis above—and thus expecting relief or reassurance regarding their symptoms.

Point 5

There are existing materials that could be provided to interested parties including patients, patients' families (and surrogates), IRBs, and patient advocacy organizations, but additional information needs to be generated to facilitate informed participation in early intervention projects. Further, valid means of assessing the ability of that information to be used by research subjects and their representatives need to be developed and tested, and the information generated as part of the *DSM*-5 analysis of the APS proposal included in information for prospective subjects and their families. In a potential research

context of capacity to consent, I have worked with Paul Appelbaum, MD, and helped develop a version of the MacCAT for clinical research that initially was designed for several diverse early intervention projects, and which could be adapted to the APS. Also see the suggestions in Northoff (2009) regarding the use of the MacCAT in neuroethics.

Point 6

A number of articles discussing the negative aspects of the APS have identified stigma as a potentially major issue. A review of stigma literature in this area, including self-stigma, familial stigma, social stigma, including insurance stigma, has outlined some cautionary concerns (Yang et al. 2010). McGorry's group in Melbourne ameliorates the stigma issue by treating its patients in more general youth health settings. These include the PACE clinic, where the name was chosen as a deliberately general term. Also, because the clinic has been situated in a nonpsychiatric setting, (i.e., a young people's health service named the Centre for Adolescent Health), McGorry's group has published data that shows reduction in distress in all patient groups together with high levels of attendance at the clinic, suggesting that this setting can decrease stigmatization of the patients (McGorry, Phillips, and Yung 2001). Since 2006, the Melbourne group has also developed a broad spectrum and stigma-free Australian platform for youth mental health care called headspace; see www.headspace.org.au, which in addition connects to various help lines (accessed August 10, 2013). Though these approaches are encouraging, stigma is likely to be very resistant as a component of psychiatric stigma in general, and schizophrenia stigma in particular. That said, the potential stigma effects of a Section II *DSM* APS diagnosis needs to be balanced against the assistance that this positioning of the APS diagnosis could offer to patients via psychiatric and societal support; thus, this will remain as a salient issue in the neuroethics of early interventional conception and its studies.

Scientific research in the EI area continues, with a broad North American consortium generating new data (Addington & Heinssen 2012) and research continuing in Australia, Denmark, Norway, the United Kingdom, and other countries. One philosophically intriguing as well as scientifically novel approach to further sharpen the APS diagnosis and weed out some false positives is the use of subtle inquiries and analysis of changing concepts of

the self (see Nelson et al. 2009; Nelson et al. 2012; Parnas 2011; Parnas et al. 2005). This is an approach that revisits some of the earlier phenomenological-based approaches, though now in the context of advances both in general assessment methodology as well as the neuroscience of endophenotypes and brain imaging. This "subjective" orientation includes not only the approach of Huber and the basic symptom analyses as cited earlier, but also as developed by Jaspers in his magisterial text (Jaspers 1997). Both in Parnas' work in Denmark and Nelson's research in Australia, the subjective aspect is supplemented both with objective clinical sign data as well as information based on neuroscience and imaging material.

Accordingly, this EI area will continue to be explored further in the next few years, initially in terms of the APS but likely also as broader constructs reflecting various aspects of psychosis risk, and be a fertile area for both scientific and neuroethical analysis. It remains to be seen at what point these analyses will have produced sufficient advances that the APS or a cousin of it will move into Section II of some future *DSM*-5.X, or even a *DSM*-6.0.

Summary and conclusions

This chapter sketched an overview of the relatively new and still emerging field of neuroethics. After reviewing how the subject developed against the backdrop of the HGP's subproject on ethical, legal, and social implications of genomics, I focused on three salient topics in neuroethics. We considered what the initial implications of the neurosciences, and also genetics, were for freewill or free agency. This analysis served as an introduction to a more "practical" topic involving legal implications of our understanding of free agency limitations, and the subject of what has been termed "dangerous brains." That topic took us into considerations of classic cases such as the Phineas Gage example, but also more current cases, involving both brain pathology as well as recent genetic advances. My third area of analysis looked at how neuroscience may assist our understanding of psychiatric disorders, especially schizophrenia and the early intervention studies on attenuated psychosis syndrome, in connection with the ethics of research on this disorder's psychiatric and neuroscientific aspects.

