STONE TOOLS AND FOSSIL BONES

The stone tools and fossil bones from the earliest archaeological sites in Africa have been used over the past fifty years to create models that interpret how early hominins lived, foraged, behaved, and communicated, and how early and modern humans evolved. In this book, an international team of archaeologists and primatologists examines early Stone Age tools and bones and uses scientific methods to test alternative hypotheses that explain the archaeological record. By focusing on both lithics and faunal records, this volume presents the most holistic view to date of the archaeology of human origins.

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Stone Tools and Fossil Bones

Debates in the Archaeology of Human Origins

Edited by

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CAMBRIDGE
UNIVERSITY PRESS

CAMBRIDGE UNIVERSITY PRESS
Cambridge, New York, Melbourne, Madrid, Cape Town,
Singapore, São Paulo, Delhi, Mexico City

Cambridge University Press
32 Avenue of the Americas, New York, NY 10013-2473, USA

www.cambridge.org
Information on this title: www.cambridge.org/9781107022928

First published 2012

Printed in the United States of America

A catalog record for this publication is available from the British Library.

Library of Congress Cataloging in Publication Data

Domínguez-Rodrigo, Manuel.
Stone tools and fossil bones : debates in the archaeology of human origins / Manuel
Domínguez-Rodrigo.
p. cm.
Includes bibliographical references and index.
ISBN 978-1-107-02292-8 (hardback)
1. Tools, Prehistoric. 2. Human evolution. 3. Social evolution. 4. Fossil hominids.
5. Human remains (Archaeology) 6. Animal remains (Archaeology) I. Title.
GN799.T6D65 2012
930.1–dc23 2011041172

ISBN 978-1-107-02292-8 Hardback

To Mary, Gabriel, and Liam for everything they have given me

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Introduction

The archaeology of human origins has experienced profound change during the past three decades, mainly spurred by the development and implementation of new theoretical approaches and analytical techniques, which have enabled archaeologists to retrieve more accurate information from the archaeological record. Despite this progress, old debates on the origins of human behavior seem to be as alive today as they were thirty years ago. Even if they have contributed to the increase of knowledge during this time, they frequently appear to lack resolution, jeopardizing the outsider's perception of archaeology as a scientific endeavor capable of providing increasingly better answers for the most relevant topics involved in how we became humans.

Archaeology has the potential to be a scientific discipline, even if in practice it is seldom treated as such. Most broad-scope interpretations in archaeology are not usually scientifically derived, which has been a point of debate among archaeologists, who are divided in their perception of the field as a scientific discipline. Recently, controversy arose when the American Anthropological Association changed its statement of long-term goals from "The purposes of the Association shall be *to advance anthropology as the science that studies humankind in all its aspects*" (emphasis added) to: "... to advance public understanding of humankind in all its aspects." This highly criticized modification, deemphasizing the scientific nature of anthropology – and with it, archaeology – reflects the differences at the heart of most current debates in this discipline. The critics of this change have adamantly argued that it will have a further negative impact on the field; however, the new statement actually might be an accurate reflection of the current praxis of the discipline. If one evaluates most anthropological interpretation in terms of epistemological accuracy, one finds that anthropology is hardly ever scientific (see Bunge, 1998a, 1998b).

Crucial concepts and methodological approaches required in the standard praxis of natural sciences are uncommon in archaeology. In the field of Pleistocene archaeology, researchers frequently overlook the fact that interpretations without referential frameworks, and moreover interpretations without contrasting hypotheses, are epistemically incorrect. Such interpretations, induced from the archaeological data without setting up frameworks and hypotheses first, are merely speculations. Such interpretations of the Paleolithic record are abundant and convey the impression to the broader public that long-held debates are more speculative than scientific. We find this situation even at the most basic levels of inference. For example, in taphonomic analyses of faunal assemblages, cut marks have sometimes been deemed of little inferential use because they are highly stochastic in nature. This interpretation, however, stems from the fact that most researchers supporting this claim are using archaeological information in isolation, rather than with experimentally derived referential frameworks. Cut marks in the archaeological record seem stochastic because the archaeological record itself is stochastic. Each site has a particular taphonomic history, and that determines the way in which cut marks are represented. Using a large set of "unknowns" (sites) to make interpretations of other unknowns is epistemically unacceptable. When one compares a large set of archaeological assemblages, some of the most relevant variables determining this stochastic nature can be identified (Domínguez-Rodrigo & Yravedra, 2009). Conversely, researchers using experimentally derived data show that cut-mark variability has a much narrower range and that this can be easily accounted for by a small number of variables. Researchers connecting interpretations to experimental referential frameworks will create bridges capable of using cut-mark data efficiently. Some archaeologists, for example, might argue that specific patterns of cut-mark density and orientation reflect specific hominin behaviors (e.g., Stiner et al., 2011). In the absence of experimental data, however, they overlook other equally feasible alternative scenarios (e.g., such mark patterns being created by novice versus experienced butchers). Experimental work also shows how cut marks are distributed anatomically when hominids have primary rather than secondary access to defleshed carcasses. Researchers who have not undertaken any experimental work on butchery and make interpretations only inferentially, through comparisons of archaeological data alone (often of data that cannot be compared at the same level owing to each site's having its own taphonomic history), frequently disregard this body of data.

One of the more appalling examples of disregard of scientifically derived analogical frameworks can be found in Pante et al.'s (2012) recent work, which bases interpretations of cut marks on experimental assemblages in which only disarticulation was carried out or on experiments in which some butchery was carried out, but cut marks are quantified following nondiscriminant methods (see critique in Domínguez-Rodrigo, 2009). A long list of criticisms to this approach is ignored, as well as other more complete butchery experimental sets contrasting opposite hypotheses (Domínguez-Rodrigo, 2009). Ignoring these critical arguments and data may create the illusory impression that some interpretations are more scientifically supported when they are constructed on metaphysical foundations. These authors also uncritically use experimental data ignoring the effect of the heterogeneous variables involved (e.g., butchering tool type [metal versus stone], mark tallying method, novice or expert butcher) and the arguments provided by other researchers on the importance of the proper use of these variables (e.g., Domínguez-Rodrigo, 2003). These authors try to rescue the scientifically falsified wreckage of the passive scavenging hypothesis by using bootstrapping methods on statistically insignificant samples ($n = <10$), where bootstrap resampling is as statistically meaningless as other parametric approaches (Chernick, 1999). Finally, they end up defending interanalyst correspondence in mark identification only when excluding most researchers other than the ones who were trained by the same person. These assertions, which oddly find their way to certain peer-reviewed journals, do a poor favor to the scientific endeavor of our discipline.

Any interpretation of the prehistoric record must be, first and foremost, taphonomically sound. Renouncing the heuristics of taphonomy, especially when they do not support determined hypotheses, is the first sign of an unscientific approach and contributes to postmodern visions of archaeology as a way of creating discourse of the past only from the present. For instance, using the same example as above, one could question the utility of cut marks (and, by extension, of other taphonomic variables) by arguing that they are subjected to extreme variability, but doing so makes two serious mistakes. One is confounding the intrinsic variability of specific taphonomic processes (e.g., butchery) with the extrinsic variability caused by heterogeneously designed experiments (see Chapter 2, and also Domínguez-Rodrigo [2003], for further critique). The former is limited; the latter can be as extensive as human imagination. This situation is an artifact of method. The other mistake is advocating a top-down (as opposed to a bottom-up) approach, in which one renounces the heuristics of taphonomy and engages into postprocessual discourse by directly drawing inferences from other major theories without any scientifically supported direct link. This is epistemically unwarranted. Inferences from the archaeological record can be made only when we understand how it was formed and the behavior of the participating agents. Experiments over the years have unveiled a specific range of intrinsic variability for bone surface modifications during butchery, and taphonomists now have the tools to understand this variability. They can opt for using this analogical knowledge or present it as relative because it does not fit one's ideas, and continue elaborating empirically unsupported interpretations of the past. Strong theory is only strong when it is empirically grounded and archaeologically linked in an epistemically correct way. The purported equifinality in the interpretations of carcass acquisition strategies by hominins argued by some is such only when partially selecting the information and its supporting arguments, and by artificially limiting the heuristics of taphonomic research. The plethora of arguments and analogical frameworks that taphonomy has built over the past 30 years effectively breaks equifinal interpretive scenarios and provides an unbalanced list of evidence for hunting and scavenging hypotheses. There never was a fallout of this debate, and never has taphonomy provided as much information to address this issue as at present.

Archaeologists also commonly infer basic hominin behaviors from archaeofaunal assemblages based on taphonomically unjustified assumptions. For instance, anthropogenic bone breakage frequently is inferred from the presence of bone notches or cone fractures (e.g., Stiner et al., 2011). Other biotic agents, such as carnivores, can cause these marks. Differentiating these agents requires using experimental information, which is rarely done systematically. If the interpretation of archaeological data without justifiable referential frameworks happens at the most basic level of inference (e.g., how a bone is modified), what guarantees do we have that broader interpretations regularly published in journals (e.g., reconstruction of subsistence strategies) are epistemically valid? Theory should guide the inferential procedures, so that archaeologists have some degree of certainty that what they are interpreting contains an element of truth.

This book was created with the goal of providing methodological perspectives to suggest that theory and epistemics are crucial for the scientific praxis of archaeology. The book does so by critiquing the most relevant debates for the archaeology of early humans. Most of these debates are theoretical in nature and hinge on how archaeologists build their references and models to interpret archaeological sites. Theory should be more important today in the practice of our discipline than it was several decades ago. This book emphasizes this by showing how the two most conspicuous elements in early Pleistocene archaeology (lithics and fossil bones) can be used to develop alternative (sometimes opposing) interpretations. These interpretations depend highly on analogical frameworks. Crucial issues debated are: was meat eating a dietary change that enabled the adaptation of early *Homo*? Was meat obtained through hunting or scavenging? In the former case, hominins must have been cognitively more advanced than Australopithecines and chimpanzees, especially given that the types of animals that were consumed at early sites probably required different skills to be obtained from those hunted by chimpanzees. Can we use modern human and chimpanzees (as extremes of an evolutionary continuum) as referents for understanding these behaviors? What can stone tools tell us about the behavior of early stone tool–using hominins? What knowledge do archaeologists gain of the early human mind and cognition when studying those tools?

Given the emphasis on theory (and how archaeologists use analogy), this book often presents alternative views on similar topics. The reader must be aware that this does not reflect ambiguity or lack of resolution but demonstrates the debates between different interpretations, which contain different heuristics. Not all of the interpretations exposed in this book are equally correct. Let us escape from the ghost of postmodern discourse from the very beginning. The reader must judge from the arguments provided which ones are biased and which ones seem closer to the inapprehensible truth. As editor, the advice that I would like to give the reader is the following: trust the arguments for what they are worth empirically.

The book opens with a section containing a challenging chapter discussing what archaeology should incorporate into its operational structure to function as a science, as defined by the scientific realist school of thought. Scientific anthropologists have long held that their discipline can fit the epistemological requirements to qualify as a science. Some of the most adamant defenders of this assertion – processual archaeologists stemming from the New Archaeology theoretical current – argued that their approach to the past, using middle-range theory, enabled them scientifically to uncover crucial aspects of human behavior. By the time the middle-range theory was being applied (with various degrees of success) to several types of archaeological contexts, a new philosophical movement, scientific realism, became the dominant epistemological trend in modern natural sciences. This philosophy of science emphasizes a systemic approach to the investigation of questions created within theories, involving various degrees of heuristic resolution across a hierarchy of (intertwined) hypothesis types. The first chapter of this book argues that the predominant role of grand theories and the use of dependent hypotheses in natural sciences have been discarded from scientific anthropology and that this affects the criteria to be met to qualify as a science, according to scientific realism. An example is selected, a theory on the origin of human behavior, to emphasize that most of the various models proposed to explain the earliest archaeological record have traditionally been descriptive-analogical and that none of them has been structured in a systemic and testable way that could be defined as scientific. A new theory, drawing on some previously analogically derived criteria, is proposed suggesting that early human behavior emerged as a package of interrelated features triggered by the occurrence of cooperative behaviors leading to solidarity. A way of testing this theory is presented that follows scientific realistic criteria.

The remainder of the book is divided into two sections, one devoted to the study of faunal remains to reconstruct hominin behavior ("On the use of analogy I: The earliest meat eaters") and the study of lithics to reconstruct early human stone tool making and cognition ("On the use of analogy II: The earliest stone tool makers"). Both sections emphasize the use of analogy, because most interpretations in them are derived from analogical reasoning. The analogies debated in both cases are dual, coming from studies with chimpanzees and from modern human experimentation. The goal is to show how complementary or divergent interpretations can be when the analogies used are based on one primate or the other. The types of analogies used in this book are empirically derived from studies and experiments with humans and chimpanzees and are not descriptive but relational. The use of analogy in the context of faunal analysis and a critical discussion on how analogy is conceived in archaeology is shown in Domínguez-Rodrigo's contribution in Chapter 2. In it, it is argued that experimental archaeology embodies a large array of conceptual approaches. Only those methodological approaches maximizing comparability between experiments and case-specific archaeological problems are heuristically scientific. This reduces the range of analogies that can be applied to the past. The adequacy of analogies depends on how the conceptual premises of experiments are designed. A practical example of this is provided through the comparison of referential frameworks created to understand the utility of cut marks to reconstruct butchering behaviors.

This is followed by Egeland's contribution (Chapter 3), containing a summary of the studies on bone surface modifications and their application to the archaeofaunal assemblages to interpret hominin behavior. Many actualistic studies are discussed, and when applied to the Oldowan sites, they suggest primary access to carcasses by hominins. This interpretation seems to be well supported by these taphonomic data and is a position shared by most of the contributors to this book. The information shown here renders outdated the hypothesis of passive scavenging of defleshed carnivore kills. Egeland is cautious about whether primary access necessarily implies hunting but stresses that this should be considered a possibility.

The debate of whether hominins were hunters or scavengers is displayed in the two chapters that follow. Lupo's contribution (Chapter 4) defends the scavenging hypothesis with the interpretation that hominins might have enjoyed different types of access to carcasses before these were defleshed. Lupo summarizes the hunting-scavenging debate while de-emphasizing the importance of meat in early human evolution, owing to its sporadic obtainment by some modern foragers and to the observed variability in its contribution to human diet. Lupo stresses that many behavioral features associated with meat eating, such as food sharing and social organization, should be carefully considered.

Pickering and Bunn's (Chapter 5) contribution picks up where Egeland's leaves off (with a summary of the taphonomic evidence for hominins' primary access to small and medium-sized carcasses) and elaborates on the possible ways that early stone tool–using hominins might have engaged in hunting. For Pickering and Bunn, the hunting-versus-scavenging debate is somewhat obsolete, because they argue that the available taphonomic evidence overwhelmingly suggests that in the few anthropogenic Early Pleistocene sites, hominins were not passive secondary agents in carcass access and acquisition (see also Bunn & Pickering, 2010).

In contrast to the chapters that draw on analogies to modern human behavior, in the last contribution of this section (Chapter 6), Pickering and Domínguez-Rodrigo use chimpanzees to develop a referential framework for early human meat eating and hunting. The use of referent taxa, and especially chimpanzees, in modeling human evolution has been harshly criticized. No doubt, chimpanzee data are often misused in models of early hominid behavior, but those misuses are examples of careless, formal analogizing. In contrast, it is equally possible to create nontrivial chimpanzee analogies. These analogies can in turn be linked together to construct credible models of human evolution, from which emanate hypotheses that are testable using paleoanthropological data. Unique among potential referents, chimpanzees are very closely related to early hominids, and some populations reside in ecological contexts that are comparable to those of our African ancestors. These two variables form the core of evolutionary behavioral ecology. Pickering and Domínguez-Rodrigo use chimpanzee and early hominid continuities and employ nontrivial analogies to provide a model of basal hominid hunting. The model is testable, and the topic is worthy because hunting and meat eating are argued by some to be the basis of human sociality.

The next section, focused on lithics, analyzes the emergence of Oldowan and Acheulean stone tool industries. Following the previous contribution, Chapter 7 emphasizes the use of analogies derived from the studies of chimpanzees and stone tool use. Carvalho and McGrew stress that chimpanzees are good analogies for Oldowan toolmakers because they generate an abundant lithic record through their nut-cracking activities, although their low-density loci may frequently be hard to detect archaeologically. Braun's (Chapter 8) review on the Oldowan stresses the lack of consensus on how the lithic data inform our perception of hominin behavior. Braun adopts a behavioral ecological approach to the understanding of stone tools, trying to underscore the positive contributions from functional/adaptive perspectives and more technologically oriented *chaîne opératoire* approaches. The inconvenience of the latter approach is that although it emphasizes understanding the knapper's intentions, it is difficult (Braun says "impossible") to independently test whether these intentions have been identified correctly. The behavioral-ecological and cognitive-technological approaches seem epistemically divorced. Braun argues that other constraints add to the difficulty in interpreting these assemblages. For example, the Oldowan is the result of a behavior that has no modern analogue (neither modern humans nor chimpanzees are adequate proxies), and cognition is hard to interpret.

Chapter 9 further elaborates on precisely this topic. Díez-Martín and colleagues present a critical reassessment about inferences on planning capabilities and predetermination skills of hominins of this period and demonstrate a way of contrasting interpretations scientifically. Predetermination of flaked products has been considered a hallmark of complex cognitive skills in human evolution. Traditionally, the landmark of this has been the Levallois technique and its products, which become unambiguously detectable in the Late Middle Pleistocene. A few years ago, the industrial assemblages from the type section area (Maritanane, Peninj, Tanzania) were used to argue that predetermination of flaked products was observable in East Africa during the Early Pleistocene. The conceptual consequences of this would be revolutionary: hominins would have planned the complete series of knapping steps prior to detaching any flake from a core and would have carried them out successfully until cores were exhausted and discarded. This would reflect not only great technical skills but also in-depth planning, because such behavior would be expected in environments where raw material availability was restricted, thus limiting the free exploitation, use, and discard of artifacts by early *Homo*. These important interpretations were never framed in a hypothesis-testing, scientific way, and no experimental background was provided to support them. In Chapter 9, two experimental strategies, aimed respectively at predetermining the reduction sequence and at opportunistically exploiting the geometric shapes of flaked cores, show that most of the criteria applied to discern predetermination in the Peninj assemblages are deficiently supported. Although technically possible, it is shown that the available evidence provides no unambiguous argument that can be used to infer that the Early Pleistocene hominins at Peninj displayed predetermination of complete reduction sequences involving the use of hierarchical surfaces from beginning to end of core exploitation.

Linking the discussion of the Oldowan to the emergence of the Acheulean, Chapter 10 reviews the impact of the type-fossil paradigm in the studies devoted to the African Acheulean and exemplified in the well-known debate on the developed Oldowan/Acheulean interface in Olduvai gorge. An in-depth discussion on the validity of Clark's technological mode conceptual framework for the study of the Acheulean is developed to scrutinize the recurrent overestimation of the hand ax and large cutting tools in the definition of the African Early Acheulean and the urgent need of a more holistic definition of the technical procedures that are behind the dawn of the Acheulean technocomplex. This chapter finishes with a brief review of the current state of our knowledge on how the early Acheulean appeared and suggests further research avenues covering issues such as the technological characterization of this stone tool complex, paleoenvironmental settings, regional analyses, depositional contexts, functional studies, and experimental approaches.

These chapters contain a critical summary of each topic and state-of-the-art arguments to support the interpretations that they contain. They show not only how much early Pleistocene archaeology in Africa has advanced but also how much work remains before it turns into a fully developed scientific discipline capable of providing answers to the main questions about how we became humans. This book was created to contribute to this development, and with two goals in mind: to emphasize that knowledge of the past can be reliably obtained only if derived, from data collecting to final interpretation, through scientific methods (and, hence, paraphrasing Willey and Phillips [1952], that archaeology is a science or it is nothing), and that to achieve this goal, analogical referential frameworks are crucial.

In the past few years, an increasing trend toward minimizing debate in meetings and peer-reviewed journals contrasts with previous efforts in debating anthropology more than thirty years ago. We are in postmodern times, and this might have something to do with this situation and its inherent perception of what should be politically correct in academia. Debating is an intrinsic part of what science is. Those of us who believe in the scientific nature of anthropology see debate as an opportunity to generate knowledge. This book was created from this perception of anthropology, and readers will benefit from it.

Putting this book together was not easy. I am most thankful to (in alphabetical order) J. Baena, D. Braun, H. T. Bunn, S. Carvalho, F. Cuartero, F. Díez-Martín, C. P. Egeland, K. Lupo, B. McGrew, T. R. Pickering, D. Rubio, and P. Sánchez for their excellent contributions. They have shown that despite differences of opinion, scientific debate keeps this discipline healthy. I am personally indebted to T. R. Pickering for his friendship and insightful exchanges regarding the contents of this book. I thank L. Perkins and the Taylor & Francis Group for their permission to use a paper published in *World Archaeology* (n. 40: 67–82; http://www.informaworld.com; "Conceptual premises in experimental design and their bearing on the use of analogy: an example from experiments on cut marks"), which was expanded into a new version (see Chapter 2). Finally, my deepest appreciation to M. Prendergast, as usual, for her insightful comments and constant support.

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CHAPTER 1

Toward a scientific-realistic theory on the origin of human behavior

Manuel Domínguez-Rodrigo

For almost half a century, anthropologists have debated to what extent their discipline can be considered a science, and if so, whether it should be viewed as a social science with different methods and heuristics from natural sciences (Binford, 1962, 1968; Aberle, 1968; Clarke, 1968, 1972; Watson et al., 1971; Schiffer, 1975; Gould, 1978, 1980; Zeitlin, 1990; Wylie, 2002; Turner, 2007). In the current state of affairs, and despite the firm belief that some anthropologists (mainly processualists) have that our discipline is a scientific endeavor, there are reasons to think that even processualists have not gone far enough to render their approach to anthropology fully scientific. The belief in systemics, the use of statistics, and the contrasting of null hypotheses are not enough to qualify as a science. A scientific undertaking requires a well-defined theory, articulated around a set of well-contrasted hypotheses in which every premise and assumption are clearly defined (Bunge, 1998a). Theories abound in archaeology; as a matter of fact it would not be an exaggeration to claim that there are as many theoreticians as there are field researchers these days (note that these terms should be complementary, not mutually exclusive). Most of these theories could be better served, however, by being described as theoretical positioning or nonscientific theories rather than scientific theories per se, because most of them lack well-articulated contrasting sets of hypotheses. One could subscribe to Marxist, historical-cultural, cultural-materialist, functionalist, or any other theoretical school; however, the main axioms of these schools' theories remain untested, tested but not supported by evidence, or plainly not testable (Popper, 1957, 1965, 1972; Bunge, 1998a, 1998b; Psillos, 1999).

Thus, anthropologists are often perceived by philosophers of science as researchers who are

> [d]iffident regarding theory . . . making hypotheses, which they often dignify with the name of theories. . . . Actually, aside from vague "grand theories" such as evolutionism, diffusionism, functionalism, conflict theory and cultural materialism – all of them largely programmatic hypotheses that have inspired fruitful research projects – anthropology contains hardly any theories, that is, hypothetico-deductive systems. . . . The typical research project in anthropology is a fact-finding mission. (Bunge, 1998b, 47)

Some processualists (see Binford, 2001) have strived to emphasize that programmatic questions produce hypotheses that must be tested against the empirical evidence of the archaeological record and interpreted under the analogical dynamics of referential frameworks. Theory thus should be of utmost relevance. Partly to accomplish this purpose to a certain degree, the middle-range theory was borrowed from Merton's (1967) work in sociology and articulated into several uniformitarian forms: ethnographic analogy, ethnoarchaeology, experimental archaeology, and neo-taphonomy, among others (Atici, 2006). The systemic concept of human culture itself to which this methodology was applied (New Archaeology) was never scientifically tested in a successful way, however, but was inferred from patterns of relatedness among selected variables for which the available information is heterogeneous (Binford, 2001). This differs from assumptions derived from systemic relations among selected behavioral variables phrased in the form of testable hypotheses, with their corresponding falsifying premises. This shortcoming should be emphasized, despite the acknowledgment that a systemic approach to human behavior is the only way to understand patterns and regularities in it (Bunge, 1982, 1998b).

Although traditional critics and even some supporters of the processual approach emphasize its logical positivist foundation (Deetz, 1970; Flannery, 1973; Read and LeBlanc, 1978), the rejection of the metaphysical components of theories, the emphasis on hypotheses not necessarily articulated over theories, and the necessity of justifying *all* of the assertions of interpretations empirically, as defended in the logical positivism philosophy, conflict with the emphasis that processualists in their early years placed on theories with all their metaphysical components (although one can argue that most of them were not necessarily scientifically articulated). The New Archaeology thus "diverged sharply from the deductivist models of explanation and confirmation associated with such latter-day exponents of logical positivism/empiricism as Hempel" (Wylie, 2002, p. 81). Furthermore, the conceptual schemes and procedures of processualists are better understood within "scientific realism, a theory of science that . . . offers a much more congenial framework for the New Archaeology than does Hempelian positivism" (Wylie, 2002, p. 24). I concur with Wylie (2002) that processual anthropology better fits the epistemic criteria of scientific realism than those of positivism but argue that it is still far from adopting a scientific realistic framework for its praxeology.

Although much closer to a scientific approach than other theoretical approaches, the classical processualist approach, as is discussed later in this chapter, also lacks complete epistemological justification and does not quite fit into what is currently understood as a scientific theoretical body of knowledge, as is defined in scientific realism, which is the pragmatically dominant philosophical epistemic school in natural sciences (Putnam 1972, 1975; Bunge, 1973, 1982, 1998b, 2006; Byerly and Lazara, 1973; Toumela, 1973; Boyd, 1983; Miller, 1987; Niiniluoto, 1987, 2002; Lipton 1993; Aronson et al., 1995; Bhaskar, 1998, 2007; Psillos, 1999).

Scientific realism requires logic reasoning that is organized systematically. Scientific systematization implies the use of concepts within hypothetical constructs (containing inferable entities or properties), which should avoid vagueness and be properly defined if they are to be meaningful. These concepts should form theorems deduced from prior axioms or founder hypotheses. The systemic articulation of theorems or hypotheses containing theoretical well-defined concepts creates factual theories (Bunge, 1998a; Niiniluoto, 2002). These systemic articulations (theories) constitute the body of knowledge within which problems at the root of research arise. These problems lead to new hypotheses with testable consequences that after testing can be evaluated, providing corroboration or rejection and a subsequent increased body of knowledge. This is what is epistemologically called the method of successive approximation (Bunge, 1998a), truthlikeness (Niiniluoto, 1987), or lineal (evolutionary) progression of knowledge (Popper, 1972). Inherent in this widespread conception of "theory" is the interpretation of theories as approximate images of reality containing webs of laws or patterns. This renders theories as modeled patterns of reality. Because the goal of a theory is not to provide an answer to exceptions and oddities, theories should be interpretive patterns explaining laws, regularities, and general processes of reality (Niiniluoto, 1987; Psillos, 1999). Exceptions to these laws or patterns must be explained with auxiliary hypotheses. These hypothetic-deductive systems are the only ones that can generate enough background knowledge to provide new problems for research.

Such hypothetic-deductive systems are poorly defined in anthropology. In the case of anthropological research, theories are weak because they lack clear definitions with which to start. As Bunge (1998b) acknowledges, the very first problem anthropologists face is the definition of their object of study: What is man? What is defined by *human*? Without clear definitions, there are no valid theoretical concepts, and therefore no theories. Regarding the latter question, an answer for a definition of human behavior from an evolutionary perspective was provided by Isaac (1978) when he identified as human all the novel features that humans do not share with the rest of primates. This exemplified a widespread concept of interpretation in anthropology, which is using analogic reasoning following the uniformitarian principle of actualism (Binford, 1962, 1978, 1981, 2001). What was missing from Isaac's diagnosis, however, was a way of applying those features to the archaeological record by means of a well-articulated scientific theory. A similar criticism could be applied to models of modern human behavior (reviewed in Henshilwood and Marean, 2003), which are based on analogically derived lists of ethnological and archaeological traits.

My modest goal here is to provide an example of an articulated theory for the origin of human behavior that could be used as the contrasting body of knowledge against which research problems can be created and potentially solved through palaeoanthropological research. Thus, a scientific approach to this issue could be more successfully defended than it has been so far and would encounter less epistemological criticism by philosophers of science subscribing to the realistic school of thought. To carry out this task, I intend to use formal logic and the principles of theory building as described by scientific realists (Niiniluoto, 1987; Aronson et al., 1995; Psillos, 1999), with special emphasis in the guidelines provided by a variant of scientific realism: hylorealism (Bunge, 1998a, 2006). I do not intend here to champion this approach over any other theoretical approach; my argument is that it is more scientifically sound than previous science-related (processual) approaches regarding the issue of the emergence of human behavior. Intended mainly as a guideline on how to construct scientific theories in anthropological research, the founding theoretical body presented here must be subjected to scrutiny by future research and "elucidated" both by new theoretical axioms and problem-testing processes. My prediction is that, if accepted, it will be modified, implemented, and better defined as expected by a philosophical perspective based on an evolutionary concept of knowledge.

TABLE 1.1. *Principal elements for a definition of human behavior (drawn from comparison with nonhuman primates)*

Socioreproduction		A. Groups composed of family units [male and female(s)]
Cooperation*	Unit cooperation	B. Males and females cooperate in bringing up offspring: food-sharing type I
	Group cooperation	C. Family units cooperate for obtaining resources: food-sharing type II
Subsistence†	Omnivorous quality diet	D. Dependence of meat eating: >40% of energy comes from meat
	Tools	E. Dependence on tools for survival
	Central places	F. Spatial adaptation: home bases
	Complex thinking	G. Planning and forethought: anticipation of future needs
		H. Symbolism: abstract thought
	Communication	I. Articulated phonetic language
Locomotion		J. Committed terrestrial bipedalism

[*] Cooperation involves labor division, which can be intersexual (hunting and gathering) or intrasexual (different activities individually assigned).

[†] Subsistence is defined here as basic adaptive behaviors, artificially separated from cooperation, to emphasize the prominent role of the latter.

Defining the concepts and formulating the hierarchy of the theory components

A theory requires a hierarchy of theoretical factual concepts, organized systematically. At the apex of a theory on the origins of human behavior is the generic ontological concept of "human behavior." If we recall Isaac's (1978) diagnosis and expand it, we can structurally define human behavior, as opposed to nonhuman primate behavior, following the categories and components shown in Table 1.1. This reasoning is analogical and compares human and nonhuman primates, and the differences are the defining features. Such an analogical reasoning cannot be uncritically applied to the past, however. First, it is not articulated systematically within the body of a theory. Second, it does not define how many of these features are necessary to document in the evolutionary record to qualify as "human." Third, it does not specify whether these features evolved independently or are interrelated. Fourth, no description was provided of how these features (converted into hypotheses) could be tested and what their falsifying premises would be. It is in these four lacking points that all strict analogical efforts applied to the interpretation of the archaeological record fail.

The formulation of a scientific hypothesis using Isaac's diagnostic criteria would imply placing human behavior within an evolutionary framework. Several theories could be used. For the sake of scientific development, I elaborate on one, acknowledging that alternative theories could be provided. The heuristics of those theories could potentially be confronted with the one defended in the present work, making up different research programs submitted to testing and subsequent differential explanatory power (Lakatos, 1978). I am arguing that the origin of human behavior can be traced to the moment in which hominins shifted their subsistence toward a cooperative social organization, resulting in solidarity. *Cooperative* here means coordinated participation of all adult individuals in various subsistence activities. *Solidarity* refers to the end result of the expectation created by cooperation (Rankin and Taborsky, 2009), in which the outcome of any collective enterprise results in communal benefit. This should apply especially to the energy obtained through food and its relevance in adaptation. None of these features figures prominently in the behavior displayed by any extant nonhuman primate and currently is the basis of the structure of the behavior of our species (Quinlan, 2008; Gurven and Hill, 2009).

A main theory requires, following traditional logic, a set of axioms postulated in the form of subsidiary smaller theories, also referred to as *founder hypotheses* (Bunge, 1998a). These serve as a starting point for deducing and inferring other subsidiary hypotheses that are made hierarchical relative to the founder hypotheses. As can be seen in Table 1.2, a set of axioms is presented as the pillar of the main theory. These axioms are not directly testable and must be supported by testable lower-hierarchy hypotheses, also referred to as *intermediate- and lower-level theorems*, and those that can be empirically tested in a direct way are referred to as *factual hypotheses* (Bunge, 1998a). The formal unity of a scientific theory consists of the existence of logical relations among the formulas of the theory, such that no formula remains isolated but remains fully integrated in a hypothetic-deductive body, which Bunge (1998a) referred to as the nervous system of a scientific theory.

The axioms in Table 1.2 need to be defined and elucidated properly, that is, we need to sharpen their meaning to avoid confusion or ambiguity. This is a crucial step in the building of theories, because it provides meaning for each axiom and subsequently helps to make well-defined falsifying premises. The first axiom (intentional food sharing) refers to a subsistence

TABLE 1.2. *A proposed theory for the origin of human behavior*

Main theory	**Human behavior emerged when subsistence was based on a cooperative and solidarious social organization.**
Founder hypotheses	The key elements of the main theory are
Axioms	A. Intentional food-sharing was the main goal of cooperation. B. Special places were selected where food sharing took place (central places) or where the yield of cooperative actions was commonly exploited (referential places). C. Meat eating was important: it would provide (one of) the resource(s) to be shared in central places. D. Some resources were collectivelly obtained and transported (e.g., food, raw materials). E. Dependence on tool use. F. Planning and anticipation of future needs to a higher degree than documented among nonhuman primates. G. It is argued that axioms A to F emerged simultaneously within the same system.

strategy in which each individual belonging to a group forages with the intention of sharing an important part of the food obtained with other individuals within the reproductive unit and within the group. Ideally, the energy invested by each individual in obtaining food to be shared should account for a minimum of 30% to 50% of the total energy on which each individual's subsistence is based. This substantially differs from the food transfer documented among nonhuman primates (referred to by Isaac [1978] as *tolerated scrounging*), in which no energy is invested with the intention of obtaining food for others.

Food sharing requires food obtained in different places to be brought to a fixed place where it can be distributed. Hence the emergence of *central places*, which should be protective spots yielding safety for group fusion and where butchery and other activities might have taken place. I avoided the term *home base*, because the evidence of those spots acting as the focus of the remainder of social life and for sleeping remains archaeologically elusive. A *referential place* is a spot used by hominins for collective or individual activities other than food sharing that involves all members of the group or part of it, with the result of those activities yielding energetic benefit profited by more than one individual. These should be subordinated to central places. Several types of reference places can exist, which leave material traces that can be interpreted (see Tables 1.3 and 1.4).

TABLE 1.3. *Hypotheses and their testing premises, composing the theory of the emergence of human behavior suggested in the present work*

Founder hypotheses	Factual hypotheses	Versions	Propositions	Testing premises	Falsifying premises
A. Intentional food sharing was the main goal of cooperation					
	A1. Primary access to animal resources				
			Presence of filleting marks		Further experimentation replicating secondary access to carcasses in nonanthropogenically disturbed ecosystem (Domínguez-Rodrigo, 2008) shows a different distribution and frequency of cut marks.
				Cut-marked long bone frequency >10% (optimal range = 15%–30%) Cut-marked meaty long bone mid-shaft frequency >50% of all cut-marked long bone specimens Cut-marked upper limb bone shafts >15% Presence of cut marks on “hot zones” as experimentally modeled	
			Presence of disarticulation marks		New experiments suggest that disarticulation in defleshed carcasses passively scavenged is energy efficient from an optimal foraging point of view.
				Cut marks on epiphyseal portions linked to ligament cutting	
			Presence of evisceration marks		New experiments with passively scavenged defleshed carcasses yield evisceration marks.
				Cut marks on ventral side of ribs	
			Presence of percussion marks		Absence or significantly lower presence of percussion marks in human-first experimental scenarios
				Percussion-marked long bone shaft specimens 10%–35%	As documented by Blumenschine and Selvaggio (1988, 1991)

Bone breakage caused by dynamic loading Acute/obtuse oblique breakage planes Notch measurements Notch type distribution	New analogical frameworks provide different measurements and frequencies of types.
Lack of felid patterns in tooth-marked specimens	New experiments show felid-like tooth-marking patterns when carnivores have secondary access to hominin-deposited remains.
Taxonomic diversity in animal exploitation	New experiments replicating secondary access to felid-consumed carcasses provide a diverse range of carcasses showing the same signature as indicated by the previous propositions.
A1a. Hunting	
Age profiles different from the prey of targeted carnivores	New studies show same age profiles as carnivores' prey.
A1b. Confrontational scavenging	
Age profiles similar to the prey of targeted carnivores Differential anatomical distribution of tooth marks and cut marks in intermediate stages of carcass consumption by carnivores	New studies show identical signatures when exploiting passively scavenged carcasses.
A2. Focus on a range of carcass sizes from 1 to 3–4	
Accumulation of of animals spanning these carcass sizes, with emphasis of those >100 kg. This hypothesis is linked to A1 and therefore has to yield the same taphonomic signatures.	New studies produce evidence of nonanthropogenic accumulations showing the same taphonomic signatures as 1A, when passively scavenging carcasses ranging from size 1 to 3–4.

(*continued*)

TABLE 1.3 *(continued)*

Founder hypotheses	Factual hypotheses	Versions	Propositions	Testing premises	Falsifying premises
B. Selection of central places for sharing food and referential places for communal use					
	B1. Selection of central places				
				Allochthonous taxa (not belonging to the same habitat where the site is located) are present.	Allochtonous taxa can be explained by nonanthropogenic carcass transport or accumulation.
				Bone accumulation significantly higher than background landscape scatters	Bone accumulation is indistinguishable from background scatter density.
				Bone accumulation qualitatively different from nonanthropogenic bone clusters	No difference
				Bone accumulation involving multiple individuals (this and the previous two points should indicate carcass transport)	Taphonomic evidence against a food surplus: scavenging from carnivore kills
				Selection of location affording protection from carnivores (low trophic dynamics)	Palaeoecological location shows lack of protection and high trophic dynamics (high carnivore presence).
				Food surplus that would enhance food sharing	Food surplus unsupported by A1
				Short depositional time (ideally no more than one or two years)	Long depositional time span, involving several years
				The previous seven points are interdependent.	
				Evidence of other activities not related to animal carcass consumption performed at the site	

B2. Selection of referential places			
	Evidence of hominin reoccupation several times (e.g., display of various weathering stages on bone with signs of having been modified by hominins)		No documentation of these propositions
		Hominid imparted marks on bone (cut and/or percussion marks)	
		Articulated remains, bones from same carcasses not scattered (clusters)	
	Sites consisting of only lithic remains		
		Lithics showing early stages of reduction sequences with later sequences missing, providing information of the potential amount of raw material that might have been transported if not present at the site	
	Multilevel sites consisting of just lithics or lithics plus other materials (irrespective of whether there is a functional link or not)		
	Lithic remains discarded in time-averaged deposits spanning a minimum of hundreds of years		
C. Importance of meat eating			
C1. Abundant evidence of butchery			
	In taphonomically supported anthropogenic sites, systematic evidence of exclusive hominin flesh exploitation		Sparse evidence of butchery (as described in A1) in well-preserved anthropogenic sites
	Arbitrary threshold >50%		Redundant taphonomic evidence of secondary access to carcasses
	Indicators of butchery as described in 1A		

(*continued*)

TABLE 1.3 *(continued)*

Founder hypotheses	Factual hypotheses	Versions	Propositions	Testing premises	Falsifying premises
D. Collective obtainment of resources					
	D1. Transport of (almost) complete size 3–4 carcasses				Discovery of their accumulation on the spot by natural processes other than humans
			Skeletal part profiles showing even representation of high-survival set, or		Taphonomic evidence in favor of a death site
			Skeletal part profiles showing even representation of high-survival set (excluding skull)		
			Biometry of long bone ends according to taxon or tribe (relative to carcass size)		
			More exceptionally: abundance of part of the low-survival anatomical set		
	D2. Collective transport of lithic raw material				
			Abundance of lithic raw material exceeding transport capacity of one individual		Support for inferring multiple individual trips, especially in sites formed over redundant reoccupation for several years
E. Dependence of tool use					
	E1. Tools were needed for every subsistence activity.				
			Use of cutting tools (ideally a minimum of fifteen flakes per MNI should be present)		Discovery of the whole set of propositions in a nonhuman primate
			Evidence of long-distance transport of raw materials		
			Transport of materials in various stages of their reduction sequence		

	Intensive reduction of exotic or distantly sourced raw material		
	Use of tools without discard		
		Taphonomic evidence of cut-marked bone in absence of spatially associated tools	
F. Planning and forethought			
F1. Hominins anticipated adaptive needs.			
	Differential selection and use of raw materials		Discovery of the whole set of propositions in a nonhuman primate
		Differential reduction sequences and typologies for tools according to raw material type	
	Evidence of long-distance transport of raw materials		
	Evidence of transport of tools		
		Technological analyses show that tools were not made on site.	
	Use of tools without discard		
		Taphonomic evidence of cut-marked bone in absence of spatially associated tools	
		(Seasonal) reoccupation of the same spots	
G. Systemic nature of these hypotheses			
	Behaviors from axioms A–F are documented.		Documenting their emergence at different times during human evolution.

A central element in food sharing among modern hunter-gatherers is meat (Stanford and Bunn, 2001). It is therefore stressed that if food-sharing behaviors took place in the past, meat must have been a key element in them. For this purpose, meat consumption must have been a regular rather than a marginal (i.e., fallback) food type. By *regular*, it is meant that it was consumed year-round even if meat consumption peaked in certain moments (i.e., seasons) more than others.

Collective (cooperative) behaviors are a requirement prior to food sharing. They do not have to have been restricted to food obtainment and can be displayed in other activities. It is argued that the best way to trace them is to detect them in the obtainment and transport strategies of certain material elements that were required for the subsistence of hominins.

Our species is the only one on the planet whose survival and adaptability depends entirely on technology. It is also argued that one of our defining features involves not just use of tools in a more intense way than documented in other species (e.g., nonhuman primates) but also dependence on artifacts to the point where their removal from the behavioral repertoire would cause inadaptability. This idea of dependence might explain why stone artifacts appeared in human evolution. Archaeologists have been arguing for decades that stone tool use emerged in conjunction with meat eating as the essential butchering tools. Should meat eating be a marginal activity of hominins, then it should be inferred that stone tools were not used year-round and therefore that they were not essential to the survival of hominins. This suggests that this founder hypothesis should be studied in conjunction with the previous one.

Forethought and planning are described in terms of anticipation of future needs related to subsistence, reflected in the innovation of solutions prior to the appearance of the problem or need.

Finally, all of these axioms are conceived of within a systemic conception of behavior, as defended in the predominant behavioral ecology school of thought (Brooks and McLennan, 1991). This implies their interdependence and their emergence in a set, instead of each of them appearing at different times.

After each axiom has been defined as a founder hypothesis, which is the highest-ranked type of hypothesis, lower-ranked hypotheses stemming from the former are created to establish a link with the empirical record. These act as a bridge between the data and the founder hypotheses, which can be affected by various types of metaphysical components (and hence can be beyond direct testability). The factual hypotheses, because they concern facts and data (Bunge, 1998a), can be (1) analogical, when they emerge from the intuitive realization of similarities; (2) inductive, when they derive from examination of information; (3) intuitive, when their origin cannot be linked with either previous information or an analog; and (4) deductive, when they are derived from higher-ranked propositions. According to the degree of abstraction of a hypotheses, a further division can be made between observational and nonobservational hypotheses. The former are low-ranked hypotheses, because they are the first to make contact with reality through direct testing (factual hypotheses), whereas the latter are higher-ranked hypotheses, linking factual hypotheses to the main axioms of the theory. Hypotheses have also been described as phenomenological and representational, when addressing the external behavior of a theory without focusing on its working system and when they specify some mechanism, respectively (Bunge, 1998a).