Notes

1 I thank Erik Parens for suggesting that I consider the distinctions that Adina Roskies proposed back in 2002 in the context of this chapter.
2 This section is partially indebted to the analyses of "dangerous brains" found in Schleim (2012).
3 I thank Josef Parnas for directing my attention to this earlier work by Huber.
4 The history of the early intervention movement is a complex story and though summarized in various articles, as cited in the text, no one has yet put together the book-length account it deserves.
5 For the now classic article on defining the concept of endophenotype in psychiatry, see Gottesman and Gould (2003).
6 I thank Patrick McGorry for pointing out to me this comparison with prediction of conversion from prediabetes to diabetes.

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8

Science as Cultural Performance: Leveling the Playing Field in the Theology and Science Conversation

James K. A. Smith

Scientism and the religion/science dialogue

I would like to float just one tiny little provocative claim. Consider it a discussion starter: The dialogue between religion and science, as it has often been conducted, operates on the basis of a category mistake. In particular, I think that the dominant paradigm in science-and-theology discourse plays with loaded dice, such that the house (science) always wins. Or, for more sanctified folks who won't get the gambling metaphor, I suggest that one of the dominant paradigms in the theology/science dialogue has set up an uneven playing field that has put religion in the position of having to play uphill. This is because, in covert ways, religious folks with the best of intentions have bought into a version of scient*ism*.

My goal in this chapter is to level that playing field by simply emphasizing the ways in which science is a human, cultural activity. This angle of approach is rooted in a basic assumption or intuition I need to first explain. "Scientism" can be defined in a number of different ways. On the one hand, scientism could be defined as reductionistic: reducing the complexity of the cosmos and human life to the narrow purview of what can be measured and arbitrated by the natural sciences. On the other hand, and as the flipside to this, scientism is the result of science overstepping its jurisdiction of authority. In other words, science becomes scient*ism* when it overreaches its purview—when, for example, it makes metaphysical pronouncements about naturalism that could

never be rooted in empirical data, or when it extrapolates from the material conditions of human consciousness as if those were the *only* (and sufficient) conditions for consciousness—*as if* biology or neuroscience, for example, are able to canvas *all* aspects of human consciousness. So I'm working with a baseline assumption that scientism is both reductionistic *and* overreaching.

With that in mind, I want to argue that scientism is what happens when we forget that science is a human, cultural activity—just the kind of cultural activity that cannot itself be accounted for by the natural sciences. Now, in fact, I think practicing scientists rarely forget this; they are too closely involved in the concrete material practices of science to ever be tempted to let it become an abstract magisterium that determines all truth. (In fact, I think the social sciences are more prone to scientism, but will leave that for another day.) Rather, I think scientism is the result of a story that a secular culture wants to tell *about* science. And in many ways, the contemporary dialogue between religion and science buys into that story.

Category mistakes: Science, nature, culture, theology

As I said, in many ways the religion/science dialogue rests on a category mistake because it has operated on the basis of a certain positivism vis-à-vis "science," and taken the "findings" of science *as if* they were pristine disclosures of "nature."[1] Thus, we increasingly encounter familiar tropes about what "we now know" or "what science says," which are all too often followed by identifying some religious doctrine or dogma that needs to now be abandoned or modified.[2] Both "new atheists" and religious scholars tacitly work within this paradigm. Indeed, some of the features of this paradigm are what we usually associate with "liberal" Christianity. But I suggest that some religious scholars—even "conservatives" who hold a high view of Scripture—have unwittingly bought into aspects of this paradigm.

In my own tradition, this has been exhibited in recent debates about evolution, human origins, and the Fall, where the implications of an evolutionary account of human origins are thought to necessarily entail the abandonment of historic theological convictions about sin. But one could consider other cases: for example, often claims about the material conditions of consciousness are thought to entail a reductionism that precludes historic

religious convictions about "the soul." And so one can find religious scholars bending over backward to come up with "monistic" accounts of the human person in order to satisfy the strictures of what science will admit as plausible. In other words, in many ways, both new atheists and religious scholars share a lot of the same convictions *about science*, and in many ways I think those convictions are scient*istic*.