Each of the factual hypotheses that make up the bulk of the theoretical body supported here, with some of their crucial testing and falsifying premises, can be seen in Table 1.3. The elucidation, sharpening, and definition of these hypotheses (*senso* Bunge [1998a]) are described in Table 1.4. I will comment on only some of the reasons why some variables and hypotheses commonly used by some researchers to tackle the first founder hypothesis presented here were excluded. Food sharing requires food surplus, which in turn, if regularly obtained, requires primary access to carcass resources in modern African savannas. I have used some taphonomic indicators as analytical variables (converted into silogistic propositions inside hypotheses), which emphasize the careful and correct identification of hominin-imparted signatures preserved in the fossil record (namely, cut marks). This goes against a common trend among some Plio-Pleistocene archaeologists who postulate equifinality scenarios for those signatures and emphasize indirect reading of hominin authorship in any given bone assemblage through the imprints created by carnivores (Blumenschine, 1995; Capaldo, 1995, 1997; Blumenschine et al., 2007; Ferraro, 2007; Pobiner, 2007). This may seem counterintuitive, and indeed it is. These authors claim that a high frequency of tooth-marked specimens on long bone midshafts is indicative of carnivores (namely, felids) having had priority in carcass exploitation and a significant input in the formation of bone assemblages, whereas a low frequency of tooth-marked specimens reflects the opposite, that is, the intervention of scavenging carnivores postdepositionally to ravage bones previously exploited by hominins. This assertion is incorrect for the following reasons, however: first, from a theoretical point of view it is aberrant, because to make the original claim of primary access by strict flesh-eating carnivores (felids, to enable hominins to intervene secondarily [as in Blumenschine, 1995]), experiments were conducted with durophage carnivores (e.g., bone-crunching hyenas) yielding high tooth-marking estimates, instead of with felids. Second, as expected, given the different tooth properties of felids and hyenids, taphonomic studies made on felid-eaten carcasses yielded much lower frequencies of tooth-marked broken bone specimens, indistinguishable from those abandoned by some durophage carnivores having secondary access to human-exploited bone assemblages (Domínguez-Rodrigo et al., 2007a, 2007b). Researchers relying on carnivore-made tooth marks therefore should look for low frequencies of tooth-marked midshaft specimens if interpreting that flesh-eating carnivores had access to carcasses before hominins did. Third, tooth marks on midshaft portions of long bones are subject to equifinality: most researchers assume that they were caused by nonprimate carnivores, but nobody has been able to exclude other tooth-marking agents such as nonhuman primates (Pickering and Wallis, 1997; Domínguez-Rodrigo, 1999), hominins (White and Toth, 2007), or suids (Domínguez-Solera and Domínguez-Rodrigo, 2009).

In contrast, the purported equifinality when using cut marks has been argued to be merely methodological; methods ignoring the real location of cut marks (linked to the behavior that caused them: filleting, dismembering, or skinning) and the type of bones where they occur yielded ambiguous interpretations (see extended discussion in Domínguez-Rodrigo [2009]), which can be overcome by using more accurate tallying methods (Domínguez-Rodrigo et al., 2007a). Furthermore, the use of "unknown" referents, such as other archaeological assemblages (which are assumed to be completely anthropogenic only because they are of late Pleistocene or Holocene age) is not only epistemologically wrong (an "unknown" cannot be used to explain another "unknown") but taphonomically unjustifiable. Many of those Holocene sites used as analogs are either very fragmented diagenetically or have poorly preserved bone surfaces, which affects mark frequencies drastically. These recent sites cannot be directly compared to assemblages that lack diagenetic breakage and have well-preserved cortical surfaces.[1] Domínguez-Rodrigo and Yravedra (2009) showed that variability in percentages of cut-marked bones is tightly related to several different variables, but when cortical preservation is controlled, there is an expected range of cut-marked specimens that enables the use of cut-mark frequencies

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[1] This, for instance, happens when using assemblages like SK400 (LSA, South Africa; Dewar et al., 2006) as referents of low frequencies of cut-marked bone (<1%) in anthropogenic assemblages (for instance, Ferraro, 2007), when almost one-third of the bones are burnt; the remainder seem to be fairly badly preserved, covered with root etching and sediment (Dewar et al. [2006], figure 5); and bones were not clean when their surfaces were analyzed for marks.

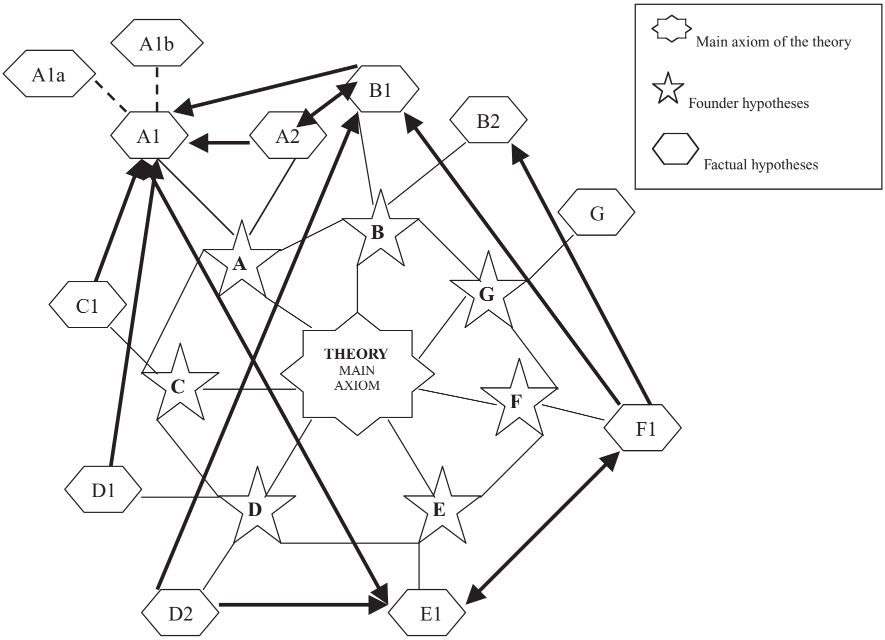

FIGURE 1.1. Interrelations among founder and factual hypotheses, with special emphasis on the links among different factual hypotheses (*thicker lines*). Direction of interdependence between hypotheses is indicated by arrows. See key for each hypothesis in Tables 1.2 and 1.3.

as a good indicator of the anthropic component of any given assemblage.[2] Ideally, comparisons among assemblages have to be made when it has been confirmed that all the variables involved are equally (un)biased. All the taphonomic components of the hypotheses contained in the theory that I present affecting bone surfaces are based on well-preserved assemblages and should be applied as analogs on the unbiased parts of archaeological assemblages; that is, the sample comprising specimens with well-preserved cortical surfaces.

The types of factual hypotheses proposed for the theoretical body presented here are observational (ten factual hypotheses) and nonobservational (seven axiomatic hypotheses; Figure 1.1). The factual hypotheses are either deductive (stemming from hypothetic-deductive inferences; A2, B2, D1, D2,

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[2] Domínguez-Rodrigo and Yravedra's (2009) comprehensive review of cut-mark frequencies in twenty-eight sites, including forty-four archaeological levels spanning different chronologies, based on the well-preserved green-fractured assemblages, excluding those specimens with poor cortical preservation, yielded an average of cut-marked long bone specimens of 19.9% (median = 19), standard deviation of 11, with 80% of the sample showing the following percentile (10–90) range of cut-marked long bone specimens: 6%–36%.

F, G) or deductive-analogical (A1, B1, C1, E). Strict analogical hypotheses have not been considered.

Hypothesis interrelatedness: The neural network of the theoretical body

Processual approaches to anthropological issues have provided hypothesis-testing frameworks, often testing opposite propositions beyond the null hypothesis. The most common pattern for anthropologists is to test hypotheses individually, however. This limits the heuristic capacity of each hypothesis given the common phenomenon of equifinality in historical and contextual processes. By linking hypotheses, this problem is frequently overcome, given that the heuristic value of each hypothesis is reinforced by those of the other hypotheses to which it is linked, and therefore on which it is dependent. For instance, the hypothesis that hominins focused on the exploitation of large carcasses (especially those larger than 100 kg; Hypothesis A2) is constructed on the premise that accumulations of animals these sizes must occur at sites. Taken in isolation, this assumption invalidates the hypothesis because other nonanthropogenic processes (e.g., natural deaths at ponds during droughts, accumulation of carcasses by certain carnivores in dens or death arenas) can produce the same result. If it is linked to hypothesis A1, the heuristic value of hypothesis A2 increases, because the co-occurrence of an accumulation of carcasses of that size range on the same spot with unambiguous evidence of having been primarily exploited by hominins invalidates the purported equifinality with a scenario of carcasses accumulated by carnivores (and initially consumed by them). If the link of A2 is extended to B1 (selection of central places), the other possible equifinality scenario of hominins exploiting naturally accumulated carcasses by catastrophic phenomena vanishes, because B1 requires taphonomic support for dynamic (transport-caused) processes in carcass accumulation.

The relationship between linked hypotheses can be of *reciprocity* (one hypothesis and another show similar degrees of dependence), or of *asymmetry* (one hypothesis is unidirectionally dependent on another without corresponding dependence on it). The linking can be double or multiple, involving several related hypotheses (Figure 1.1).

A neural network conception of hypotheses shows that the testing of independent hypotheses could be futile, because no hypothesis ever exists independently within a theory. Independent hypothesis testing is good for discarding falsified hypotheses but inappropriate for selecting among corroborated hypotheses (those prone to create equifinality scenarios).

Lyman (2004) correctly stressed that equifinality in open systems was a methodological construct.[3] Equifinality in such systems is not an inevitable situation. Current methods can be inadequate to differentiate causes from their effects, but alternative ones could potentially do so, because natural historical processes are open systems (Lyman, 2004). This conception applies perfectly to hypothesis testing. When two hypotheses have been corroborated (which should not be mistaken for confirmed) by testing, the only way to select between them is to provide different testing premises or methods, or as scientific realists advocate, to construct meta-hypotheses, which involve lumping the propositions of two originally separate but dependent (theoretically linked) hypotheses into one broader hypothesis (Bunge, 1998a). This increases the discriminatory power of testing.

The corroboration of single hypotheses also potentially increases the value of other hypotheses to which those are linked. The dendritic relationship of conceptual interdependence of hypotheses determines the heuristic value of theories.

In the theory constructed here on the origins of human behavior, most factual hypotheses show a direct link to the factual hypotheses from which they derive and also single or multiple dependence links to other hypotheses, as can be seen in Figure 1.1. This link is explained in Table 1.4. This theory can be used as a good demonstration that the contribution of each hypothesis to the main axiom of the theory is not even. Some hypotheses contribute more crucially than others. For instance, A1 shows mostly unidirectional links to almost one half of the other hypotheses, which indicates that 45% (five of eleven links) of the heuristic power of the theory depends on A1; that is, the extent to which high resolution can be reached in discerning primary access by hominins to large carcass faunal resources, producing the food surplus necessary to justify intentional food sharing by its repeated transport to the same locus.

How to measure the heuristic value of alternative theories?

Lakatos (1978) argued that programs of scientific research compete with one another according to their heuristic value, that is, their capability of explaining a bigger portion of reality. For those who argue that refutability is never absolute, the selection of hypotheses and theories should be made based on their capability of explaining things. Lakatos argued that

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[3] Lyman (2004) urged readers to look at equifinality from its original meaning as defined by von Bertalanffy (1956) when modeling general systems theory, that is, the “same final state from different initial states” in an open system.

TABLE 1.4. *Refined description of concepts contained in the factual hypotheses to be tested*

A1. Primary access to animal resources	It implies that carcasses are fleshed before hominins butcher them and that no other carnivore has already consumed them. This will be supported by filleting and evisceration marks (as defined by Binford [1981] and Bunn [1982], according to the experimental scenarios provided by Domínguez-Rodrigo et al. [2007a]), disarticulation marks (as defined by Nilssen [2000]), percussion marks (as defined by Blumenchine and Selvaggio [1988, 1991]), in proportions experimentally replicated in Galán et al. [2009]), dynamic long bone breakage and lack of felid bone-modifying pattern (as described in Domínguez-Rodrigo et al. [2007a]), and a diversity of macromammal species exploited, which would exclude any opportunistic strategy after the highly specialized felid predatory range.
A1a. Hunting	It refers to strategies in which hominins are actively engaged in killing their prey themselves.
A1b. Confrontational scavenging	It refers to carcass obtainment strategies in which hominins confront other carnivores in the early stages of carcass consumption by the latter to snatch it from them.
A2. Focus on a range of carcass sizes from 1 to 3–4	Food sharing is more justifiable if at any point there is a material evidence for a representation of a resource that would have exceeded the needs of one individual. The redundancy in this pattern, especially if occurring on the same spot repeatedly, would suggest that the finality of such behavior would have been food sharing. Carcass parts from animals weighing more than 100 kg obtained through primary access would be a good example. This is more justified if a good representation of carcass parts from single animals can be identified (high-survival set as defined by Marean and Cleghorn [2003]). Therefore the heuristics of this hypothesis are linked to A1 and B1. Carcass sizes are as defined by Bunn (1982).
B1. Selection of central places	A *central place* is defined as a locus repeatedly used (on a daily basis), to which resources are transported, processed, and discarded acting as the focal point of group fission/fusion. It requires an accumulation of remains in significantly higher density than the surrounding environment, involving various animals (some of them not present in the immediate habitat), and a justification of food surplus. It requires both primary access to these

TABLE 1.4 *(continued)*

	resources and evidence of large animal size. The heuristic value of this hypothesis depends on A1 and A2.
B2. Selection of referential places	A *referential place* is that to which hominins go with the goal of performing a specific activity (individually or collectively), which will yield communal benefit at some point. Examples of referential places are near-kill location places in some modern foragers as described by O'Connell (1997), knapping spots near or at the sources of raw material, loci where plant resources are exploited more than once (as in chimpanzee's panda nut exploitation [Mercader et al., 2002]) but with a collective benefit (in contrast with the chimp model, which is individualistic). The site qualifies as referential if it can be shown that it was used more than once.
C1. Abundant evidence of butchery	*Abundant* means repeated evidence of primary carcass butchery, preferably not just at the same site but in different sites where an anthropogenic origin is taphonomically justified. The primary access evidence makes this hypothesis dependent on A1.
D1. Transport of (almost) complete size 3–4 carcasses	Transport of complete or partial fleshed sections of animals larger than 100 kg (including access to it and initial butchery to be transported) requires the joint participation of several individuals according to carcass size. A high evenness index of the high-survival set (as defined in Marean and Cleghorn [2003]), together with biometric indicators, in association with long bone siding, would further support this hypothesis if primary access in the exploitation of these resources is demonstrated. This hypothesis is thus dependent on A1.
D2. Collective transport of lithic raw material	Collective transport of lithic raw material is inferred when the amount of lithic artifacts discarded at a site exceed the physical capability of having been accumulated by one single individual, provided that the accumulation took place in one occupational episode and not across a diachronic sequence of various occupational episodes. This hypothesis is dependent on B1 and E.
E1. Tools were needed for every subsistence activity.	Dependence means complete reliance on tools for survival. If so, the exploitation of raw material and use of artifacts across the landscape must be curated. This hypothesis depends on F. Its heuristic value is also mutually dependent on A1, because it would be expected that systematic butchery depends on systematic use of tools.

(*continued*)

TABLE 1.4 *(continued)*

F1. Hominins anticipated adaptive needs.	Refers to anticipation of future needs as reflected in raw material procurement and exploitation and is linked to the reduction sequence concept. This hypothesis is tightly linked to E. Planning also can be justified if selection of specific spots is made with expectations of shared goals in the near (central places) or not necessarily so near future (referential places). This hypothesis also depends on B1 and B2.
G. Systemic nature of these hypotheses	Systemic nature refers to the interconnectedness among the behavioral components described in the previous hypotheses.

research programs were endowed with auxiliary hypotheses (a theory's protective belt) that rendered falsification less straightforward than a Popperian approach, focusing on refutability, would suggest. It is therefore the capacity of interpreting reality by explaining things that discriminates among hypotheses. Lakatos suggested that rather than selecting one program over another because one is refuted, selection is made because one program will always discover novel facts, showing continuous growth. This qualifies as a progressive research program in opposition to a degenerative research program, which is marked by lack of growth owing to the effect of the protective belt of auxiliary hypotheses. The degeneration of a program causes its falsification (Lakato's sophisticated falsifiability).

If we switch "research program" to "theory," then it is clear that irrespective of the degree of refutability of a theory's components (hypotheses) or lack thereof, the degree of corroboration and the amount of knowledge conveyed by alternative theories determines which one is epistemically selected and which one vanishes. It is clear that progressive theories, which contribute with more knowledge, will be preferred to those that are regressive or degenerative. The degree of heuristic value of any given theory is perceived when the differences are large, however. In theories with a smaller degree of heuristic difference, this perception is less obvious.

Scientists should ideally dispose of a "heuristometer," which could provide objective value of the explanatory power of a theory, but that is missing in nonapplied sciences. For historical sciences, in which the quantification of the amount of corroborated knowledge provided is less certain than for applied experimental sciences, a theoretical indicator should be created. Figure 1.2 suggests an example of one of the many plausible indicators

```
n.hypotheses=c(10)#number of factual
hypotheses
nh=1/n.hypotheses
A1=nh*(1+5)#number of links
A2=nh*(1+2)#number of links
B1=nh*(1+4)#number of links
B2=nh*(1+1)#number of links
C1=nh*(1+1)#number of links
D1=nh*(1+1)#number of links
D2=nh*(1+2)#number of links
E1=nh*(1+2)#number of links
F1=nh*(1+3)#number of links
G=nh*(1+0)#number of links
cpk=c(A1+A2+B1+B2)#corroborated
proportional knowledge
ncpk=c(C1+D1+D2+E1+F1+G)#non-
corroborated proportional knowledge
tpfk=cpk+ncpk#total potential factual
knowledge
hi=c([cpk-ncpk]/log[tpfk])#heuristic
index

hi= 0.088
```

A

```
n.hypotheses=c(10)#number of factual
hypotheses
nh=1/n.hypotheses
A1=nh*(1+5)#number of links
A2=nh*(1+2)#number of links
B1=nh*(1+4)#number of links
B2=nh*(1+1)#number of links
C1=nh*(1+1)#number of links
D1=nh*(1+1)#number of links
D2=nh*(1+2)#number of links
E1=nh*(1+2)#number of links
F1=nh*(1+3)#number of links
G=nh*(1+0)#number of links
cpk=c(A1+A2)#corroborated proportional
knowledge
ncpk=c(B1+B2+C1+D1+D2+E1+F1+G)#non-
corroborated proportional knowledge
tpfk=cpk+ncpk#total potential factual
knowledge
hi=c([cpk-ncpk]/log[tpfk])#heuristic
index

hi= -1.14
```

B

```
n.hypotheses=c(6)
nh=1/n.hypotheses
A1=nh*(1+5)#number of links
A2=nh*(1+2)#number of links
D2=nh*(1+2)#number of links
E1=nh*(1+2)#number of links
F1=nh*(1+3)#number of links
G=nh*(1+0)#number of links
cpk=c(A1+A2+D2)#corroborated proportional
knowledge
ncpk=c(E1+F1+G)#non-corroborated
proportional knowledge
tpfk=cpk+ncpk#total potential factual
knowledge
hi=c([cpk-ncpk]/log[tpfk])#heuristic
index

hi= -0.27
```

C

FIGURE 1.2. An example of a suggested heuristic index (H.i; see text for explanation) comparing its utility with the theory proposed in this paper (A), assuming that only a portion of hypotheses have been successfully empirically supported (A1, A2, B1, B2) and two alternative theories, in this case consisting of the same theory but containing a smaller number of successfully tested hypotheses (A1,A2); (B) or a smaller theory containing a smaller number of factual hypotheses, with a similar proportion to A of those empirically corroborated (A1, A2, D2); (C) The value of the heuristic index is expressed in absolute numbers; the higher the result the more explanatory power the theory contains. In this case, A (0.088) contains a higher amount of information explained and empricially corroborated than B (−1.14) or C (−0.27). C, despite being a smaller theory and containing a smaller explanatory potential, has a higher explanatory power than B because in the latter only two hypotheses were empirically corroborated. A negative index shows a negative balance of the potential amount of explanatory power contained in the theory, when comparing the hypotheses that have been corroborated and those remaining to be empirically corroborated. Heuristic index created using R (http://www.r-project.org/).

for differentiating the explanatory power of scientific theories created in anthropological research.

The amount of empirically contrasted knowledge contained in a theory is directly related to the number of factual hypotheses contained in it. Therefore, a theory with a greater number of hypotheses is more difficult to corroborate than another theory with a smaller number of them, although if corroborated even partially, the former could provide more information than a smaller theory, in which all the factual hypotheses have been corroborated. For this reason, any measure of explanatory power must take this into account. Each hypothesis therefore represents a proportion of potential factual knowledge, which varies in each theory according to its size. Furthermore, each hypothesis within a theory does not contain the same amount of empirically tested information, but it varies according to how relevant or peripheral the hypothesis is. The degree of relevance is marked by the number of dependent relations that the hypothesis holds with other hypotheses within the theoretical body (i.e., number of links). To establish a reliable indicator of the amount of knowledge contained in any given theory, the number of hypotheses contained and their types of dependence among one another should be considered.

This would allow one to estimate the relative amount of corroborated proportional knowledge, referred to as the number of hypotheses that have been successfully tested, derived as a relative proportion of values obtained when considering the number of hypotheses involved and their value in the theoretical body. It would also allow one to estimate the potential amount of noncorroborated proportional knowledge, which refers to the number of hypotheses that either have not been successfully tested yet or that have been empirically rejected. Both the corroborated set of hypotheses and the noncorroborated set of hypotheses constitute the total potential factual knowledge of a theory. Using these parameters, as shown in Figure 1.2, can lead to a primitive indicator of the explanatory power of a theory, or heuristic index.

To apply this index accurately, one should consider whether both theories tested share the same question (in this case: what is the origin of human behavior?) even if the main axiom (e.g., cooperative behavior leading to solidarity or, alternatively, any other proposition) differs in each theory.

Discussion and conclusion

I do not intend here to provide arguments why a processual approach to anthropology is more accurate or advantageous than other theoretical approaches in terms of acquisition of knowledge. Rather, the main goal of this text is to create awareness among scientific anthropologists that their procedures might or might not qualify as scientific, depending on the philosophical school of thought that they conscious or unconsciously embrace. Today the dominant epistemological school for practitioners of natural sciences is scientific realism.

Almost all postmodern approaches to anthropology see the scientific effort of the processualist approach with skepticism. Beyond their constructivist vision of science, their view is fueled by the unfinished debates created within the processual approach itself. A purported scientific hypothesis frequently leads to interpretations that are widely divergent from another "scientific" hypothesis also aiming at testing the same question. My humble view is that part of the explanation for this is the incompleteness with which several processualists have understood the relationships between theory-hypothesis and axiom-theorem (Bunge, 1998a). Processualists began constructing their conceptual and theoretical building correctly but stopped once the foundations of the building (how to create factual hypotheses and empirically test them one by one) were finished. They also know how to build the roof (the theory containing the main axiom), but they are missing the bulk of the building (structural frame and walls) because they mostly have not articulated hypotheses successfully and hierarchically (from factual to founder hypotheses or vice versa), linking them conceptually to the theory in an interrelated way (most hypotheses of a theory are interdependent or systemic; Bunge, 1998a, 2006). This criticism is valid only if it is assumed that to gain parity with natural sciences, scientific anthropology is better defended from a scientific realistic epistemic approach. The denial of this assumption, as well as the refusal to follow scientific methods, would lead to the rejection of the bulk of the ideas expressed in this work.

Scientific realists conceive theories as permanently morphing through the testing of their hypothesis (Figure 1.3) because of an increasing body of knowledge. In the words of Bunge (1998a, p. 436)

> Since the data-gathering-and-packaging view of science ignores the aims of theorizing, it will be convenient to state such aims explicitly. The basic desiderata of scientific theory construction are the following. (i) To *systematize knowledge* by establishing logical relations among previously disconnected items; in particular, to explain empirical generalizations by deriving them from higher-level hypotheses. (ii) To *explain facts* by means of systems of hypotheses entailing the propositions that express the facts concerned. (iii) To *increase knowledge* by deriving new propositions (e.g., predictions) from the premises in conjunction with relevant information. (iv) To *enhance the testability* of the hypotheses, by subjecting each of them to the control of the other hypotheses in the system.

The latter part of this statement is crucial in the way factual hypotheses are tested and has been regularly neglected in "scientific" approaches to anthropology. This explains in part the frequent low resolution of processual projects when displaying contradictory interpretations, despite employing the empirical-hypothesis testing approach.

Factual theories are iconic reconstructions of real systems and are inferred models of reality. Modeling is an essential part of theory building, and most factual hypotheses in anthropological theory are analogical; however, this must not be mistaken with the use of analogies as proxies for theories. In the field of early human evolution, the reconstructions of hominin behavior from the Plio-Pleistocene record have been based on pure analogical reasoning. Isaac (1978) interpreted sites by using an analogically derived list of characteristics in which human and nonhuman primates differ. Binford's (1981) subsequent criticism, although better framed from a theoretical point of view, was also based on a structured and limited view of analogy (through the middle-range theory), by comparing a varied repertoire of analogies to published data from early sites: do sites look like human foragers' base camps or carnivore-formed assemblages? Binford contributed positively when looking at alternative possibilities to the anthropogenic models widely held, which could fit the empirical record better. His "marginal scavenger" model was produced as an ad-hoc interpretation to statistical treatment of data, which did not fit quite well with either strict human or carnivore models of bone accumulation. Paradoxically to his hypothetic-deductive philosophy, it also did not stem from a theory conceived prior to empirical testing but rather was inductively built a posteriori without the support of a scientifically framed hypothesis specifying what a bone accumulation modified by a marginal scavenger hominin must look like. Sept's [1992] chimpanzee-nesting model was another example of analogical interpretation not epistemologically justified: just because chimps redundantly occupy the same trees for nesting, hominins could have done something similar to generate the characteristics observed in the early archaeological record. Rose and Marshall's (1996) "resource-defense" model also failed to be theoretically framed in a similarly scientific way, with the elaboration of testable hypotheses, and was mainly based on ethological analogy. Blumenschine's (1986) passive scavenger model was also constructed by mere ethological/ecological analogy of observed availability of carcass resources in one ecosystem assuming the same processes could have existed in the past (without contemplating variability caused by different ecological conditions [see Tappen, 1992, 1995]). He initially did not produce a scientific hypothesis with clearly defined propositions and

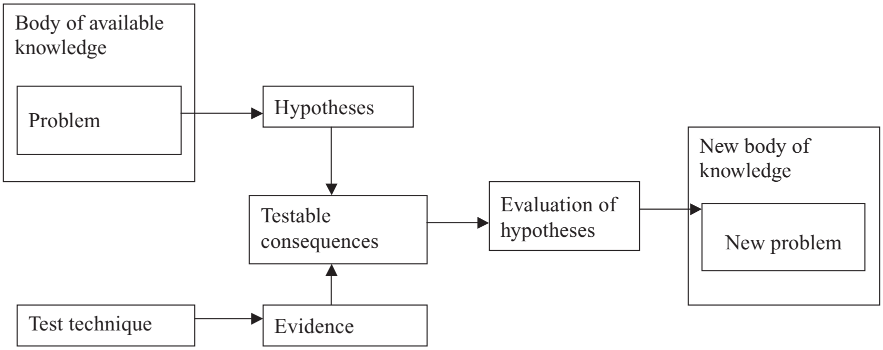

FIGURE 1.3. Scheme of the progressive increasing knowledge concept, inherent to scientific realism as shown by Bunge (1998a).

falsifying premises. When he did (Blumenschine, 1995), these were subsequently refuted (review in Domínguez-Rodrigo et al., 2007a). O'Connell's (1997) *near-kill location* model also failed to be phrased in the form of a testable theory and was based on descriptive ethnographic analogy: if modern foragers create bone accumulation near alluvial environments produced by their preparation of carcass remains to be transported elsewhere, why not assume that early sites had a similar function?

Ever since the revisionist debate started in the 1980s, the different behavioral models created to explain the early archaeological record have been either strictly analogical (most models), purely theoretical without properly phrased testable hypotheses (e.g., Pott's [1988] stone-cache model[4]), or completely lacking hypotheses and a scientific theoretical body (e.g., the *preferred site* model [Schick, 1987] or the *male display* model [O'Connell et al., 2002]). Less frequently, when some authors provided a better-described testable hypothesis (passive scavenging in barren floodplain and opportunistic refuge [Blumenschine and Masao, 1991; Blumenschine et al., 1994]), this could be tested and subsequently refuted (Ashley et al., 2010), enabling some advance in our understanding of early human behavior. The choice of models available in recent and not so recent literature is so long that modern archaeologists are confused as to (1) which one fits the empirical record better and, (2) whether uncovering the behavioral meaning of early sites is theoretically feasible. Thus, is it not strange that recent so-called behavioral models have given up tackling the functionality of sites and focus on the hominin subsistence strategy that could be most directly

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[4] Actually, critics of the model were responsible for empirically testing it and providing falsifying scenarios (de la Torre and Mora, 2005; Domínguez-Rodrigo et al., 2007a).

read from the materials contained in sites (e.g., Ferraro's [2007] *obligate carnivory* model).

It could be argued that such a variety of interpretations (most of them stemming from processual approaches to the archaeological record) reflects (1) intensive and abusive use of analogy expressed either through direct application of the analogue to the archaeological record, without a properly defined theory or (less ambitiously) a hypothesis; (2) individual hypothesis testing without framing it in a systemic theoretical body; and (3) lack of links among hypotheses, resulting in insufficient testing for resolving equifinality scenarios and opposite interpretations. Processualists might believe that the empirical testing of individual hypotheses is scientific enough, but by doing this the hypotheses thus tested are devoid of a substantial part of their heuristic power: the one generated by its association with other hypotheses.

I would argue that part of the reason why processualists specialized in testing single hypotheses derives from the application of Binford's "middle-range theory" approach. Middle-range theory in archaeology was borrowed from its counterpart in sociology, as pointed out earlier. Merton (1967) had argued that to adapt theory to its empirical consequences, scholars had to give up the attempts of finding a general theory that would explain holistically all aspects of social life. A "grand theory" would be hard to test empirically, especially in a field that lacked control of most of the variables, such as sociology. Merton argued that sociologists had to concentrate on fragments of social reality until the body of knowledge acquired would enable sociology to converge with natural sciences and provide explanations in the form of laws or patterns of human social behavior. Theories, in their broadest sense, had to be avoided (but only initially). Binford (1981) borrowed the approach literally by linking middle-range theory to determined aspects of human behavior or natural processes. Middle-range theory in archaeology provides answers to specific functional questions; that is, how a determined process results in certain material diagnostic characteristics (e.g., how certain butchery practices leave diagnostic features on bones). By deliberately emphasizing the role of middle-range theory, Binford avoided the testing of larger theories. Middle-range research was conceived as a prerequisite to the enquiring of questions that implied a combination of more than one process. It would not be an exaggeration, however, to say that the role that middle-range theory played in processual archaeology was the same as that of scientific factual hypotheses in applied sciences. It limits testing to specific hypotheses. Processualists have not realized that a systemic use of middle-range theory would enable them to test grand theories only if processes could be intertwined. Linked hypotheses provide higher heuristic power, which would make their testing through middle-range approaches more efficient and discriminatory. Instead of that, Binford's limited conception of the use of middle-range theory does not go as far as Merton's, which implies that when the knowledge provided by middle-range theories builds, then grand theories can be approached. This handicaps Binford's goal of making archaeology a fully scientific endeavor, because isolated hypotheses cannot be efficiently patched to construct reality (Bunge, 1998a).

In Plio-Pleistocene archaeology, this translates into one basic consequence: the issues of the emergence of stone tool use or meat eating are not independent from the fact that both behaviors occur in selected spots where materials accumulate in high densities, which archaeologists call *sites*. Something as complex as the meaning of early sites cannot be grasped through middle-range theory alone but through a wider range theory. Stone tool use or meat eating, thus, cannot be explained independently from the behavioral meaning of those sites where they occur. Archaeologists have been blinded by the immediacy of the middle-range theory. It applies to factual hypotheses alone, but its power would be broader if applied systemically. The time is ripe for processualists to think bigger; grand theories (as in natural sciences) should guide the way of scientific research in anthropology, knowing that the building of scientific knowledge implies starting empirical testing by the foundations (factual low-level hypotheses) and going upward toward the roof (founder hypotheses and main axiom of the theory).

The goal of this work is to present the structure of a scientific theory on the emergence of human behavior without putting it to test. That would take another significantly longer work. I can predict, however, that the status quo of similar heuristic value or ambivalence affecting most of the available models to explain early archaeological sites will vanish when this, or other similarly framed theories, are applied to the fossil record. I also predict that most of these models will be definitively discarded.

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PART I

On the Use of Analogy I: The Earliest Meat Eaters

CHAPTER 2

Conceptual premises in experimental design and their bearing on the use of analogy: A critical example from experiments on cut marks

Manuel Domínguez-Rodrigo

Experimental archaeology and actualistic research are integral parts of *middle-range theory* and thus of modern scientific archaeology, which is based on the testing of alternative hypotheses (Binford, 1981; Gifford, 1981). Hypotheses are framed within specific referential analogs created by careful observation. These referential frameworks are elaborated by controlled documentation of processes, in which behaviors of independent agents are understood within their specific contexts and the resulting actions of these agents are diagnosed (Binford, 1981; Gifford, 1981; Gifford-Gonzalez, 1991; Gould, 1965, 1979; Wylie, 1982, 1988, 1989).

If there is a hierarchy of principles that can be applied to the components of actualistic research, it can be argued that the most important one is the adequate use of premises (see Wylie, 1988) in the elaboration of referential frameworks. Researchers create these analogs primarily to understand behaviors represented in and responsible for the archaeological record. The significance of analogy as a nonobjective entity was initially stressed by Richter (1928). It entails a series of assumptions, some of them selected by the researcher, in a dialectic dynamic between the ideas that researchers try to test and the way the testing is eventually carried out.

A systemic evolutionary taphonomic approach (innovated by Fernández-López, 2006), considering taphonomic entities as endowed with properties subjected to change according to their structure, behavior, and environment, also shows that the selection of criteria to be replicated in experiments is ultimately dependent on what has been called taphonomic redundancy.[1]

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[1] Taphonomic redundancy has been defined as the capacity of taphonomic elements to repeat the same message. Taphonomic redundancy, as well as replication, allows the

A widely accepted articulation of theoretical principles guiding actualism was outlined by Gifford-Gonzalez (1991). She differentiated between "formal" analogy and "relational analogy." The former is obtained through observation and the latter through inference. She conceived that there was a continuum from one form of analogy to the other within a hierarchical conception of taphonomic processes defined by six nested analytical categories (i.e., trace, causal agent, effector, actor, and behavioral and ecological context). Formal analogies can be used in the first four categories, because actors can be observed, provided equifinality can be overcome (Lyman, 2004). In contrast, the behaviors and the ecological factors that determine them can never be directly reconstructed from the analysis of bones and have to be indirectly inferred. In this case, relational analogy applies. Gifford-Gonzalez (1991) argued that the six analytical categories were interdependent. Starting from the broadest categories, Gifford-Gonzalez argued that every single taphonomic process is primarily understood in specific ecological contexts. If ecology conditions behavior, then actors should react in a predictable way according to those conditions; in turn, any such actions should be reflected in the traces imprinted on bones. Any experiment that obviates the relationship of these nested categories would be conceptually flawed.

Every analog is in essence incomplete, because it only reproduces a selected and limited set of variables and can only control for a determined number of these. Similar processes in the past in which other nonexperimentally considered variables might have intervened make the application of analogs systematically imperfect. Given that researchers must be aware of the imperfect nature of analogy, the relevance of the correct use of premises and assumptions in experiment design cannot be overemphasized.

Some analogies in taphonomy can be defined as substantive because they reproduce general processes that are not subjected to a significant degree of variability. For instance, the patterns of bone breakage (i.e., notches, planes) resulting from experimenting with physical processes such as dynamic (hammerstone) or static (carnivore dentition) loading are more generally applicable as analogs than other processes subjected to greater contextual variability. In many studies involving controlled experimentation of physical processes limited to the actor-trace sequence, analogies can be justifiably used within generalized referential frameworks. In contrast, and more specifically in archaeology, analogies depending on ecological-behavioral factors are subjected to a higher degree of variability and can be used

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estimation of the "repeatedness" of taphonomic groups under particular environmental conditions, on the basis of their actual properties (Fernández-López, 2006).

confidently only as referential frameworks of determined taphonomic problems; they are case specific and could be labeled methodological analogies. In this type of analogy, given the large array of variables at play, researchers must be aware of the list of assumptions that they are making, on how these assumptions translate into hypothesis premises (Wylie, 1988), and eventually, how these premises and the hypotheses that contain them are subjected to testing. Failure to do so will produce false equifinality scenarios, ambiguity in interpretation, and eventually fuel postprocessual criticism of the subjective nature of the scientific method.

The present work uses experimental studies of cut marks as an example of the variability of criteria used by researchers when conducting experiments and designing referential frameworks. It is argued that this variability is not always scientifically acceptable, either because some approaches to experiment design are conceptually flawed (incoherent use of assumptions and premises of what is supposed to be replicated) or in other cases, because comparisons across experimental data sets cannot be sustained when the premises of the tested hypotheses by different researchers are not the same.

Analogy, uniformitarianism, and the concept of regularity

The only way to "reconstruct" the past is to assume that there are certain regularities in the way in which the world works that are not subjected to time and are therefore observable in the present. Thus, these regularities can also be inferred for the past. The assumptions of uniform rates and the implication of slow and gradual change in *substantive uniformitarianism*, using Gould's (1965) term, have proved incorrect in many cases. The modern conception of uniformitarianism does not assume the constant rate of change and acknowledges that the agents of change cannot be verified empirically. The laws that govern these agents remain permanent, however. This new uniformitarianism is methodological and vital to scientific procedure. Spatial and temporal invariability in the laws that control processes is absolutely critical if any general conclusion about the past is to be made from observations in the present (Gould, 1965). Methodological uniformitarianism does not directly inform on nature but provides an approach with which to understand it (Shea, 1982). This approach, in assuming that natural laws are invariant in time and space, does not invoke unknown hypothetical processes if the observed results can be explained through modern processes (Gould, 1965).[2]

[2] Gould was rewriting Occam's Razor: One should not increase, beyond what is necessary, the number of entities required to explain something. Even fourteenth-century scholars can remind experimental archaeologists of the need to keep it simple.

Simpson (1970) further elaborated on this concept by separating those aspects of the world that remain unmodified in time and space (*immanent properties*) and those that are contingent on particular interactions in each moment and place (*configurational properties*). Immanent properties allow historical processes (or parts of them) to be interpreted precisely because they are not subject to variation in space and time; that is, they are universal. Configurational properties must be approached in a different way. Unlike universal immanent properties, configurational properties are based on *regularities* in the variables that regulate them. There are two types of configurational processes, however: (1) those that are highly variable and therefore difficult to predict; and (2) those that are highly regular and therefore predictable. Obviously, only the latter can be reliably applied to past dynamics.

I agree with Gould (1980) in that only those processes that the properties and range of variation of which can be measured should be used in scientific archaeology. Some researchers think that only geological/physical processes can be understood from such an approach (e.g., Nairn, 1965). Simpson (1970) stresses that for any process to be understood it needs only to be uniform, however; that is, it must exhibit regular properties. This is possible whether reconstructing the mechanic aspects of the world or biotic behaviors. Much misunderstanding emanates from the misconception that only universal, or immanent, laws apply to the past. Because even universal laws are never absolute (Popper, 1956, 1972), however, we are left with heuristic explanations (Lakatos, 1978) that are grounded in the predictability of their regularities.

Regularity is derived from probability. Simpson (1970) notes two difficulties with inferring historical processes, however: (1) multiple processes can have similar results (i.e., equifinality); and (2) configuration makes processes unpredictable. Simpson (1970) himself suggests a solution for the latter. Scientific prediction depends on what is periodical and repetitive. Although historical events are unique and therefore unpredictable on at least some level, there are different degrees of distinctiveness, and historical events can be considered predictable to the extent to which we understand their causes and the regularity of their behavior. This is how Simpson (1970) defines historical configurations: based on probabilities and with similar heuristic power (*sensu* Lakatos, 1978) as natural laws. An historical event is determined by the immanent characteristics of the universe that act on it, but in a configurational way.

This leads us to the use of analogy. To understand past configurational processes, the context and variables that generate modern processes must also be understood. This understanding differentiates descriptive from formal analogies and from relational (dialectical) analogies (Gifford-Gonzalez, 1991). Analogies play an important role because they can discern and document variability in observed regularities. Most important, analogies can be observed and replicated. To use an analogy as a referential framework properly (*sensu* Binford, 1981) for interpreting taphonomic processes in the past, researchers must clearly be able to (1) differentiate whether the analogy is case specific or general; (2) in the former case, produce a list of assumptions using collected data from the assemblage where hypothesis testing will take place; (3) justify that the experimental premises match the set of assumptions made for the formulation of a hypothesis. This can be better explained with an example.

A practical example documenting conceptual variability in hypothesis testing: Experimental replication and interpretation of cut marks

The use of replication in experimental archaeology during the 1980s enabled a certain optimism that cut marks could be scientifically used to infer human butchery behaviors (Binford, 1978, 1981; Bunn, 1981, 1982; Bunn and Kroll, 1986; Gifford, 1977; Lyman, 1987; Gifford-Gonzalez, 1989). Nowhere has this been more illustrative than in the hunting-scavenging debate of Plio-Pleistocene sites in East Africa (see review of this debate in Domínguez-Rodrigo, 2002). The possibility that cut marks could be equally linked with hunting and scavenging behaviors prompted the development of new experimental protocols of opposite-hypothesis testing to distinguish both behaviors (Domínguez-Rodrigo, 1997a, 1997b). Nilssen (2000) also contributed with new experimental protocols to differentiate diverse butchery behaviors. In the past ten years, however, the diverse array of experimentation on carcass butchery has yielded a varied interpretive repertoire comprising the following claims (Table 2.1):

1. Cut marks are of limited value to interpret butchery behaviors and the order of access to carcasses by hominids because they are subjected to equifinality, given that they could be the result of the removal of the scraps of flesh surviving carnivore consumption of their prey (Capaldo, 1995, 1997, 1998).
2. Cut marks, when applied to early Plio-Pleistocene sites, support the hypothesis that they were the result of hominids butchering carnivore kills (Selvaggio, 1994).

TABLE 2.1. *Interpretation of the methodological utility of cut marks and of the data on these bone surface modifications from east African Plio-Pleistocene sites by the main researchers whose research is discussed in the present work*

Researchers	**Methodological interpretation of cut marks**	**Archaeological interpretation of Plio-Pleistocene hominid carcass acquisition and butchery**
Selvaggio (1994)	Subjected to equifinality	Hominids were passive scavengers from carnivore kills.
Capaldo (1995, 1997, 1998)	Subjected to equifinality	Hominids were passive scavengers from felid kills and mass drownings.
Domínguez-Rodrigo (1997a, 1997b)	Cut-mark frequencies and anatomical distribution (per element and bone section) can be used to differentiate between butchery of fleshed carcasses (primary access) and removal of scraps of flesh from some carnivore kills (secondary access).	Hominids were having primary access (probably including hunting) as the dominant strategy of carcass acquisition.
Lupo and O'Connell (2002)	Not valid to discriminate primary versus secondary access to carcasses because they are statistically indistinguishable in both experimental scenarios	Hominids were using mixed strategies of hunting and confrontational scavenging (in various stages of carcass completeness).
Pobiner (2007)	Subjected to equifinality	Hominids were passive scavengers from felid kills.

3. Cut-mark patterns found in Plio-Pleistocene sites, when compared to those documented in modern foragers (e.g., Hadza, in Tanzania), support a mixed strategy of early, intermediate, and late access to variously fleshed carcasses (Lupo and O'Connell, 2002).
4. Actualistic referential frameworks are useful to interpret cut marks as resulting from primary access to fleshed carcasses by humans versus defleshed carcasses abandoned by carnivores, and support the hypothesis of primary access to fleshed carcasses by hominids

(Domínguez-Rodrigo, 1997a, 1997b, 2002; Domínguez-Rodrigo and Pickering, 2003).

5. Comparisons of different experimental sets of cut marks, which test primary or secondary access to carcasses, show that they do not provide resolution, because they are statistically indistinguishable (Lupo and O'Connell, 2002; see critique in Domínguez-Rodrigo, 2003).
6. Recent experiments have widened the degree of variability of flesh abandoned in carnivore kills, which suggests that cut-mark patterns previously derived from experiments that recorded flesh distribution in a more restrictive sample of kills are no longer valid, providing more evidence of the behavioral ambiguity of cut marks (Pobiner, 2007).
7. The tremendous range of variation in frequencies and anatomical distributions of cut marks across multiple assemblages prompts skepticism that the behavioral meaning of cut marks could be effectively inferred from prehistoric assemblages (Lyman, 2005).