This is because, on both ends of the continuum, there is a similar (though perhaps unwitting) assumption about the nature of *science*: science is either the pristine deliverer of the cold, hard, secular truth; or science is the crystal clear lens for disclosing the "message" in the book of nature.[3] As such, science is taken to be an odd sort of transparent black box that simply discloses the "objective" features of *nature*. Thus, while the "dialogue" is purportedly between "science" (roughly, a constellation of academic disciplines) and "theology" (roughly, another academic discipline), *in fact* or *functionally*, the dialogue assumes that religion is a kind of human cultural product whereas science is merely the conduit for disclosing the cold, hard realities of "nature"—to which faith must answer, demur, or affirm. After all, who's going to argue with "nature"?

So like a schoolchild of years ago, we have to suck it up, lay out our hand, and bear the brunt of the strap. Religion needs to be *disciplined* by the findings of science and submit itself to the cold, hard realities of nature.[4] If this turns out badly for some traditional or "fantastic" religious claims, then believers have to take that as part of their whipping and leave the principal's office grateful that they've been chastised, since this will make them more intellectually responsible.

This configuration of the theology/science dialogue sets up an asymmetrical relationship—and skewed playing field, as I put it earlier. While the conversation claims to be a dialogue between "science" and "theology," I think that *functionally* it is taken to be a confrontation between *nature* and *culture*.

science : : theology

nature : : culture

But that is a category mistake. In fact, a dialogue between "science" and "theology" is always already a dialogue between "culture" and "culture," both of which are confronted by, constrained by, and must answer to a certain "givenness" that we often describe as "nature."

science : : theology

culture : : culture

nature[5]

In other words, the theology/science conversation has tended to ignore the fact that science is a *cultural* institution. By a "cultural institution" I mean that science is a product of human *making*, a contingent product of *poiesis*.[6] Culture is the unfolding of potentialities that are latent or implicit in "nature," as it were. So aspects of "culture" are the fruit of human making and unfolding; they are not "natural kinds." A painting by Picasso, an elementary school, a Boeing 747, and a political constitution are all examples of "culture," of human making. They are not "naturally occurring" entities that one would bump into if there weren't human agents that unfolded them and brought them into being. So cultural institutions are networks of practices, habits, and material environments that are the product of human making. A hospital is a cultural institution that is "unfolded" by a human community and is composed of both a particular built environment (ER and ORs, ambulances, CAT scan machines, etc.) and networks of practices and traditions that are learned by apprenticeship (e.g., the "disciplines" of surgery and medicine, the traditions of care that define nursing, etc.). Hospitals don't fall from the sky, nor do they simply crawl up from the lagoon in the La Brea Tar Pits. They emerge as products of human making—which means that they are essentially historical, contingent, and that they unfold over time.

Now, it seems to me that the science/religion conversation happily acknowledges that religion is a cultural institution. How could one not? Theology is a product of religious traditions and communities, which are themselves paradigmatic instances of "cultural institutions" that are historical, contingent, and certainly not "natural."[7] Even if launched by a supernatural impetus, they have unfolded over time, have unfolded differently in different places, could have unfolded otherwise, and might even have not unfolded at all. Thus "theology," as a cultural institution, is recognized as a kind of "hermeneutic" reality—it offers interpretations of the world, is shaped by different traditions and presuppositions, and represents a "take" on things. From the perspective of the dominant paradigm in the theology/science conversation, this means that theology is sort of one step back from "reality." It is a cultural institution

that ascribes "meaning" to reality/nature, whereas "science" is a conduit for disclosing the realty of nature *as such*.

This paradigm has failed to *functionally* appreciate (even if it might officially concede) that science is also a cultural institution. "Science"[8] is not a naturally occurring entity like igneous rocks or sea horses; that is, science is not something that emerges from the swamp or falls from the sky apart from human making. Rather, science is a network of material practices, built environments (including laboratories, instrumentation, etc.), traditions of apprenticeship, and learned rituals that emerged over time, in particular configurations, in different places (see Gaukroger 2009). So any conversation between "science" and "religion" is never going to be simply a matter of getting theology to face up to "nature"; rather, it is always already a *cross-cultural* dialogue. It is a conversation between two different cultural institutions, each with its own traditions, practices, built environments, and meaning systems. Because of its lingering positivism, the theology/science dialogue—at least as I've seen it—tends to operate in isolation from a vast (and growing) literature on science *as* culture, such as the social history of experimentation, the politics of the Royal Society, the material dynamics of apprenticeship, the economics of instrumentation and technological developments, the cultural embeddedness of medicine, etc. Ignoring that sort of *history* of science makes it a lot easier to treat science scientistically—as a kind of pure, transcendent, secular magisterium.