The obvious message is this: the ambiguity of cut marks hampers their resolution to understand butchery behaviors and therefore the order of access by hominids to carcasses. Most of the experiments and interpretation of cut marks in the previous points (six of seven) have been carried out and applied to a restricted number of Plio-Pleistocene sites in East Africa to understand the butchery behavior that these sites have preserved for our understanding of a crucial stage of human evolution. More specifically, most of those have been applied to one site: FLK Zinj. This clearly shows that, in principle, the focus of these experiments was case specific. In blatant contradiction to this, however, most statements on the meaning of cut marks (this author's included) were thought to be of universal applicability. This is wrong: cut-mark frequency and anatomical distribution result from processes that belong to the "ecological" and "behavioral" spheres of Gifford-Gonzalez's (1991) nested set of inferences and are therefore subject to variability. This prevents any experiment carried out to test the meaning of cut marks in the kind of "inferred" ecological and behavioral contexts to be applied anywhere else where both variables might have been different. As Lyman (2005, p. 1722) recently admitted: "well-founded interpretations of frequencies of cut-marked remains may require unique kinds of contextual data." FLK Zinj was formed in an alluvial "near-lake" habitat within an ecosystem where felids and hyenas seem to have been fairly abundant (Domínguez-Rodrigo et al., 2007). Given that resource availability for scavenging hominids is ecologically dependent (Blumenschine, 1986;

Tappen, 1992), when modeling opposing hypotheses of access to carcasses, researchers have to elaborate their experimental premises trying to ensure maximal consistency between experiments and the inferred context.

I will use the example of the behavioral meaning of cut marks at FLK Zinj to illustrate how a specific set of assumptions, premises, and hypotheses can be designed and successfully tested. I will also use a comparison with currently available experimentation to explain why some researchers might be closer than others to accurately testing the hypotheses of primary or secondary access to carcasses by hominids, and indirectly to providing high-resolution (rather than ambiguous) referential frameworks. This comparative exercise can be graphically followed in Figure 2.1. The null hypothesis is that cut marks lack resolution to infer primary or secondary access to carcasses. A subsequent null hypothesis is that hominids were scavengers (secondary access hypothesis). Proving that both versions of the null hypothesis are wrong would imply that early hominids had primary access to carcasses and that this can be inferred by specific placement and frequencies of cut marks.

Assumption 1. The essence of any experimental study is control. The only way to effectively link actor-effector-causal agent-trace is by having as much control as possible of the complete experimental/observational process. In the case of the hypotheses under testing, one factor in which control is key is resource availability from carnivore kills as potential scavengeable resources for hominids. This is especially relevant in the case of flesh scraps. The assumption is that no data derived from uncontrolled experiments should be heuristically used in this regard, because we could be inferring the wrong actor, producing an equivocal diagnosis. The resulting premise is that the experiment has to be carried out with as much control as possible or otherwise discarded.

For the secondary access hypothesis, the experiments that are inadequate according to this premise are those made by Pobiner (2007) in the wild, who never witnessed a complete process of carcass consumption in her lion sample, because she documented hunts in the late evening–early night and evaluated carcass modification and resource availability the next morning. Her study lacks control and is based on inferences that cannot be empirically supported. The reported tooth mark damage from the carcasses that she collected in the wild also could be the result of other carnivores having access to carcass remains during the night. This could explain why the only controlled sample that she collected in captivity shows a more intense consumption of flesh than that reported in her wild "lion" sample, and in apparent contradiction, almost one half the tooth mark frequency.

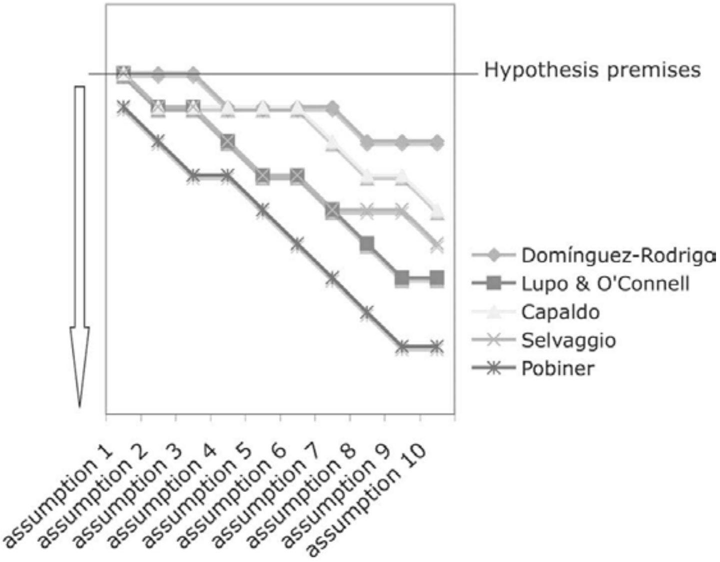

FIGURE 2.1. Experimental matrix showing the conceptual assumptions and premises of hypothesis testing as described in the text (*top horizontal line*) and the deviations from these by various researchers. Each experimental premise for each researcher that does not take any given outlined assumption into consideration is reflected in a step down from the horizontal line of the matrix. The lower the experimental research appears (*direction of arrow*) compared with the top horizontal line, the more conceptually distant the experiment is from the hypothesis premises, and the more inappropriate it is for comparison with the specific case of the behavioral meaning of cut marks at FLK Zinj.

Pobiner (2008) does not agree with this evaluation of her work and justifies her study by saying that the control in her sample is enough to support her interpretations. She collected data from observations that lacked causal knowledge of actor-trace, however; that is, the whole process of hunting and carcass consumption by lions was not observed and completely documented. Control would have been the *only* guarantee that the data collected could be attributed exclusively to any specific agent. Evaluating carcasses "as soon as possible" after carcass abandonment by carnivores, "or to the earliest possible time the next day," cannot be used as an epistemologically valid argument, because the interval between carcass abandonment and data collection could span several hours, and therefore the possibility of intrusion of other nonobserved (nondocumented) agents is fairly plausible. Any research program based on the belief that "actualistic researchers presumably aim to exert as much control over their sample as possible, but we are only as successful as circumstances allow" (Pobiner, 2008: 469) should be interpreted with caution, because that limited control can also be taken as the boundary that separates scientific hypothesis testing from speculation (Barnes et al., 2005; Niiniluoto, 1987, 2002; Bunge, 2006).

Pobiner (2008) fails to show convincingly that lions were the only carnivores intervening in the long nighttime hours that the carcasses were exposed. If that were the case, it would certainly show the highly disturbed ecological nature of the setting where she undertook her research, because absence of carnivores at kills during the night would be odd in any African park or reserve lacking human impact. None of the arguments that she uses to infer exclusive lion authorship in carcass modification can be sustained without some degree of faith, something that no scientific testable hypothesis can allow (Popper, 1956, 1972; Lakatos, 1978; Niiniluoto, 1987, 2002).

Pobiner admits that the bone damage that she obtained working with lions in captivity is different from that of wild lions and is probably due to boredom chewing. The question therefore is: where is the epistemological bridge that allows the use of such experiments as useful analogs to be applied to the past? It is widely known that boredom chewing by felids is documented only in captivity. Not even in their dens (e.g., leopard lairs) do felids show this behavior (Domínguez-Rodrigo and Pickering, 2010).

Assumption 2. Following a basic Popperian principle, hypotheses can be tested only when confronted with their opposite. Our whole understanding of the use of statistics in science is based on this principle: the null hypothesis. Experiments used to test a hypothesis must be able to test the opposite and reject one of them. Inferences drawn from unilateral testing are not scientifically reliable. The assumption made is that an opposite-testing hypothesis is only well founded when the same set of assumptions, premises, and analytical variables have been used. This happens most frequently when it is the same researcher who carries out the testing of both hypotheses. The scientific premise is that only in equally comparable analytical sets can opposite hypotheses be tested and compared.

In the comparative set of experiments, most researchers have unilaterally tested a hypothesis, relying on the results obtained by a different researcher for the opposite hypothesis. Because the set of variables used by every researcher is unique (see description in Domínguez-Rodrigo, 2003), however, the comparisons are not necessarily valid.

Assumption 3. The constraints of elementary taphonomic alteration (as defined by Fernández-López, 2006) are primarily determined by the ecological context where it takes place. FLK Zinj was formed in a near-lacustrine habitat where a large array of carnivores was present. Actualistic studies have shown that the interplay between felids and hyenids is the most determinant to understand modern bone modification and deposition and resource availability for scavengers in modern African savannas (Blumenschine, 1986; Domínguez-Rodrigo, 1996; Tappen, 1992). Competition conditions the way that each carnivore consumes prey remains and also conditions the way that terrestrial felids feed. When under pressure from either hyenas or human, felids tend to consume their carcasses hastily, leaving more scraps of flesh (Domínguez-Rodrigo, 1999). At Olduvai, both during Bed I and Bed II times, hyenas seem to have been using the alluvial habitats with even higher frequency than they do today in similar modern settings (Domínguez-Rodrigo et al., 2007; Monahan, 1996). This can be inferred from the intensity of hyenid-modified assemblages in these settings with no modern equivalence. This has specific relevance regarding the amount of scavengeable resources by hominids. The assumption and subsequent premise made from this inference is that experimental replication of cut marks has to be carried out (especially those on carcasses obtained from carnivore kills) in similarly competitive settings to guarantee comparability.

When applied to the compared experimental set (Figure 1.1), all researchers but one comply with this premise. Capaldo (1995, 1998) and Selvaggio (1994) made their experiments in the Serengeti. Domínguez-Rodrigo (1997a) carried out his experiments in Maasai Mara, Tsavo, Galana, and Kulalu. Lupo and O'Connell (2002) made their observations in Eyasi – with a much lower presence of carnivores than the national parks where the previous authors carried out their research, but similarly diverse in carnivore taxa. In contrast, Pobiner (2007) conducted her research in a Kenyan private ranch, where some carnivores were systematically chased. Most of the hyenas were either poisoned or shot at, and given their abundance, lions were also shot sometimes, prompting them to be mostly nocturnal (L. Frank, personal communication, 2006). In this human-altered ecosystem, lion behavior was conditioned by two variables: marginal interspecific competition owing to the removal of hyenas and the human impact on the demographics of lions. As a result, the amount and anatomical distribution of flesh that Pobiner documented in carcasses abandoned by lions differ (in some cases drastically) from the more consistent descriptions reported by Selvaggio (1994) and Blumenschine (1986) for the Serengeti and Ngorongoro ecosystems and Domínguez-Rodrigo (1997a, 1997b, 1997c, 1999) for the Maasai Mara, Tsavo, Galana, and Kulalu ecosystems, which were more similar when compared to each other. Thus, it can be seen that diverse environments in different ecosystems in national parks with minimal anthropogenic impact in trophic dynamics yield very different results from those documented in a highly altered environment, such as the one used by Pobiner (2007) or in specific situations of human–lion interactions as documented by Domínguez-Rodrigo et al. (2007). (See later discussion.) This supports the claim that only experiments conducted in environments unmodified by humans reliably document variability in carnivore behavior. Indeed, although Tsavo, Galana, and Kulalu offered different ecological conditions from Maasai Mara, Serengeti, and Ngorongoro, the documented manner of lion consumption of wild game was very similar in the resulting flesh availability. This is being currently supported by similar studies in progress in Tarangire National Park (Tanzania).

Pobiner's (2007) study obviates the ecological impact of the altered environment where she conducted her study, and she claims that the results obtained are heuristically useful for discriminating the real utility of cut marks inferred from the amount and variation in the anatomical distribution of flesh abandoned by felids. From the experimental frameworks currently available to understand flesh abandoned by felids in the Zinj environment, Pobiner's is the least appropriate, given the drastic ecological differences documented between both types of environments.[3]

Pobiner (2008) disagrees with this evaluation of the context where her research was carried out and tries to justify its suitability for actualistic research. The ranch where Pobiner conducted her study is surrounded by other ranches, however, and the hyenas had been systematically killed for years before her arrival at the site. The hyena population was (and still is) extremely low. This is reflected in the fact that if Pobiner were right about carcasses being exposed all night without other carnivore intervention after abandonment by lions, it would certainly suggest that hyenas were not a meaningful ecological factor shaping competition and therefore carnivore (lion) behavior. Such circumstances would also be incompatible with the statement that jackals are fairly common in the reserve. If so, why would they skip the chance of a generous meal at abandoned lion kills during the night? That is not what is documented in protected national parks. According to Pobiner, ranchers in Laikipia "enthusiastically tolerate a healthy population of large carnivores" *other than hyenas*, but they also sometimes do not tolerate lions. (See later discussion; emphasis added.) The ranch includes forty-three lions and two to five leopards; no cheetahs are reported. Lions are therefore virtually free from competition. Many zoos have a larger representation of large African carnivores than that.

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[3] This refers to human-modified ecosystems (like that used by Pobiner for her experiments) and savannas not impacted by humans in their trophic dynamics (like those used by the other researchers referenced in the previous paragraph).

In contrast, national parks have much higher counts, expressed in thousands of individual animals: for example, the Serengeti carnivore census reports 2,800 lions, 9,000 spotted hyenas, 800 to 1,000 leopards, 6,300 jackals, 200 to 250 cheetahs, and 50 wild dogs (Caro and Durant, 1995). The differences in the ecology of these ecosystems are obvious.

Pobiner (2008) argues that no lion shooting ever took place, that lions were not mostly nocturnal, and that no systematic hunting on the area has been documented. Lawrence Frank, a highly respected authority on local carnivores, argues otherwise:

> [This is] a ranch where they have always shot hyenas, and there are very few, if any. There are places in Laikipia where there are a reasonable number, but probably nowhere to compare to the Mara or parts of Serengeti. Further, this is bush country, so visibility is poor, plus the carnivores are wary of people – not good for observational studies of the kind you describe. Lions are shot regularly for eating cattle, so they are totally nocturnal, and not easy to find or watch. Hyenas are equally nocturnal. (Lawrence Frank, written communication, October 4, 2006)

Of note here is not only the systematic shooting of the hyena population but also that shooting of lions took place, which makes carnivores (including lions) wary of people. In the lion research carried out in Galana and Kulalu (described in Domínguez-Rodrigo et al. [2007]) Domínguez-Rodrigo witnessed two patterns of carcass consumption by lions: one on wild game (where humans, given their small number, left lions undisturbed), resulting in utterly defleshed carcasses; and one on cattle, where lions fed very fast during part of the night and subsequently fled the spot because of fear of humans, abandoning the carcass when it was still partially or even very fleshed. The fact that population density in the Laikipia area is relatively high and that as a result carnivores are wary of humans could cause lions to abandon carcasses earlier than they normally would, which has an impact of the availability of resources for other carnivores. The question again is this: how do we relate this analog, produced under specific circumstances that did not exist in Plio-Pleistocene savannas (caused by modern humans and their twenty-first-century technology), to the past to reconstruct prehistoric butchery?

Pobiner (2008) trivializes the importance of hyenas in the feeding behavior of lions. She says that she does not know of any references showing that such interaction is reflected in the amount of flesh available after abandonment of carcass by lions. Blumenschine (1986) argued that he mostly focused his actualistic research in the Serengeti because the high lion–hyena competition in Ngorongoro Crater did not allow him to find enough carcasses prior to hyena consumption. Schaller (1972) shows that prey availability conditions the way that lions process their carcasses. He documents how lions can give up carcass remains when hyenas are very bold. He also reports that lions frequently fail to keep their kills during the night. Because of hyenas, 17% of kills of a sample monitored for 23 nights were abandoned after lions had eaten a portion of the carcass, and 39% were eaten thoroughly by lions in the absence of hyenas. In these interactions, Schaller (1972: 273) reports that in the presence of hyenas, lions "begin to feed rapidly as if anticipating the loss of their kill." In the same ecosystem where Blumenschine reported more than 200 defleshed carcasses, Schaller (1972) shows that during the concentration of the migratory wildebeest in the rainy season, lions may engage in "mass killing," eating prey only partially (this constitutes 4% of their kills). Hyenas also engage in this type of "surplus killing" (Kruuk, 1972; Wambuguh, 2007). Therefore, the availability of prey resources (in nonmigratory ecosystems determined by carnivore competition) conditions the amount of flesh available on abandonment of carcasses. This has been reported by Domínguez-Rodrigo (1999), who also showed that the amount of flesh scraps found in lion kills varied according to habitat, because of carnivore interaction and different degrees of competition.

Pobiner (2008: 472) argues that "since the lions at SGR were not under pressure from hyenas or humans, they should presumably leave fewer flesh scraps." If one is considering the analogs from undisturbed reserves (whose trophic dynamics are very different from privately owned reserves and ranches and should be compared at different levels), it can be seen that in low-competition settings like the reserve where she conducted her research, the amount of prey is well above the needs of the lion population, producing an effect that could be compared to the surplus-killing behavior exhibited by lions and hyenas in periods of prey abundance. This would be reflected in a less thorough consumption of carcasses, as is the case. For a closer example, several years ago I conducted some studies on lions that were kept in captivity and well fed. They barely consumed the flesh of complete carcasses. Working with felids outside their ecological context, where competition and resource availability shape their feeding habits, calls for caution when using such experiments as analogs.

Pobiner (2008:473) uses Blumenschine's data to claim that lions abandon more flesh in the Serengeti than reported by Domínguez-Rodrigo (1997a,1997b, 1997c) in Maasai Mara. She uses as support Blumenschine's (1986:86–89) data, which refer to the short periods during which prey abundance (coinciding with mass killings) is higher. These partially eaten kills make up only a small portion of the sample reported by Blumenschine. He immediately acknowledges that "the relatively large amounts of flesh abandoned during periods of prey abundance will quickly and thoroughly be scavenged by vultures . . . relatively large lion feeding group sizes result in the infrequent abandonment of any flesh. Unattended lion kills of medium-sized adults will therefore provide *little to no flesh at all times of the year* in the Serengeti, with further feeding opportunities being restricted to tissues within bones" (Blumenschine, 1986: 87; emphasis added). That is exactly what Blumenschine discovered: secondary access to lion kills in the Serengeti generally allows no access to flesh. Most carcasses are defleshed, just as in Domínguez-Rodrigo's (1997a, 1997b, 1997c) Maasai Mara sample. Ongoing research in Tarangire National Park in Tanzania is yielding exactly the same results. If we had to sum up the available results of flesh availability after lion feeding in national parks and reserves where these kind of studies have been made until present, we could not support Pobiner's interpretation of variability of flesh availability at kills reflecting the variability of ecological contexts. On the contrary, all of these studies show a similar amount of available flesh and the same anatomical distribution when the data have been collected in controlled samples. The data collected by Pobiner in the privately owned and anthropogenically modified reserve remain anomalous. Until proved otherwise from studies conducted in undisturbed ecosystems, defleshed small and medium-sized carcasses with few scraps available are therefore the most common feeding pattern exhibited by lions.

Assumption 4. Adequacy of the sample. Experimental samples should replicate (as much as possible) the archaeological samples in terms of the range of animal size and the range of body parts represented. This can be further defined by two independent analytical variables described by Domínguez-Rodrigo (2003): animal size used in butchery experiments (small versus large) and experiment type (using complete carcasses, all limbs, or only a few limb bones). Carcasses accumulated at Zinj comprise a large number of individual animals, documented (despite the abundance of limb bones) by all skeletal elements from small and large animals. The assumption is that an experiment replicating complete carcass consumption of small and large individuals would more accurately reflect what happened at Zinj than experiments based on a few bones from a single carcass size and from a single individual. Domínguez-Rodrigo and Barba (2005) and Pobiner and Braun (2005) showed that cut-mark patterns could be distinct in different carcass sizes. The premise is that to maintain the appropriateness of the comparability of experiments, cut-mark patterns obtained from specific carcass sizes should not be applied to interpret cut marks in different carcass sizes. In the Zinj case, the use of complete carcasses for experimentation might also be more adequate than partial carcasses. Only Capaldo (1995) uses this premise correctly. The other researchers either use one variable alone correctly, or both variables are inadequate. While Pobiner (2007) used complete carcasses, she never conducted a study of cut marks.

Assumption 5. Sample size and composition (see discussion in Domínguez-Rodrigo, 2003) are crucial for correct inference. Sample sizes in all the compared sets of experiments are highly variable, from large samples, like those obtained by Capaldo (1995, 1998) or Domínguez-Rodrigo (1997a, 1997b) for butchery of fleshed carcasses, to samples composed of multiple experiments of single elements or a pair of bones per carcass (Selvaggio, 1994), which do not reproduce the assumption that carcasses were accumulated at Zinj in a more complete state (whether hunted or scavenged). The interpretive model developed by Pobiner (2007) for flesh availability on large carcasses at lion kills is derived from a total of nine carcasses in contrast with Domínguez-Rodrigo's (1997c, 1999) sample of twenty-nine individuals where flesh distribution was documented and almost twenty carcasses from lion kills where secondary access was experimentally modeled. If an arbitrary threshold of a minimum of ten carcasses[4] per tested hypothesis (comprising at least complete limbs in each experiment) is used as a premise, some researchers' samples would be left out (Figure 2.1).

Assumption 6. To interpret the validity of cut marks to infer different butchery behaviors and primary or secondary access to carcasses by hominids, the observation of the anatomical distribution of flesh (whether bulk or scraps) in carnivore kills is not enough; experimental butchery is also necessary. Within Gifford-Gonzalez's (1991) conceptual scheme of hierarchical order of inference, the documentation of flesh at carnivore kills would be situated in the ecological sphere. From there to the final obtainment of cut-mark patterns (traces), one should be able to document how actors, with the aid of effectors, produce specific traces. Pobiner (2007) did not conduct any of these experiments, and her interpretation of the use of cut marks rests on the assumption that one can skip the experimental process linking ecology and traces by indirect assumption. This is conceptually flawed.

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[4] Experiments with a smaller number of carcasses usually yield large variation ranges, which make hypothesis testing more difficult.