Philosopher Robert Brandom articulates the nature/culture distinction as the distinction between things that have *natures* and things that have *histories*. While the stuff of physics has a "nature," physics *as a discipline of scientific study* has a history. And in fact, "even concepts such as *electron* and *aromatic compound* are the sort of thing that has a history" (Brandom 2000, pp. 26–27). So the encounter between theology and science is not equivalent to an encounter between theology and *nature*. As Joseph Rouse comments,

> Scientific practices are often construed as apart from any surrounding culture, and even free from culture, but such construals are not adequate to the richness and complexity of scientific work. Recognizing the intimate entanglement of the sciences with other practices does not diminish or blur their significance but instead acknowledges their pervasiveness throughout the world. (2002, p. 166)

The point here is *not* a debunking project; that is, I'm not pointing out that science is a cultural institution in order to dismiss it or give us permission to ignore it. Rather, the point is to situate science *as* a cultural institution in order to clarify this category mistake and thus level the playing field.

Science takes practice

What would it mean to appreciate science *as* culture, as a *cultural* institution? What are the implications of recognizing science as a cultural institution for the theology/science dialogue? Briefly, I would note just a few.

Leveling the playing field

As already indicated, one important implication of recognizing science as culture is a leveling of the playing field in the theology/science dialogue. While it might be the case that theology must rightly be constrained by the "givenness" of nature—the world that pushes back on our claims—that is not the same as saying that theology must bow at the feet of *science*. We need to recognize a distinction between science and nature, a distinction too often erased. Science is not just a transparent magnifying glass or pristine conduit that delivers nature "as it really is." Science is a cultural institution (or, better, a constellation of cultural institution*s*) that is, of course, especially attentive to nature, is interested in describing and perhaps even explaining nature, and exposes itself to nature's pushback through the rigors and disciplines of experimentation and observation. But that doesn't make science "natural." It remains a cultural layer of human making. And in this respect, it is in the same boat as theology (and literature and sociology and . . .).

Therefore, theology should no longer feel that it has to defer to science *as if* it was thereby subjecting itself to nature or "reality" (as in, "science tells us . . ."). While theological claims are rightly disciplined by the ways in which the givenness of the world "pushes back" on our claims, this is not synonymous with being disciplined by science. Furthermore, religious communities bear witness to the fact that the "push back" we feel from the cosmos also includes a pressure felt from the transcendent, from eternity—precisely the "push back" that scientism wants to ignore.

Appreciating the role of practices

The theology/science conversation should also stop thinking of "science" as a static body of *findings* and instead consider science as a dynamic process of *finding*. The "science" in the theology/science dialogue is a remarkably disembodied phenomenon—as if there were no laboratories, instruments, or communities. But science is not just the *results* of science, the data sets or images that get produced at the end of a very long process. Nor is science just a matter of *theory*. Rather, "science" is perhaps best identified as the *practices* that yield such fruit. Science is a deeply social, communal project, composed of material practices and rituals that are handed on as traditions, absorbed as habits, and enacted in experimental performance that, literally, create worlds.[9] With its own rituals and traditions, science is not unlike religion.

Philosopher Robert Crease emphasizes that science is not just theory; it is actually defined by experimentation that is a kind of "performing art":[10]

> A scientific entity does not show up in a laboratory the way an airplane shows up on a radar screen, a fully formed thing out there in the world whose presence is made known to us by a representation. Nor is a scientific entity like a smaller version of the airplane, which could be perceptible if only scaled up large enough. Nor, finally, is a scientific entity like some distant and unknown object on the radar screen that when closer becomes perceptible. A scientific entity becomes perceptible only in performance. (Crease 1993, pp. 85–86)

While science seeks to be disciplined by nature, there is also a sense in which science *creates* its own phenomena. It constitutes its world through experimental performance which is a *learned* performance requiring its own set of virtues and skills, deft employment of instrumentation, and a kind of "know-how" that is not theoretical, and perhaps not even "intellectual."

Thus, Hans-Jörg Rheinberger, in his stunning philosophical history of the protein synthesis, notes the way in which the "stuff" of science—"epistemic things" or "research objects"—in some ways emerge because of experimental conditions that are created by "technical objects" (such as instruments). The epistemic things "articulate" themselves "through" a "wider field of epistemic practices and material cultures," which include both instruments and theories (Rheinberger 1997, pp. 28–29). In important ways, the "epistemic things" that will emerge "usually cannot be anticipated when an experimental arrangement

is taking shape" (p. 74). (So there are a lot more surprises in science than one would guess from the picture we get from the theology/science dialogue.) Thus, "experimental systems are necessarily localized and situated *generators of knowledge*" (p. 76, emphasis added). What science *finds* is significant, determined not just by what science goes looking for, but *how* it looks, and what it looks *with*. And that "how" is not primarily a theory but a constellation of practices that constitute an experimental system. As these systems build up over time and generate linkages with other experimental systems, there emerge what Rheinberger calls "experimental cultures" that "share a certain material style of research" or "laboratory style" (pp. 138–139). At that point, experimental systems begin to take on a life of their own. They generate epistemic things by generating microworlds—which are responses to nature but should not be identified with nature.[11]

Hence, once again, we see the importance of not mistaking science with nature. We also note Rheinberger's concluding caveat—cautioning that this is not meant to thereby reject science:

> To characterize science as practice and as culture does not amount, as far as I apprehend it, to determining the social influences hindering or furthering the sciences. It does not amount to a critique of ideologies of science in the traditional sense. Rather, it amounts to characterizing the sciences themselves as cultural systems that shape our societies and all the while trying to find out what makes the sciences different and confers on them their peculiar drive, not privileging them with respect to other cultural systems. (Rheinberger 1997, p. 140)

Meaning and interpretative practices

This priority of practice to theory should make us attentive to the nature of scientific practices—which is what defines the landmark (but underappreciated) work of Joseph Rouse.[12] Rouse emphasizes a "normative" understanding of practices that attunes us to just how "loaded" scientific practices are. "What a practice is," he emphasizes,

> including what counts as an instance of the practice, is bound up with its significance, in terms of *what is at issue* and *at stake* in the practice, to whom or what it matters, and thus with how the practice is appropriately or perspicuously described. (Rouse 2002, p. 175)

What's at stake and what's at issue are embedded in the practices and constitute a particular hermeneutic construal of the world. There is always a normativity at work in practices, including experimental practice. Practices are "defined" not just by the specific activities that "compose" them but also "by what those activities are *about* (what is 'at issue' in the practices) and by what is *at stake* in their success and their continuation" (Rouse 1996, p. 142, emphasis added).[13]

This is the basis for Rouse's core thesis: *practices matter*. Practices have something at issue and something at stake.[14]

> One has not understood a practice unless one has grasped the point of the practice, that is, what is at issue and what is at stake. The recognition that practices are focused by such issues and stakes does not, however, challenge my earlier insistence on the openness of the practice. (Rouse 1996, p. 142)

This means that scientific practices are not just pure conduits of a "given" world of "facts," but rather are world *constituting*. It is practice that "gives meaning," and thus scientific practices—as cultural institutions—are as "meaning-giving" as those of theology. So we have to reconnoiter how we have traditionally understood the theology/science distinction. Scientific practices are not merely passive, "observational" practices that simply yield "facts." Like theology, they also give meaning—they render significance.

The encounter between religion and science can't be a division of labor whereby science discloses the "facts" and then faith renders a "meaning" consistent with those facts. While there is no inherent conflict between religious faith and science[15] (where science is understood as the human cultural practice of attending to and understanding the natural world), we need to recognize that there *can* be conflict between the different meanings they assign to the natural world—in short, such conflicts arise precisely when we're dealing with, not science, but scientism. Sometimes in our eagerness to dispatch with simplistic, unproductive models that posit a battle between science and faith, we too quickly look to reconcile what really are competing visions of the world. Recognizing science *as* culture should at least grant us permission to demur from the magisterial authority that science assumes in its disclosure of "the facts of the matter." But it should also enable us to recognize when science *as a cultural institution* begins to assume roles that other cultural

institutions have generally assumed. I don't think it's too outlandish to suggest that scientism is a way to describe the cultural phenomenon whereby science (really: scientism) takes over the role of religion, begins to function *as* a religion that offers *ultimate* explanations.