Assumption 7. Butchery at Zinj was carried out with stone tools. The primary access hypothesis would assume that if hominids were hunters, they had regular access to carcasses and they might have been efficient and knowledgeable butchers. The alternative hypothesis, that hominids were scavengers that had only sporadic access to carcass remains, would imply that they might not have been expert butchers. The premise in the former hypothesis is that experimental butchery must be carried out by expert butchers, because butchery implies a learning process that is reflected in the decreasing number of cut marks imparted on bones according to experience; novice butchers leave more cut marks on bones than do expert butchers (Domínguez-Rodrigo, 1997c). The premise in the latter hypothesis that stone tools would be used to remove every single scrap of flesh and will not be focused on flesh bulk removal alone, which is most habitual in common butchery practices. For both hypotheses, a second premise is that the use of stone tools, preferably of the same raw material type as is archaeologically documented, is an experimental requirement.

Most researchers use these variables (tool type and butcher type) differently. Capaldo's and Lupo and O'Connell's butchery samples were made with metal knives, whereas Selvaggio's and Domínguez-Rodrigo's implied the use of stone tools of the same kind as found at Zinj. Selvaggio did not consider the experience of the butcher an important factor, however, and probably obtained higher frequencies (especially in certain bones) of cut marks than if an expert butcher model were used.

Assumption 8. The assumption of the type of carcass processing carried out at Zinj depends on the hypothesis to be tested. In the primary access hypothesis, processing assumes three butchery behaviors: skinning, disarticulation, and defleshing. From an optimal foraging point of view, the secondary access hypothesis assumes that the most efficient behavior is the removal of the flesh scraps at the kill. Disarticulation is the most time-costly butchering activity and also produces the highest degree of tool wear. Disarticulation of fairly defleshed carcasses, like those that one would obtain at carnivore kills, is unnecessary. In both cases, it is assumed that demarrowing followed. The premise is that in each of these hypotheses, no other type of processing activity should be experimentally reproduced.

Of the comparative sample of experiments, Domínguez-Rodrigo did not reproduce skinning and disarticulation. Capaldo's introduced an activity (periosteum removal) that is unnecessary for the butchery of most elements. This apparently irrelevant activity can actually produce a high frequency of tool marks on bones, biasing the assumed butchery behavior described above. Lupo and O'Connell's butchered carcass samples obtained from Hadza also include another processing activity not assumed for the Zinj hominids: grease extraction by bone boiling. Whereas this might not directly affect the resulting cut-mark frequencies, it indirectly affects them by limiting the type of bone fragmentation introduced by postravaging hyenas, thus, conditioning the resulting frequencies. Hyenas have been suggested to play a secondary role in bone modification and fragmentation at FLK Zinj (Bunn and Kroll, 1986; Domínguez-Rodrigo et al., 2007). Bone fragmentation ultimately determines bone surface modification frequencies. The hyena was one of the agents, other than hominids, that probably played a role in bone breakage at Zinj. Experiments suggest that hyenas are interested in modifying bones in human-accumulated bone assemblages when grease is available and preferably while it is fresh (Marean et al., 1992; Capaldo, 1995; Pickering et al., 2003; Marean et al., 2004). By deterring hyenas from early access to bones or by removing grease from bones during boiling, hyena postravaging is modified and therefore the degree of bone fragmentation is also modified, affecting the resulting bone surface modification frequencies.

Assumption 9. The species of the animals used for butchery experiments also could determine both the amount of flesh available for secondary access and the resulting cut-mark pattern from their processing. For instance, equids have stronger muscular attachments to bones (as reflected in the stronger muscular/ligament insertions on certain bones (e.g., caudal tibia and femur) than bovids, and consumption of their bones by carnivores tends to leave more flesh on abandonment (personal observation[5]). Likewise, human bulk defleshing of equids also produces more scraps of flesh than in bovids. As a result, equids tend to appear more highly cut marked than bovid remains. An example is provided by Lupo and O'Connell (2002) with various assemblages created by Hadza. In these bone sets, zebras tend to appear cut marked at rates more than one-third higher than bovids. Most of the processed animals at FLK Zinj were bovids. The assumption is therefore that experimental butchery should preferably be carried out on the same kind of carcasses for the sake of comparability. The premise is that experiments should be made by using bovids to test both hypotheses, and that experiments based on either butchery of equids or observation of flesh distribution in carnivore kills composed only of equids are not heuristically valid.

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[5] Several zebras were used by Domínguez-Rodrigo (1997a, 1997b, 1997c, 1999) in his observation of availability of flesh in lion kills and in his experimental replication of the scavenging hypothesis.

All but one of the experimental samples used for this comparative approach accepted the premise. Pobiner's (2007) sample of lion kills in the wild is primarily composed of zebras (eight zebras and one eland). The eland appears more defleshed than several zebras, and midshafts from upper limb bones appear virtually defleshed (see figure 2.10 in Pobiner 2007: 56). The resulting pattern documented in the equid sample is not adequate to infer flesh availability in bovids scavenged from lion kills.

Pobiner (2008) is critical with this point and requested some citations showing that cut-mark frequencies are different in equids versus bovids. She dismisses Lupo and O'Connell's (2002) data because she argues it depends on the data set. Domínguez-Rodrigo (2008) used all the data from equids butchered by Hadza that Lupo and O'Connell report, whereas she selects only the base-camp subsample because it is convenient for her arguments, not because they support any justified selection criteria. By doing that she renders the analogical sample smaller (which already is fairly reduced) and does not justify why selecting a smaller subsample is better than using the complete sample. Domínguez-Rodrigo (1997c) showed that experimental zebra remains frequently were more highly cut marked than bovid remains. A much larger body of data on cut-marked bones from bovids and equids can be obtained from the archaeological record. In extensive fossil samples of cut-marked bones, it can be seen that in assemblages where equids and bovid remains occur together, equids are substantially more highly cut marked than bovids (Voormolen, 2008; Yravedra, 2005; see also Yravedra [2001] for a summary of data and references from the Iberian Upper Paleolithic showing the same trend).

Pobiner (2008: 471) argues that "that the difference between flesh abundance on these two carcasses is due not to species, but to the number of lions feeding on them." To make such a statement, Pobiner (2008) should have contrasted both hypotheses, and whereas the number of lions feeding on a carcass obviously determined the amount of available flesh after consumption, the hypothesis that the prey taxa also determines the amount of available scraps remains untested by her study and cannot be rejected. Further experimental studies should be conducted by taking into account the taxonomic factor, but the reported differences suggest the influence of such variables as indicated earlier.

Assumption 10. This is one of the most important assumptions. Flesh is differentially distributed across the anatomy of an animal. The assumption is that a methodological approach that does not consider the type of element and the actual location of cut marks on these elements would not accurately reflect human butchery behaviors and the dynamics of these vis-à-vis the available scraps of flesh from carnivore kills, which are differentially distributed across the anatomy of carcasses. See an extensive methodological critique in Domínguez-Rodrigo (2002). The premise is that to reflect butchery behaviors accurately, cut marks have to be tallied according to element and bone section, as described in Domínguez-Rodrigo (1997a).

In the experimental comparative sample, Capaldo and Selvaggio are the only ones not to consider this assumption. They use a general method of tallying marks according to bone portion, irrespective of element type and the actual location of marks. They are also the only ones who have experimentally advocated equifinality in the use of cut marks as a result of their method, which lacked resolution in differentiating butchery behaviors.

Discussion

On the use of analogy

Some of the previous assumptions have failed to establish a link between their theoretical premises and the way in which they have been experimentally replicated (Figure 2.1). Their validity as analogies is therefore questionable. This, as published in Domínguez-Rodrigo (2008), has encountered some criticism (e.g., Pobiner, 2008) that relates to the core of the present debate: how analogy is built and how interpretations are epistemologically justified.

Paleoanthropological thinking is necessarily analogical. Some (Aronson et al., 1995; Bunge, 2006) would argue that all scientific reasoning is based on analogical modeling. Modern scientific analogical modeling differentiates between "descriptive models" and "explanatory models" in the relations of constituent models of hypotheses and theories (Aronson et al., 1995).[6] A school of thought in scientific realism argues that theories are structured around model systems (Giere, 1985) articulated in the form of type-hierarchy frameworks (Aronson et al., 1995). This school differentiates among positive, neutral, and negative analogies, although nonqualitatively.[7] A correct

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[6] Similarly, in theoretical archaeology, and applied to a smaller inferential scale, one has differentiated between "formal" analogies and "relational" analogies, the former being a mere transcription of an observed analog to the past, the latter being a constructed inference built on analogical reasoning (Gifford, 1981; Gifford-Gonzalez, 1991).

[7] These are defined as follows: "If A is a theoretical model for some real system B, then the positive analogy is those properties or respects in which A and B are similar. The negative analogy consists of those respects in which A and B are different, and the neutral analogy consists of those properties or respects which either have no corresponding map to the other or which have not yet been explored" (Aronson et al., 1995, p. 91).

use of analogical reasoning should combine these types. A mere formal analogy (e.g., female chimps use tools more frequently than male chimps do, and therefore early hominid females also preferentially used tools) uses the analog incompletely and incorrectly.[8] How could we test that female *Australopithecus* would have the similar tendency as female chimpanzees? Furthermore, very frequently the use of formal analogies is made without knowing their contextual or universal character (e.g., are female chimpanzees more habitual tool users everywhere?).[9] This dichotomy (formal and relational analogies) has been also referred to as the difference between "trivial" and "nontrivial" analogies (Harré, 1986; Aronson et al., 1995). Analogical reasoning in scientific interpretation is not based on the use of trivial formal analogies but on the elaboration of testable models. These are created through a dialectic use of groups of nontrivial analogies linked together.

Although archeological research has thrived under the use of analogical reasoning, little has been done conceptually to expand the use of analogy and to make it epistemologically supported. Analogy is at the core of several natural scientific disciplines. The field of theory of general systems has produced clearly defined concepts that scientific realists use to claim (1) that not all the analogies are equally valid, and (2) that to differentiate between valid and invalid analogies, an heuristic devise needs to be applied, which (3) can also be used to discriminate among the validity of scientific analogies that initially could be equally well structured and reliable.

One of the most widespread concept in the use of analogical reasoning for general and dynamic systems stems from Bunge's (1981) definition. Bunge criticized that most analogical reasoning was either undefined or too narrowly defined under isomorphic (and sometimes homomorphic) applications of the concept. He developed a qualitative concept of analogy embedded within the concept that most analogical reasoning in science occurs in dynamic systemic structures. These systems depend on the tight interaction of three components: composition, structure, and environment. Composition refers to the collection of components in any of two given systems. Structure refers to the relationship of those components within

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[8] Incompletely because it assumes that there is a perfect match between the analog (only positive analogy) and the model that needs to be elaborated to explain the reality of a past behavior. Formal analogies are also frequently used incorrectly because it is assumed that analogy and model are the same concept, whereas the latter is frequently composed of sets of analogies with attached testable hypotheses to overcome two facts: single observed analogies cannot represent the totality of a past behavior, and the analogy *per se* does not provide any bridging apparatus to test the adequacy of its application to the past.

[9] For a nonsupporting view see Carvalho et al. (2008).

each system. Environment impacts the structure by determining how the system components interact. This third element is of utmost importance because it shows that when comparing two systems (as analogical reasoning does), even if both systems have similar composition, their structure could be different because of the environmental differences of each of them.

From this point of view, two systems are *substantially analogous* when they share the same components, *structurally (or formally) analogous* when they share similar structures, and *environmentally analogous* when their contexts are similar.

To emphasize that not all analogical reasoning is equally valid, Bunge (1981) stressed that there were different (heuristic and epistemic) degrees of analogy. The degree of similarity between system A and system B could be proportional to their similarity in composition (degree of substantial analogy), structure (degree of structural analogy), and environment (degree of environmental analogy). The most important criterion in using degrees of analogy lies in the combination of the three types of intertwined parts of analogical reasoning, which is what Bunge (1981) identified as the degree of total analogy defined as the average of the degree of substantial, structural, and environmental analogies shared between two systems. Bunge expressed this definition in the following formula:

$$\alpha\,(\sigma 1, \sigma 2) = 1/3\,[\alpha_{\mathrm{C}}\,(\sigma 1, \sigma 2) + \alpha_{\mathrm{E}}\,(\sigma 1, \sigma 2) + \alpha_{\mathrm{S}}\,(\sigma 1, \sigma 2)]$$

where α is the degree of analogy, σ is for each system, C is for substantial analogy, E is for environmental analogy, and S is for structural analogy.

Bunge (1981) thus produced a final definition of analogy according to the result in the application of this logical formula in which he described two systems as *analogous* if their degree of total analogy was greater than 0, *weakly analogous* if their degree of total analogy was close to 0, and *strongly analogous* if their degree of total analogy was close to 1.

Bunge used this to show that not all analogies were equal in their heuristic power. Unless archaeologists (and especially taphonomists) assimilate this, they will continue to make epistemically blind interpretations produced as the result of matching prehistoric data with modern analogical frameworks, which could be conceptually inappropriate.

Pobiner's (2008) recent response to the critical description stated earlier on why several experimental programs fail to reproduce hominid butchery behavior ignores all these epistemological references (especially those that relate substantial and structural analogy to their environmental contexts) and raises several points that can be used to differentiate trivial from nontrivial referential analogues as we have seen above. She denies the relevance of the anthropogenic impact of the context where she conducted her research and justifies it by arguing that many of the settings where Domínguez-Rodrigo (1997a, 1997b, 1997c) conducted his experiments were also anthropogenically modified owing to the presence of pastoralists and the existence of poaching. This is inaccurate. The bulk of Domínguez-Rodrigo's research was conducted either in Maasai Mara or in the Olchorro le Musiara area of the reserve, where no pastoralists lived while he was conducting his research, nor was any poaching documented there for that period. Poaching and problems with humans became an issue only in the past decade and only in peripheral areas outside the region where Domínguez-Rodrigo conducted his studies, owing to the increase of the population surrounding the reserve (Norton-Griffiths, 1995; Norton-Griffiths et al., 2008). Despite this, the figures of game currently poached there are far below those reported for Serengeti (Campbell and Hoffer, 1995). Furthermore, poaching targets specific herbivore taxa, not carnivores, as is the case in the ranch where Pobiner conducted her research (Campbell and Hoffer, 1995). Therefore, the carnivore trophic dynamics of Maasai Mara and Olchorro le Musiara regions when Domínguez-Rodrigo was conducting his study remained similar to those of the Serengeti, thus explaining the similar results obtained in flesh availability at lion kills by independent studies (Blumenschine, 1986; Domínguez-Rodrigo, 1997a, 1997b, 1997c). The only place where Domínguez-Rodrigo (1996) conducted some independent research in a human-impacted environment was in Galana and Kulalu, near Tsavo, and lions abandoned their cattle prey in state similar to that described by Pobiner, because humans chased them. This is an informative coincidence. It was never argued that Serengeti and Maasai Mara were pristine ecosystems but that they are the closest we have (with other protected reserves and national parks) to natural trophic dynamics in savannas prior to the arrival of food producers. These ecosystems are fairly different in terms of mammalian trophic dynamics from private properties used by humans as hunting grounds, where certain carnivore taxa are reduced to the limit of survival. The former remain the closest proxy for Plio-Pleistocene savannas. The latter are something different, and their application to the past remains epistemologically unjustified.

Pobiner questions the assertion that felids and hyenids were abundant in Olduvai Bed I times, which is crucial to select the adequate modern proxy for interpreting trophic dynamics in the Olduvai paleolandscape and modeling the resulting availability of scavengeable resources. Obviously neither predator populations nor herbivore biomass can be determined for the past; however, Domínguez-Rodrigo justified it because their remains are relatively abundant for this period compared with other Plio-Pleistocene sites and because in all the sites that have been taphonomically analyzed, their intervention in the formation of faunal assemblages has been documented (Domínguez-Rodrigo et al., 2007). As a matter of fact they have been argued to be responsible for all but two of the Olduvai Bed I and Bed II sites (Domínguez-Rodrigo et al., 2007). This interplay of felids and hyenids is documented in several modern national parks and reserves but lacking in the reserve where Pobiner conducted her studies. It is therefore logical to claim that the former can be potential proxies and the latter ought to be excluded.

Pobiner argues that three ecological circumstances could produce similar meat surpluses to those documented in her research: droughts, mass drownings, and scavenging from saber-toothed felids. There is a problem with this: mass drownings are a very marginal occurrence in modern savannas and affect only specific herbivore taxa. They do not occur in reduncini and antilopini, nor in alcelaphini adapted to edaphic grasslands (e.g., topi in modern savannas or its extinct counterpart *Parmularius*), which form the bulk of the FLK Zinj herbivores. None of the bulk of the taxa exploited at FLK Zinj have been documented in mass drownings. Ongoing work at Olduvai Bed I has uncovered various sources of water during the formation of the Bed I sites (Domínguez-Rodrigo et al., 2010). This presents an excellent testing case for the drought and mass drowning hypotheses. Both processes produce concentrations of carcasses near the remaining water sources (mass drowning in lake environments [Capaldo and Peters, 1995] and carcasses accumulating during droughts in river beds and ponds [Haynes, 1991]). The number of carcasses that accumulated near these water sources during the formation of FLK Zinj is extremely small (much smaller than at water sources in modern savannas in the absence of extreme climatic conditions), showing that neither phenomenon is the source of the carcasses butchered by hominids during FLK Zinj times.

Regarding the other possibility of scavenging large amounts of flesh from saber-toothed felids, it should be stressed that Pobiner references Marean (1989) as support, selectively ignoring the later work by Marean and Ehrhardt (1995) on a *Homotherium* den, which showed that saber-toothed felids defleshed carcasses more thoroughly than previously thought based on tooth morphology alone. Furthermore, saber-toothed felids were also subjected to the competition created by thousands of other carnivores in Plio-Pleistocene savannas. This brings into question: (1) whether flesh availability as documented by Pobiner in an almost competition-free environment could be applied to these felids, and (2) whether these felids could have afforded to be such inefficient flesh eaters given the competitive environment to which they were adapted. What Pobiner has epistemologically modeled is not resource availability at sabertooth kills, but strictly resource availability at lion kills in a savanna context lacking competition and with altered carcass consumption habits by lions because of human presence and humans chasing them. The epistemological bridge between this analogue and its application to the Plio-Pleistocene past is missing. Despite this, Pobiner's (2008) claim that saber-toothed felid kills could be a good source of scavengeable meat remains untested and with decreased heuristics after Marean and Ehrhardt's (1995) work. Recently, the hypothesis of a scavenging niche made viable by sabertooths because they may have lacked the morphology necessary to use all parts of carcasses fully, leaving an open niche in the form of high-quality scavengable remains available for hominins, has received a further blow. Quantifications of occlusal radii-of-curvature (ROC) of carnivore premolars and the study of the correlation of this morphology with carcass-processing behavior

> do not support the hypothesis that sabertooth felids were more hypercarnivorous than modern felids (*but the opposite*). Thus, this study shows no evidence that members of the paleo-carnivore guild were capable of producing higher quality scavengable carcasses than are modern carnivorans, and based on these analyses of fossil carnivorans, it does not appear that high-quality scavengable remains were more available in the Plio-Pleistocene than there are today" (emphasis added). (Hartstone-Rose and Wahl, 2008: 630)

Pobiner's belief in the "scavenging from sabertooth kill" hypothesis, in absence of empirical support and contradicted by currently available evidence, requires another leap of faith.

Pobiner argues that "the amount of flesh abandoned on lion kills is highly variable, e.g. bulk or flesh scraps on 18 per cent of lion-eaten larger prey carcasses (Domínguez-Rodrigo 1997a, 1999) vs. 56 per cent (Tappen 2001 – unknown predators on larger adult ungulates) vs. 70 per cent (Blumenschine 1986) vs. 95 per cent (Pobiner, 2007), and dependent on a series of ecological variables." I argue that this is incorrect. Blumenschine (1986) does not report 70% of bulk flesh surviving lion consumption of carcasses (otherwise he could not claim that flesh and viscerae are not available for a secondary scavenger). Pobiner is using data from the short periods of prey abundance, which do not reflect what happens in the Serengeti the rest of the year (see also Schaller [1972]). Blumenschine's (1986: table 4.8) data for bulk flesh from medium-sized adult carcasses after abandonment by lions is less than twelve, more similar to what Domínguez-Rodrigo (1999) reports in Maasai Mara. Likewise, Tappen's (2001) data should be taken with caution. She documents a list of carcasses found in virtually two states: complete or defleshed. The description of the defleshed carcasses is similar to those reported by Blumenschine and Domínguez-Rodrigo. There is no variation in lion feeding behavior if lions were responsible for consuming them. The complete carcasses present an important problem: they cannot be shown to be the result of lion kills. Most of them were found without any definitive indicators that lions had hunted them. They could easily be natural deaths. Once again there is a sample derived through an uncontrolled procedure. If lions have not been witnessed to kill the carcasses, the interpretation that lions abandoned fully fleshed carcasses cannot be fully supported. By lumping the complete carcasses with the defleshed carnivore-eaten carcasses, Pobiner is artificially creating a bulk estimate that is epistemologically unsupported. If we consider the carcasses that carnivores (probably lions) ate, their description does not support the 56% bulk flesh survival inferred by Pobiner and is very similar to what has been reported for Serengeti and Maasai Mara. Furthermore, Tappen never published any quantification of resource availability that would have allowed any quantifiable estimates of surviving bulk flesh. Pobiner's (2008) method of deriving it from mere general descriptions is thus flawed.

In sum, no heavily fleshed carcass survives lion consumption in the Serengeti or Maasai Mara on a regular basis but instead on exceptional occasions. Simpson (1970) argued that our analogs should be constructed based on regularities and not exceptionalities. From what is the most commonly documented pattern, therefore, secondary access to flesh in small and medium-sized carcasses abandoned by lions in modern savannas (not modified by humans) remains a highly marginal scenario. This is ultimately reflected on the frequencies and anatomical location of cut marks when these carcasses are butchered with stone tools.

Conclusion

I have argued here that a systemic evolutionary taphonomic approach (as outlined by Fernández-López, 2006), considering taphonomic entities as endowed with properties subjected to change according to their structure, behavior, and environment, also shows that the selection of criteria to be replicated in experiments ultimately depends on what has been called *taphonomic redundancy*. Taphonomic redundancy, a crucial element in the way that we construct analogies, is the capacity of taphonomic elements to repeat the same message. Taphonomic redundancy, as well as replication, allows the estimation of the "repeatedness" of taphonomic groups under particular environmental conditions, on the basis of their actual properties (Fernández-López, 2006). This was also argued as essential by Simpson (1970) in his concept of configuration and regularity. Unlike universal immanent properties, configurational properties are based on regularities in the variables that structure them. There are two types of configurational processes: (1) those that are highly variable and therefore difficult to predict; and (2) those that are highly regular and therefore predictable. Obviously, only the latter can be reliably applied to past dynamics.