Scientism as the master narrative of "exclusive humanism"

The Catholic philosopher Charles Taylor, in his monumental genealogy of our "secular age," provides some tools to understand how this happened. On Taylor's account, scientism is exactly what "exclusive humanism" needs: a way to account for what's ultimate without any appeal to transcendence—an account of "the way things are" that is solidly within the "immanent frame." This is why the Enlightenment is still with us, despite our so-called postmodern moment. Taylor diagnoses its endurance in a fragilized secular age through a fascinating little psychoanalysis of a convert—not a convert *to* religious faith but rather someone (or even a culture) that has converted from belief to unbelief (Taylor 2007).

If someone tells you that they've converted to unbelief because of science, don't believe them. Because what's usually captured them is not scientific evidence *per se*, but the *form* of science: "Even where the conclusions of science seem to be doing the work of conversion, it is very often not the detailed findings so much as the form" (Taylor 2007, p. 362). Indeed,

> the appeal of scientific materialism is not so much the cogency of its detailed findings as that of the underlying epistemological stance, and that for ethical reasons. It is seen as the stance of maturity, of courage, of manliness, over against childish fears and sentimentality. (Taylor 2007, p. 365)

But you can also understand how, on the retelling, the convert to unbelief will want to give the impression that it was the scientific evidence that was doing the work.

And the belief that they've converted *from* has usually been an immature faith that could be easily toppled. So while such converts to unbelief tell themselves stories about "growing up" and "facing reality"—and thus paint belief as essentially immature and childish—their "testimony" betrays the simplistic shape of the faith they've abandoned. "[I]f our faith has remained at the stage of the immature images, then the story that materialism

equals maturity can seem plausible" (Taylor 2007, p. 365). But in fact their conversion to unbelief was also a conversion to a new faith: "faith in science's ability" (p. 366).

Such tales of maturity and "growing up" to "face reality" are stories of courage—the courage to face the fact that the universe is without transcendent meaning, without eternal purpose, without supernatural significance. So the convert to unbelief has "grown up" because she can handle the truth that our disenchanted world is a cold, hard place. But at the same time, there can be something exhilarating in this loss of purpose and teleology, because if nothing matters, and we have the courage to face this, then we have a kind of epicurean invulnerability. While such a universe might have nothing to offer us by way of comfort, it's also true that "[i]n such a universe, nothing is demanded of us" (Taylor 2007, p. 367). Now the loss of purpose is also a liberation: "*we* decide what goals to pursue." God is dead; *viva la revolution*.[16]

In the "liberating" power of the loss of meaning, one can already see burgeoning hints of what's coming: Nietzsche, and other "post-Schopenhauerian" visions (Taylor 2007, p. 369). What we get here, according to Taylor, is an internal critique of modernity, the "immanent counter-Enlightenment" that turns against the values of the Enlightenment precisely insofar as those values were secular analogues of a Christian inheritance (think: *Geneaology of Morals*, which targets Kant *and* Jesus, Hegel *and* Paul). What we get here is a critique of that strand of exclusive humanism that secularized agape, giving us the universalized "agape-analogue" (pp. 369–370; cf. 27). What we get from this Enlightenment formalization or secularization of Christian sensibilities is "a secular religion of life" (p. 371)—and it is *that* to which the post-Schopenhauerian strains of counter-Enlightenment are reacting.[17] On their account, Kant is still immature; still blind to the harsh realities of our cold, cruel universe; thus, still captive to slave morality, unable to be a hero (p. 373). It is worth noting that this post-Schopenhauerian vision is still a minority report in contemporary Western culture, however. But one can see the countermovement already within modernity itself.

We have arrived at a new place in human history: "A race of humans has arisen which has managed to experience its world entirely as immanent. In some respect, we may judge this achievement as a victory for darkness, but is a remarkable achievement nonetheless" (Taylor 2007, p. 376).

Conclusion

But Taylor (2007) also gives us reason to suspect that scientism is ultimately not viable—that there is a "fullness" and persistent transcendence that haunts us, even in our secular age. Such a closed "take" on the world can't seem to get rid of a certain haunting, a certain rumbling in our hearts. There is a specter haunting our secular age, "the spectre of meaninglessness" (p. 717)—which is, in a sense, a dispatch from fullness. And because this won't go away, but rather keeps pressing and pulling, it generates "unease" (p. 711) and "restlessness" (p. 726). That pressure tends to make scientism shout all the more loudly. We can bear witness to transcendence, not by shouting back, but by patiently and carefully pointing out the problem with scientism, thereby leveling the playing field and making room for our age to hear a still small voice that whispers beyond the molecules, inviting us to so much more.