The way we construct analogies determines to which extent their configurational properties respond to highly variable or highly regular properties. Pobiner's reliance on exceptional occurrences (e.g., "as lions sometimes abandon kills of larger animals with large amounts of flesh, hominins scavenging from social felid kills could have access to well-fleshed carcasses" [Pobiner 2008: 476]) produces analogies that cannot be applied to the past: they lack taphonomic redundancy, and therefore they produce ambiguity. They are not highly regular and thus are unpredictable and by extension nonapplicable, because they are not solidly tied to environmental conditions observable in the absence of human impact on carnivore trophic/competition dynamics. Furthermore, their application to interpreting the past is linked to inferential scenarios based on untestable hypotheses (e.g., scavenging large amounts of flesh from saber-toothed felids was feasible). There is an important missing link between past and present, which is the correspondence between the premises used for testing hypotheses and the way in which testing is implemented.

The range of interpretations about the use of cut marks to infer human butchery behaviors, derived from the experimental sets compared in the present work, is not a reflection of the variability of these behaviors and their ecology but is a methodological artifact of the diversity of assumptions made in experimental design and their corresponding experimental premises. Researchers have reacted differently to what is supposed to be tested and the way in which testing was conducted. Some researchers claim ambiguity in the use of cut marks not because they can prove it, but because of their methods of documenting cut marks (Assumption 10), or because they disregarded the determinant interrelated inferential categories of ecology behavior and skipped the hierarchy of inferential categories. Others failed to document the utility of cut marks to reconstruct butchering behaviors because their selection of premises to articulate their hypotheses and the corresponding variables used during experimentation were different from those that should have been inferred and used to interpret the targeted fossil assemblage. Figure 2.1 shows how far each researcher is from the experimental matrix created by the articulation of assumptions and their corresponding premises. The more distant the experimental sets are from the matrix, the less heuristically appropriate they are to interpret cut marks from the fossil assemblage. Some of the studies casting doubt on the utility of cut marks (e.g., Pobiner, 2007) mistake the degree of comparability of the data sets used, disregarding ecology, behavioral variability, and confiding in untested assumptions to the point of not even replicating butchery when testing the secondary access hypothesis.[10]

Only one out of the researchers whose work has been compared claims that cut marks can be used successfully to differentiate between primary access to fleshed carcasses or secondary access (Domínguez-Rodrigo, 1997a). Challengers to this claim could support their position either by proving that the set of assumption-premises used by that researcher is equivocal or by using the same experimental premises to document a greater variety of results than that reported in the referential framework provided by his or her research. Instead of that, by selecting a different experimental path, they set themselves up to test-prove something similar but essentially different.

This brings us to reconsider the use of analogy and the importance of combining its substantial, structural, and contextual-environmental properties (Bunge, 1981). Given the variability of criteria when one is designing experiments and using analogies, an outline containing the set of inferences and premises guiding hypothesis modeling seems necessary. This will help researchers understand when new results from experiments provide new compelling evidence to challenge established ideas, or when they simply represent the testing of a completely different set of premises and assumptions, even if the hypotheses appear to be the same.

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[10] Pobiner (2007) assumes that the distribution of flesh in lion kills that she observed is enough to discredit the utility of cut marks when applied to fossil assemblages, without proceeding to remove flesh through butchery and compare it to a null-hypothesis experimental scenario.

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CHAPTER 3

The use of bone surface modifications to model hominid lifeways during the Oldowan

Charles P. Egeland

Ever since the ground-breaking taphonomic work of Bunn (1981) and Potts (Potts and Shipman, 1981) documented cut marks on bones from early Pleistocene deposits at Olduvai Gorge and Koobi Fora, bone surface modifications have played an increasingly prominent role in understanding the formation of Oldowan faunal assemblages. The analysis of surface modifications, which include most prominently hominid butchery (cut marks, percussion marks) and carnivore (tooth marks) damage, can address many important issues in Oldowan archaeology, including (1) Which carcass resources did Oldowan hominids exploit? (2) How often did they obtain carcasses? (3) When they did acquire carcasses, did hominids have their choice of resources, or was the menu limited to what was available after other carnivores had had their fill? (4) What was the nature of the interaction between hominids, as a relatively new member of the large carnivore guild, and Plio-Pleistocene carnivores? These questions, and thus the analysis of bone surface modifications, must be integrated into any model that seeks to shed light on the socioeconomic function of Oldowan sites.

The role of bone surface modifications in understanding faunal assemblage formation

The process of faunal assemblage formation can be usefully understood in three distinct, albeit interdependent, components (Egeland et al., 2004: 345). The first is carcass acquisition. This involves gaining access to a carcass regardless of the mode of that access (e.g., hunting or scavenging) or the nutritional condition of the carcass (e.g., fresh or desiccated). The second is carcass accumulation. Here, a carcass or carcass part is transported to and eventually deposited at a particular locale on the landscape. The third component is carcass modification, which occurs when bones or parts thereof are broken or partially/wholly destroyed. It is during this last process that bone surface modifications are created.

Two important points must be made here. First, carcass modification, and thus the infliction of bone surface modifications, can occur at any stage of assemblage formation. Second, the modification component of assemblage formation is the most directly inferred because bone surface modifications provide one of the few unambiguous taphonomic indicators of hominid and carnivore involvement with bones (assuming, of course, that they can be correctly identified; see discussion later). What flows from this is the realization that the formation of a faunal assemblage, be it Oldowan or Neolithic, simply cannot be addressed with any rigor without the analysis of bone surface modifications.

The role of actualism in identifying and interpreting bone surface modifications

The perspectives offered in this chapter are all guided by actualism, which involves "observing present-day events and their effects in order to give meaning to the prehistoric record" (Gifford, 1981: 367; see also Simpson, 1970; Lyman, 1994: 46–69; Pobiner and Braun, 2005a). Because it provides unambiguous linkages between traces (e.g., a mark on a bone), causal agencies (e.g., a stone tool slicing a bone), effectors (e.g., a sharp-edged flake), and actors (e.g., a hominid wielding a stone tool; terminology follows Gifford-Gonzalez, 1991), actualism, and the uniformitarian assumptions that accompany it, provide *the* critical referential framework for understanding past processes.

Marean (1995) has provided a useful distinction between naturalistic and experimental actualism. Experimental studies directly control the variables that produce the observed traces, as in studies that purposely vary tool raw material to examine differences in cut-mark frequencies between, for example, obsidian and flint flakes (Dewbury and Russel, 2007). Naturalistic research observes actors and their resultant traces but does not intentionally manipulate the variables. An excellent example of this form of actualism is found in Blumenschine's (1986) observations on the natural sequence by which carnivores in the Serengeti ecosystem consume different carcass parts. As we will see, actualistic studies, both experimental and naturalistic, play a central role in reconstructions of hominid butchery behavior and hominid/carnivore interactions in the Oldowan.

Types and morphological features of hominid and carnivore bone surface damage

The utility of bone surface modifications depends wholly on our ability to link a taphonomic trace (e.g., a linear striation on a bone) to a taphonomic actor (e.g., a hominid using a stone tool to butcher a carcass). Therefore, identifying attributes that reliably and consistently distinguish between different types of taphonomic actors is of paramount importance. The best way to become familiar with the morphological features of surface modifications is to work with actualistic assemblages; in these situations, one can be sure that the process of mark creation was observed, and thus the taphonomic actor(s) is known unambiguously. Because excellent discussions of signature criteria for hominid and carnivore damage are already available (Blumenschine et al., 1996; Fisher, 1995), the next two sections provide only a brief summary of these attributes as revealed through actualistic studies.

Hominid damage

Hominid-imparted damage includes those marks created by either sharp- or blunt-edged stone tools (hominid tooth marks are discussed later). Stone tool cut marks (Figure 3.1) appear as fine, linear striations with V-shaped cross-sections that often possess parallel to subparallel microstriations both within and on the wall of the main groove (*shoulder effects*; Bunn, 1981; Potts and Shipman, 1981; Shipman and Rose, 1983). Some cut marks also preserve barbs, which are small hooks that occur at the heads and/or tails of cut marks that result from "small, inadvertent motions of the hand either in initiating or in terminating a stroke" (Shipman and Rose, 1983: 66).

Percussion marks (Figure 3.2) result from the use of unmodified (simple cobbles) or modified (e.g., choppers or polyhedrons) hammerstones to breach the medullary cavities of long bones for fat-rich marrow. Classic percussion marks "occur as pits, grooves or isolated patches of microstriations" (Blumenschine, 1995: 29). Microstriations are found within and/or emanate from the percussion pit (Blumenschine and Selvaggio, 1988, 1991; Turner, 1983; White, 1992). In addition to classic percussion marks (pits with associated microstiations), analysts have also identified *striae fields*, which "are composed of extremely shallow, subparallel scratches that usually cover relatively expansive lengths of cortical surface, between 5 and >50mm" (Pickering and Egeland, 2006: 462; see also Turner, 1983;

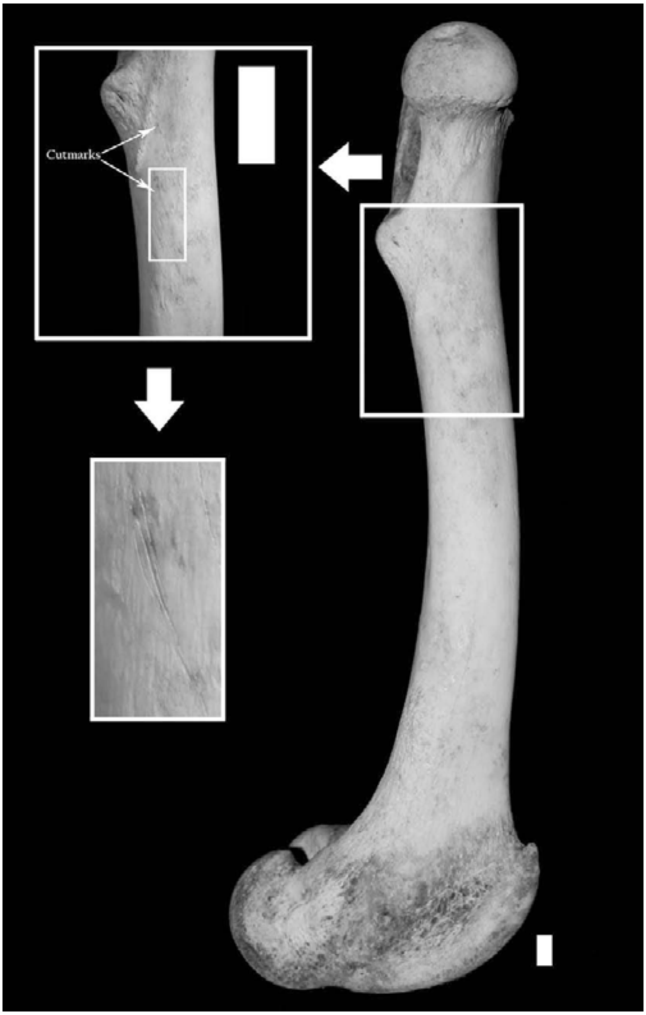

FIGURE 3.1. Medial view of the left femur of an elk (*Cervus elaphus*) showing stone tool cut marks. These cut marks were created during experimental butchery. Note the parallel to subparallel orientation, deep, V-shaped cross-section of the marks, and in the close-up, the multiple striations that are created within the grooves of the main marks. All scale bars = 1cm.

White, 1992). Experimental data show that striae fields are more often located on the surface of the bone in contact with the anvil (when one is used), rather than the surface that is impacted by the hammerstone (Pickering and Egeland, 2006). The microstriations so common to and diagnostic of percussion marks are produced when the grains of a hammerstone scrape or abrade against the bone surface during impact; however, experimental work has demonstrated that percussion marks can also manifest themselves as pits without associated microstriations. Galán et al. (2009), for example, report that nearly one-third of the percussion marks created by unmodified hammerstones lack microstriations in their sample, and because of this, they cannot be distinguished morphologically from carnivore tooth marks. Overall, however, percussion pits with associated microstriations tend to be produced in the highest frequency in experimental assemblages, followed by striae fields and then pits without microstriations (Galán et al., 2009).

Carnivore damage

The morphological features of carnivore damage have been described in some detail by Binford (1981: 44–49), Blumenschine (1995: 29), Blumenschine and Marean (1993: 279–280), Blumenschine et al. (1996: 496), Fisher (1995), Haynes (1980), and Shipman (1983). Although there is interanalyst variability in terminology, four main categories of carnivore damage are generally recognized (Figure 3.3): furrowing, punctures, pits, and scores (Binford, 1981: 44). Furrowing is caused by sustained chewing of the soft cancellous regions of bones and is frequently manifest as partial or total destruction of bone portions, particularly the epiphyses of long bones. Crenulated edges (following Pickering and Wallis, 1997: 1118) are the final product of furrowing and can therefore be included in this category. Tooth punctures result from the bone collapsing under the tooth and are characterized by distinct holes in the cortical surface. Tooth pits are roughly circular in plan view whereas tooth scores are elongated (typically with a length three or more times greater than the width; Selvaggio, 1994) with U-shaped cross-sections. Both pits and scores commonly show internal crushing as a result of tooth-on-bone contact.

Several workers have correctly pointed out that it might not be safe to assume that all tooth marking can be attributed to carnivores, and hominids in particular must be considered as potential tooth-marking agents (Brain, 1967, 1969, 1981; Landt, 2007; Martínez, 2009; Pickering and Wallace, 1997; White and Toth, 2007). Unfortunately, there are currently no diagnostic criteria that appear to clearly differentiate human chewing damage from that of carnivores (although attempts are underway; Fernández-Jalvo and Andrews, 2011). One might expect to find more hominid chewing

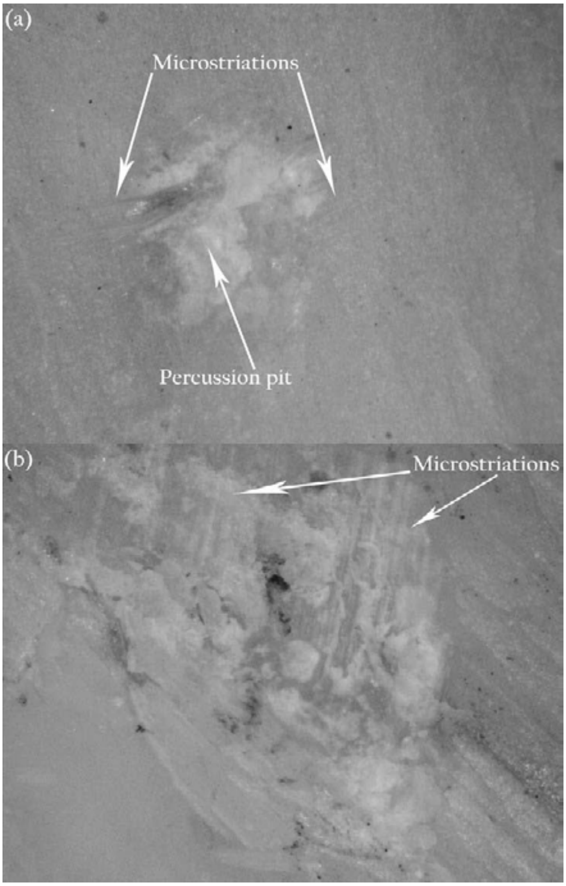

FIGURE 3.2. Examples of percussion marks. Note the microstriations emanating from the percussion pits. Photos courtesy of M. Domínguez-Rodrigo.

damage on smaller carcasses, as bones from larger carcasses would have more often been processed with stone tools rather than orally. Other non-carnivore agents that could be applicable to Oldowan sites for which tooth mark data exist include nonhuman primates, suids, crocodiles, and raptors (Andrews, 1990; Domínguez-Solera and Domínguez-Rodrigo, 2009; Njau and Blumenschine, 2006). Another class of bone surface modification that is relevant is digestive damage. Bones that have either been regurgitated after some time in the stomach or that have passed completely through the gastrointestinal tract of a carnivore (or hominid) often show characteristic thinning and rounding (Lyman, 1994: 204–205, 210–211).

Tooth mark dimensions and identifying carnivore types

There is a growing body of research aimed at identifying species-specific patterns of bone modification among carnivores (Andrews, 1995; Andrews and Armour-Chelu, 1998; Andrews and Fernández-Jalvo, 1997; Delaney-Rivera et al., 2009; Domínguez-Rodrigo and Piqueras, 2003; Haynes 1983; Pickering et al., 2004a; Piqueras, 2002; Pobiner, 2007; Pobiner and Blumenschine, 2003; Selvaggio, 1994; Selvaggio and Wilder, 2001). There are two main reasons why information on the type or types of carnivores involved in assemblage formation is important. First, carnivores are very diverse in their level of sociality, and one can easily imagine that a gregarious species like the spotted hyena would have posed a different competitive dilemma for Oldowan hominids than, say, a relatively solitary species like the leopard. Second, differences in body size and dental armament furnish carnivores with a variety of carcass modification abilities, which in turn has an effect on the availability of carcass resources to other consumers, including hominids. These factors are even more important given the greater diversity of the large carnivore guild during Oldowan times relative to today (Werdelin and Lewis, 2005).

In terms of bone surface modifications, the underlying logic is very simple: larger carnivores have larger teeth, which in turn create larger tooth punctures, pits, or scores. Most analyses have used digital calipers to measure tooth mark dimensions from high-quality negative molds of bone surfaces. As long as only well-defined tooth marks are used and the outlines are clearly marked on the molds, this method provides reliable results. In some cases, however, the molding material can peel off some of the cortical surface, particularly in poorly preserved assemblages. Digital imaging techniques can circumvent this problem, and Delany-Rivera et al. (2009) describe a reliable method that uses digital photography and open-source imaging software.

Unfortunately, the relatively simple theoretical relationship described here has proved to be somewhat more complicated in practice. The most comprehensive datasets show that there is much overlap between carnivore species in the dimensions of tooth marks. For example, Domínguez-Rodrigo and Piqueras (2003) find that the length and breadth maxima of tooth pits created by cheetahs, leopards, lions, spotted hyenas, large dogs, and jackals on the dense cortical bone of limb bone diaphyses only reliably

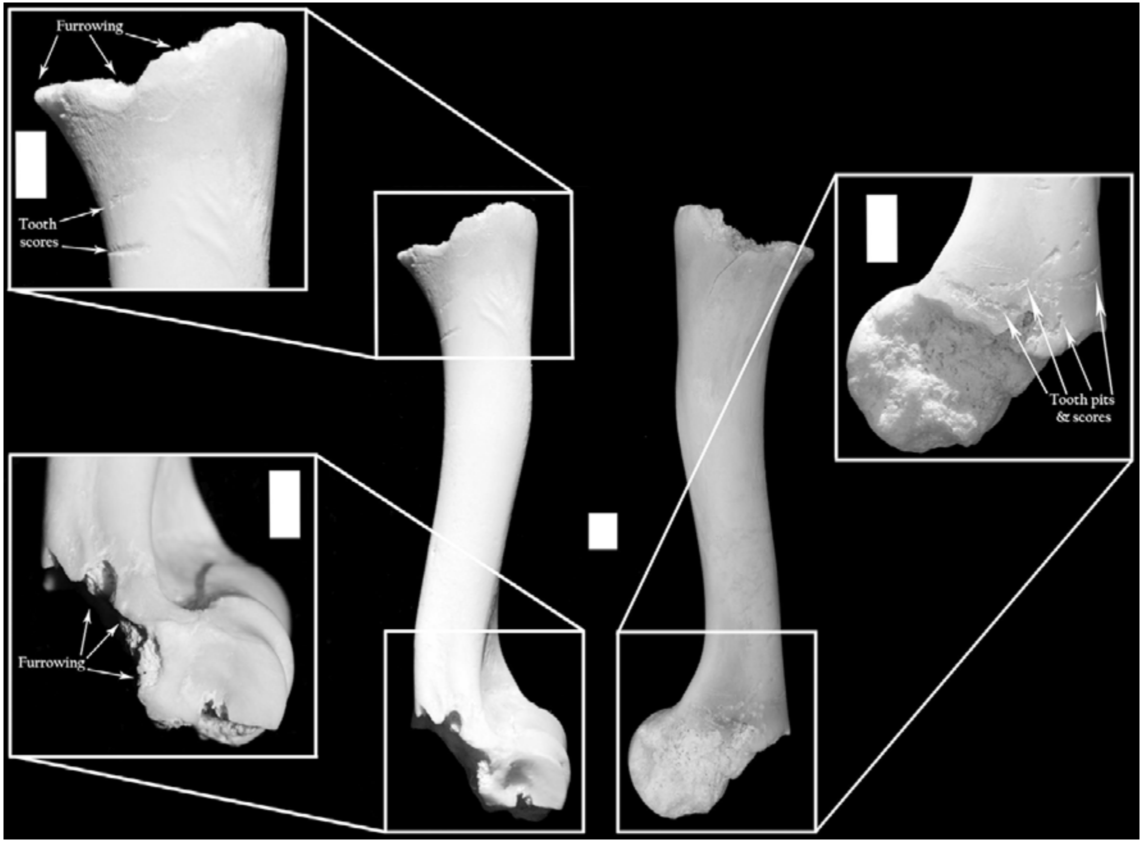

FIGURE 3.3. Medial (*right*) and lateral (*left*) views of the right humerus of a white-tailed deer (*Odocoileus virginianus*) showing various types of carnivore damage. This damage was created by a captive male mountain lion (*Puma concolor*). All scale bars = 1cm.

separate carnivores into two groups: those with less robust dentitions (e.g., cheetahs, leopards, and jackals) and those with more robust dentitions (e.g., large dogs, lions, and spotted hyenas). Although it is likely that the size of tooth marks alone will not identify the species of carnivore responsible for their creation, other taphonomic data, including carcass size, levels of bone destruction, and the frequencies and anatomical placement of tooth marks, can aid in more precisely identifying the carnivore(s) involved in the formation of Oldowan assemblages.