Notes

1 This is why theological claims generated by the theology/science dialogue tend toward versions of a "natural theology." For two robust—but very different—Christian critiques of the very project of natural theology, see Alvin Plantinga (1980), "The Reformed Objection to Natural Theology" and Stanley Hauerwas (2001), *With the Grain of the Universe: The Church's Witness and Natural Theology*. I might note that the line of argument I'm sketching in this brief chapter could be read as a "natural science" equivalent of John Milbank's radical critique of the alleged "neutrality" of the *social* sciences in *Theology and Social Theory* (1991). For a further unpacking of that, see Conor Cunningham (2010), *Darwin's Pious Idea: Why the Ultra-Darwinists and Creationists Both Get It Wrong*.

2 Consider, for example, the trope found in a recent article published in *Perspectives in Science and Christian Faith*, in which we are told about the radical theological revisioning that is required based on the "secure findings of science." See Daniel Harlow (2010), "After Adam: Reading Genesis in an Age of Evolutionary Science," at p. 192.

3 No Christian scholar is going to explicitly assert that she or he is setting science "over" Scripture. I'm suggesting that we need to move beyond explicit claims about how they perceive the relationship between science and theology and instead look at the implicit *function* of science within their proposals.

4 There is a political correlate to this: those who simply accept the paradigm of liberal democracy will assert that Christian claims in the public sphere need to be "disciplined" by the expectations and strictures of democracy.

5 This, in fact, is much too simplistic, but will have to remain heuristic for now. The nature/culture distinction is not so neat and tidy. For instance, I don't mean to suggest that "culture" is just a kind of layer on nature; nor do I mean to suggest that cultural animals aren't always already "natural" animals. But we can't do justice to these issues in this brief conversation starter. If I had more time I would pursue this in dialogue with Bruno Latour's notion of "hybridities." See Latour (1993), 85–111.

6 This should be a relatively noncontroversial claim. For a discussion of culture in terms of *making*, see Andy Crouch (2008), *Culture Making*.

7 Though certainly lines of research (e.g., accounts of altruism in evolutionary psychology, or more recently neuroscientific accounts of religion in Barrett 2004) have sought to suggest that "religion" is a kind of natural outworking of the sort of biological creatures that we are. I'm agnostic about these proposals at this point. In any case, this wouldn't harm the thesis here since the point is just that in some significant sense "culture" is nature "plus" something, not *instead of* nature. Of course, the human work of culture making is made possible by a substrate of biological capabilities.

8 It's even tendentious to keep talking about science as if it were some monolithic reality. Just what makes neuroscience, physics, and ecology part of the same thing, "science"? My thanks to Matt Walhout for continuing to press this point.

9 I think this is well attested by Bruce Alberts' recent editorial, "Designing Scientific Meetings," *Science* (February 15, 2013), p. 737.

10 Robert P. Crease (1993), *The Play of Nature: Experimentation as Performance*, esp. 74–102. My thanks to Arie Leegwater and Matt Walhout for pointing me to this resource.

11 Rouse rightly emphasizes that "there is no such thing as the 'social world' (or the 'natural world') except as reified abstractions from *the* world" (2002, p. 173).

12 I can't begin to do justice to Rouse in this context. For further discussion that is particularly relevant in this context, see Matthew Walhout (2010), "Looking to Charles Taylor and Joseph Rouse for Best Practices in Science and Religion."

13 I think one of the great missed opportunities, so far, is the lack of any serious engagement between Rouse and MacIntyre, which I think would prove especially important to Christian theorists.

14 This sounds like teleological language to me—which is why Rouse immediately emphasized that claiming that practices have something at issue/at stake does *not* challenge his earlier claim to their openness (1996, p. 142). I would agree:

teleological orientation does not equate to "shutting down" surprise (contra Derrida).

15 See Alvin Plantinga's robust argument in *Where the Conflict Really Lies: Science, Religion, and Naturalism* (2011).

16 One gets the sense, however, that Taylor thinks there are diminishing returns on this: that something in the universe is going to keep pushing back, and that something in ourselves is not going to allow us to be satisfied with what looks like "freedom." One might suggest that Jonathan Franzen's *Freedom* gets at the same malaise.

17 I wonder whether one could read Michael Chabon's and Amy Chua's critiques of modern parenting as a kind of cultural expression of a similar reaction to the politics of politeness that we get from a secular religion of life.

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