Protocol, problems, and pitfalls in the identification of bone surface modifications

Minimally, all surface mark identification in either actualistic or archaeological contexts should be carried out with a strong light source and the aid of at least 10× to 16× magnification under hand lenses or binocular microscopes. Using this methodology, Blumenschine et al. (1996) report that expert analysts accurately identify experimentally produced surface marks at rates of 99%, whereas novices with less than three hours' training with experimental controls achieve identification rates of 86%. Some researchers recommend the use of higher magnification (80×–750×), including scanning electron microscopy (SEM), for confident identifications of surface marks (e.g., Andrews and Cook, 1985; Olsen and Shipman, 1988). While such magnification might be warranted for the small number of ambiguous marks that invariably occur in any fossil assemblage, SEM analysis in particular is a time-consuming and relatively costly undertaking. Therefore, this method should be carried out only on subsamples of marks that lack distinctive morphologies when viewed under binocular microscopes or hand lenses.

As discussed previously, actualistic data clearly show that surface marks possess diagnostic morphologies, and further, that these morphologies are in most cases identifiable under relatively low magnifications. We must now confront one of the greatest challenges for Oldowan taphonomists: translating morphological criteria generated from actualistic assemblages of known derivation to fossil assemblages of unknown derivation. The reality is that, in the latter case, confident associations of surface marks with particular taphonomic actors are much less straightforward. The point here is that a familiarity with "pristine" actualistic assemblages of butchered and/or carnivore-ravaged bone is not enough; one must also take into account the myriad factors that can, in the case of the Oldowan, impact bone assemblages over one or two (or more) million years. A *configurational approach* to surface mark identification therefore should be practiced, in which not only mark morphology but also the anatomical placement of the mark and the sedimentary context from which the specimen derives are taken into account (as recommended by researchers like Binford [1981], Bunn [1991], and Fisher [1995], and implemented by various researchers).

As an example of the importance of such an approach to surface mark identification, consider the Oldowan faunas from Member 3 at Swartkrans Cave (South Africa) and the site of DK in Bed I at Olduvai Gorge (Tanzania), which date to between 1.8 and 1.0 and about 1.8 million years ago, respectively. Both faunas are relatively well preserved and were excavated in association with stone tools. Ideally, an Oldowan taphonomist familiar with the morphological features of stone tool cut marks (based on actualistic assemblages) would identify prehistoric butchery damage on the bones and then proceed to reconstruct hominid subsistence at the sites. The issue is complicated, however, by the fact that the Swartkrans and DK bone surfaces have been affected by a variety of biotic and abiotic taphonomic processes typically not operant in modern actualistic assemblages. Manganese formation, soil leaching, water action, bacterial and fungal growth, subaerial weathering, and even glue from specimen curation are present in one or both assemblages, all of which complicate the identification of surface marks.

One of the more common processes at these (and other) Oldowan sites is sediment abrasion. Such damage results from trampling and/or fluvial transport and is known to manifest as fine, linear striations similar to stone tool cut marks. Based on actualistic samples, several researchers have summarized the morphological characteristics of sediment abrasion (Behrensmeyer et al., 1986, 1989; Fiorillo, 1989; Oliver, 1989; Olsen and Shipman, 1988). The most recent experimental work indicates that a multivariate approach can help to distinguish sediment abrasion from cut marks morphologically. When comparing marks created by (1) stone tool butchery and (2) pedestrian trampling generated by human subjects walking on bones embedded in various sedimentary substrates, Domínguez-Rodrigo et al. (2009) observe that only cut marks create grooves that are substantially deeper than they are wide, have microstriations located on the wall (as opposed to the base) of the groove, are straight (as opposed to sinuous) in trajectory, and show microstriations that are continuous along the entire groove. Recall, however, that a configurational approach involves other attributes in addition to mark morphology.

At Swartkrans, the karstic colluvium that makes up the cave fill from which the bones derive contains high frequencies of large, angular (i.e., sharp) clasts, which hold great potential to create morphological cut mark mimics. This, coupled with the host of other taphonomic factors that had affected the cortical surfaces, encouraged Pickering et al. (2004b, 2007) to take a conservative approach to mark identification in their study of the Member 3 fauna. Importantly, the corroborating opinions of three analysts were required to accept a particular specimen as preserving cut marks (and/or any other sort of surface mark). While it is possible that some specimens that actually did preserve prehistoric cut marks were rejected as such, this is preferable to incorrect identifications. On the other hand, the DK fauna was excavated from a relatively fine-grained sedimentary matrix (as are most of the Bed I sites at Olduvai). There, sediment abrasion produced very superficial striae that contrast markedly with the deep grooves interpreted to have been inflicted by stone flakes. In addition, sediment abrasion resulted in randomly oriented striae that did not cluster near muscle attachments (Figure 3.4), which is different from the clusters of parallel to subparallel cut marks in the DK assemblage (Egeland, 2007a,b).

Quantification and analysis of bone surface modifications

There are several ways to quantify bone surface modifications (for useful summaries see Abe et al., 2002; Lyman, 1994: 303–306). The simplest method calculates the proportion of the total number of identified specimens (NISP) in any one category (e.g., femoral fragments, vertebral fragments, fragments from large animals) that preserve surface marks. Although probably the most commonly reported quantification method, researchers have cautioned that NISP-based data can be affected by differential fragmentation. Consider a simple example. Let us say that two bones, a humerus and a femur, were butchered by a hominid, and that all of the flesh was removed in the process. As a result, cut marks were created in several discrete clusters on the bone surfaces. Now, the still-hungry hominid decides to crack open both bones to get at the fat-rich marrow inside. This marrow extraction results in the creation of two humerus fragments and ten femur fragments. All else being equal, a NISP-based calculation will likely result in a lower cut-mark frequency for the femur (there are simply more fragments, the denominator) than for the humerus – even though both bones were butchered with the same intensity (i.e., until all of the flesh was removed). Although obviously a very simplified example, this potential shortcoming has led some researchers to suggest alternative methods of quantification. For example, Bartram (1993) argued that surface mark frequencies should also be counted as the proportion of complete bones

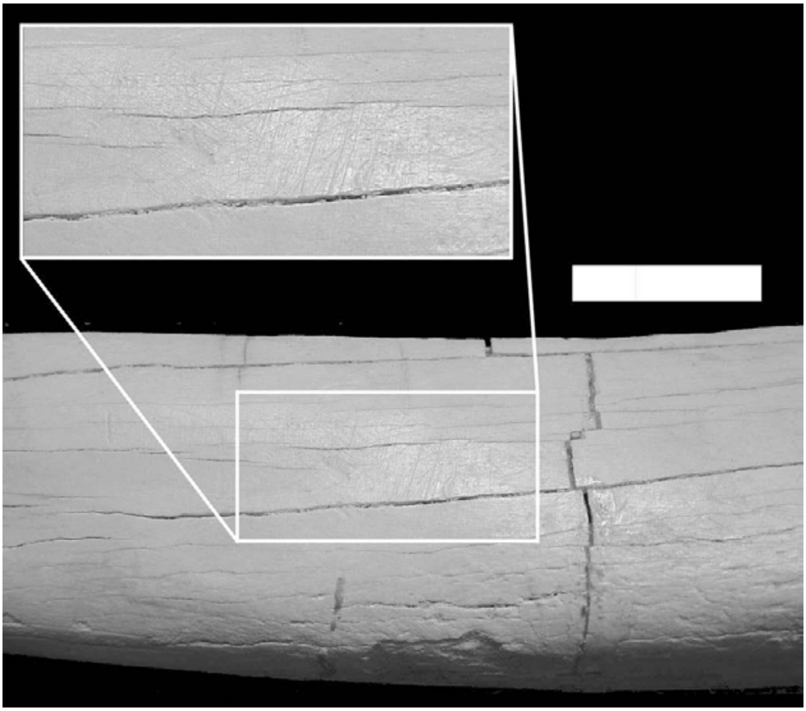

FIGURE 3.4. Tibia fragment from the site of DK in Bed I at Olduvai Gorge (ca. 1.8 million years old) showing sediment abrasion, probably caused by trampling. Note the shallow, randomly oriented striae (contrast with the stone tool cut marks in Figure 3.1). Scale bar = 1cm.

of any one element (as represented by the bone fragments and estimated by the minimum number of elements [MNE] count) that preserve surface marks. Others (Abe et al., 2002; Rapson, 1990) calculate surface mark frequencies relative to the surface area of the bone or bone fragment (a smaller bone or bone fragment is less likely to preserve a surface mark simply because it has less surface area). Another method of quantification that is seldom reported involves counts of individual marks on a specimen. The goal here is to count each spatially discrete mark that could have resulted from a single action (e.g., a cutting stroke, single hammerstone or anvil impact, and/or a tooth cusp coming into contact with a bone). Ideally, each cut-mark striation can be counted, although this is easier said than done when the striations are tightly clustered (Egeland, 2003). For percussion marks, each pit and its accompanying microstriae should be counted individually. Striae fields tend to be more diffuse and difficult to count, but Pickering and Egeland (2006) suggest that striae fields >5mm apart should be considered distinct. Each discernable tooth pit, score, or puncture should be counted separately, although again this can be difficult or impossible on specimens with intense chewing damage.

The anatomical placement of surface modifications also provides vital information on carcass utilization by both hominids and carnivores. For example, cut marks on the midshafts of limb bones have been shown to reflect flesh removal, whereas those clustering near the epiphyses are most likely to be the result of dismemberment (Binford, 1981; Nilssen, 2000). Composite diagrams of surface marks (particularly cut marks) overlain on paper templates with multiple views of bones has been common practice among taphonomists for decades. It is now relatively easy to do this digitally with any number of image analysis software programs. For example, Adobe Photoshop (and other similar programs) allows users to create "layers," each of which could represent, for instance, the location of surface marks for a particular bone fragment. These layers can then be viewed and analyzed in any number of combinations depending on the analyst's needs. Abe et al. (2002) have developed a GIS-based add-on in ArcView that not only records digital drawings of surface marks but also performs several quantification functions as well. The one drawback of templates (digital or otherwise) is that the only fragments that can be included are (1) those that can be identified to skeletal element and (2) those that can be accurately oriented anatomically.

One method of tallying surface marks that circumvents the identifiability problem, at least for limb bones, is Blumenschine's (1988: 467) bone segment approach. In this system, limb bone fragments are divided into three categories: (1) epiphyseal specimens, which bear "all or a portion of the proximal or distal articular surface"; (2) near-epiphyseal specimens, which lack "any articular surfaces, but preserving cancellous tissue on the medullary surface that is indicative of proximity to an epiphysis"; and (3) midshaft specimens, which lack "articular surfaces and cancellous bone." Although Blumenschine's (1988) system is extremely successful at determining the order of carnivore access to carcasses (more on this later) its implementation has one potential shortcoming; namely its insensitivity to the *actual location* of a particular surface mark. For example, because most epiphyseal specimens as defined by Blumenschine (1988) include an attached portion of shaft, it is impossible to tell if a marked epiphyseal fragment actually bears marks on the articular surface or if the marks in fact occur on the attached shaft. Therefore, high frequencies of marked epiphyseal fragments could give the false impression that marks cluster near the joints when most actually occur on midshaft sections.

Bunn (2001: 209–210) and Domínguez-Rodrigo (1997: 674) therefore take a slightly different approach by tallying surface marks by bone sections. In this case, limb bones and limb bone fragments are divided into three anatomical sections: (1) proximal and distal epiphysis; (2) proximal and distal shaft; and (3) midshaft (note, however, that fragments must be identified as coming from, for example, the distal epiphysis of a particular limb bone). Therefore, a single specimen in Blumenschine's segment system might include one or more sections in the Bunn/Domínguez-Rodrigo system. Here, surface mark analysis tracks the actual location of marks, which can aid in determining the order of hominid access to carcasses and identifying the type(s) of carnivore responsible for carcass modification.

Actualistic samples and the timing of hominid and carnivore access to carcasses

The preceding has prepared us for an in-depth examination of a growing body of actualistic studies that provide data on the frequency and anatomical location of surface marks. The goal of these studies is to aid reconstructions of hominid butchery practices and to assess the timing of hominid and carnivore access to carcasses. Because limb bones tend to survive at high frequencies relative to other skeletal elements such as vertebrae and compact bones, actualistic studies have tended to focus on these bone types. Blumenschine's (1988) pioneering work in particular has fostered several studies that provide surface mark data on limb bone specimens. Three general scenarios are modeled by these studies. The first involves carcasses that are processed completely and exclusively by human experimenters (i.e., human- or hominid-only). In these experiments, bones are stripped of flesh with metal or stone knives and then cracked open to expose the marrow cavity. This produces limb bone assemblages with either cut marks or percussion marks, but obviously no carnivore tooth marks. The second scenario involves the defleshing of limb bones by various mammalian carnivores, followed by the fragmentation of the bones, mainly by hyenas (carnivore-only). The final scenario models the sequential utilization of carcasses in so-called dual- or multipatterned models (Blumenschine and Marean, 1993; Capaldo, 1995). The basic premise of dual-patterned studies is that a carcass processed by previous consumers "offers a carnivore [or hominid] a shortened menu of parts and a reduced nutrient yield compared to that available on a whole carcass" (Blumenschine and Marean, 1993: 275). These experimental actualistic data have been supplemented with ethnoarchaeological studies of bone modification carried out among modern foraging groups such as the Hadza of Tanzania (Lupo and O'Connell, 2002).

Because the shaft portions of limb bones are more structurally dense than other parts (Lam et al., 1999), they tend to best survive the rigors of an assemblage's taphonomic history, particularly carnivore ravaging (Pickering et al., 2003). Therefore, midshaft fragments (*sensu* Blumenschine, 1988; see earlier) should provide the least-biased sample from which to calculate bone surface modifications. When carnivores have sole access to complete limb bones, they strip them of flesh and break them open to access marrow and grease, which results in well over 50% of midshaft fragments displaying tooth marks (Blumenschine, 1988, 1995; Capaldo, 1995, 1997; Selvaggio, 1998). In dual-patterned assemblages, in which hammerstone breakage and marrow extraction by humans is followed by carnivore (mainly hyena) ravaging, midshaft fragments are tooth marked at rates much lower than 50% (usually less than 20%; Blumenschine, 1988, 1995; Blumenschine and Marean, 1993; Capaldo, 1995, 1997). The explanation for this is very simple: hammerstone-broken midshafts no longer encase the nutrient-rich marrow cavity, which leaves scavenging carnivores little or no reason to tooth mark them. In dual-patterned assemblages where humans remove the flesh but leave the marrow cavities intact, midshaft tooth-mark frequencies remain high. The critical observation from these actualistic studies is that tooth-mark frequencies on midshafts provide a useful measure of the timing of carnivore access to within-bone nutrients; that is, did they get there before or after hominids broke the bones open for marrow?

As informative as tooth marks are in determining carnivore access to carcasses, they provide only indirect evidence on the carcass processing behavior of hominids. For direct evidence we must turn to butchery marks. Actualistic and ethnoarchaeological datasets indicate that when humans break open all bones in an assemblage to access marrow, percussion mark frequencies range from about 10% to 35% of midshaft NISP (Blumenschine and Selvaggio, 1988, 1991; Pickering and Egeland, 2006). When humans have primary access to flesh, cut-mark frequencies cluster around 5% to 40% of midshaft NISP (Bunn, 1982; Domínguez-Rodrigo, 1997, 1999b; Domínguez-Rodrigo and Barba, 2005; Lupo and O'Connell, 2002). Cut-mark frequencies in situations in which humans have secondary access to carcasses (removing scraps of flesh after carnivore consumption) are typically less than 10% of midshaft NISP (Domínguez-Rodrigo, 1997; Selvaggio, 1998). Other studies show, however, that there is no consistent relationship between the amount of flesh that is removed and cut-mark frequencies (Egeland, 2003; Pobiner and Braun, 2005b). The considerable variation in and overlap of butchery mark values is due to several factors, including the size and taxon of the carcass, the intensity with which carcasses are butchered, tool raw material, and experimental protocols (Domínguez-Rodrigo, 2008; Domínguez-Rodrigo and Yravedra, 2008; Galán et al., 2009).

Oldowan taphonomists have attempted to solve this conundrum in several ways. Some have suggested that rather than lump all limb bone fragments together in the analysis, one should analyze them in a way that is sensitive to the amount of nutrients that they can provide to a potential consumer. Upper limb bones (femur/humerus) have substantial amounts of flesh and encase large reservoirs of marrow, whereas intermediate (tibia/radio-ulna) and lower limb bones (metapodials) have progressively less resources associated with them. Carnivores are also aware of this and in fact broadly follow this pattern in their consumption sequences (Blumenschine, 1986). It would logically follow, therefore, that when gaining access to carcasses before carnivores, hominids would butcher (and thus impart butchery marks on) those bones with the highest nutrient yields. Actualistic research does suggest that upper limb bones are cut marked at higher rates than are intermediate and lower limb bones when humans gain primary access to carcasses (Domínguez-Rodrigo, 1997; Domínguez-Rodrigo and Barba, 2005).

Actualistic work indicates further that the mere presence of cut marks on midshaft fragments may be meaningful in terms of hominid access. The argument goes like this: carnivores typically remove flesh from carcasses in a predictable sequence (Blumenschine, 1986), and if allowed to take their time, will usually leave little or no adhering flesh on midshaft sections of limb bones (Domínguez-Rodrigo, 1999a). So, if hominids were relegated to passively scavenging from picked-over carnivore kills, there would be no reason for them to impart cut marks on defleshed midshaft sections. Cut marks on limb bone midshafts (or any body part that is usually consumed early on by carnivores; for instance, rump flesh around the pelvis or viscera under the ribs) therefore would mean that a substantial amount of flesh was present when hominids butchered the carcass. Domínguez-Rodrigo and Barba (2007b) have taken this a step further by mapping the exact anatomical location of flesh scraps in a sample of twenty-eight carcasses consumed by lions in Kenya's Maasai Mara National Reserve. Those areas on limb bones that never preserved any flesh scraps were referred to as *hot zones* by these researchers (Domínguez-Rodrigo and Barba, 2007b: 90). They argue that if cut marks appear on hot zones, it is very unlikely that hominids accessed carcasses after they had been fully defleshed by felids (or other large carnivores). Other studies indicate that the amount of flesh that remains after large carnivore consumption can vary considerably (Blumenschine, 1986; Pobiner, 2007; Selvaggio, 1994), which, as Pobiner (2008: 473) points out, is likely due to differences in ecological context. For example, all else being equal, one would expect that consumed carcasses found in areas with low carnivore density would retain more flesh than those found in areas with high carnivore density. Regardless, it has become clear that the anatomical placement of bone surface modifications is as important, if not more so, than the frequency of their occurrence (Pickering and Egeland, 2009).

Comparing surface mark frequencies between fossil and actualistic assemblages

The researchers who have conducted these actualistic studies have correctly stressed the importance of comparability between modern datasets and fossil assemblages (Blumenschine, 1995: 28, 33–39; Capaldo, 1997: 556–557; 1998: 312–314; Marean, 1991; Selvaggio, 1994: 194). There are two major issues here, the first of which concerns breakage. This is important because the amount of breakage controls how many fragments are created, and the number of fragments is the denominator in NISP-based counts of surface mark frequencies. Any process that creates additional fragments in fossil but not in actualistic assemblages therefore can artificially depress surface mark frequencies in the former relative to the latter. In fact, fossil assemblages are often exposed to just such processes. For example, while actualistic controls experience breakage related only to the extraction of carcass nutrients by humans and/or carnivores (referred to as *green breakage*), fossil assemblages can undergo additional breakage due to ancient diagenetic processes such as weathering or sediment compaction and/or recent breakage from excavation or curation damage. (Although a detailed discussion is beyond the scope of this chapter, there are established methods to determine when and how a bone was broken.) Of course, the only way to eliminate such bias completely would be to refit all the diagenetically and/or recently broken specimens. For various reasons, not the least of which is time constraints, this is rarely possible. Therefore, the most straightforward way of maximizing comparability is to exclude from comparative analyses the fossil specimens that show diagenetic and/or recent breakage, *even if they preserve prehistoric surface marks*. Pickering et al. (2007, 2008) offer an alternative method that allows the inclusion of specimens with diagenetic and/or recent breakage without the need for extensive refitting. They reason that because at least two fragments will be produced when a single specimen is broken, the number of diagenetically and/or recently broken specimens can be divided by two and the resulting value added to the number of green-broken specimens. Although this does not completely eliminate the bias introduced by differential breakage, it can help make fossil assemblages more comparable with actualistic control samples.

The second issue involves cortical surface preservation. In actualistic assemblages, all bone cortices are more or less pristine, and therefore any analyst who knows what to look for should be able to identify all the marked specimens accurately. As one might well expect, this is not always the case in fossil assemblages: poor cortical surface preservation results from myriad factors and can obscure prehistoric surface marks. To realistically compare fossil bone modification data with those of actualistic controls, one would need to calculate surface mark percentages based only on that portion of the assemblage that displays cortical surfaces that are well preserved enough to maintain identifiable prehistoric marks. Of course, what constitutes well preserved is another matter. In his study of faunas from Bed II at Olduvai Gorge (dated to between about 1.7 and 1.2 million years ago), Monahan (1996) assesses the "readability" of bone surfaces in quartiles, where a specimen whose entire cortex is in pristine condition is coded as 100% readable; less well-preserved specimens are coded as 75% to 99%, 50% to 74%, and so forth. He then creates adjusted surface mark frequencies by multiplying the number of marked specimens by the percentage of specimens with >50% readability. Monahan (1996) and others (Egeland et al., 2004) used these adjusted values to make comparisons between the Bed II data and actualistic assemblages. Pobiner et al. (2008) use a similar scheme in their analysis of faunas from Okote Member sites at Koobi Fora, Kenya (dated to about 1.5 million years ago) but consider specimens >75% readable as well preserved. The upside of this approach is that it is very explicit about how specimens are chosen for comparative analysis. The problem is that surface marks often appear in isolated or very restricted areas of bone fragments, and so even a specimen that is 75% readable might have once had tooth marks on what is now the remaining 25% unreadable surface. In other words, we are assuming here that the readable surface of a fragment is representative of the entire cortical surface, which might or might not be the case. Pickering et al. (2007, 2008: 33) take a more qualitative approach by assigning a score of *poor*, *moderate*, or *good* to specimens from Swartkrans, which is meant to indicate "the relative 'fidelity' of current bone surfaces for continuing to preserve prehistoric bone surface modifications." In this system, only specimens with good preservation are included in comparative analyses. The drawback here, as Pickering et al. (2008: 33) admit, is the subjective nature of the scoring system: the analyst must make the call as to which specimens are deemed well preserved enough to merit an assessment of good and thus inclusion in comparative analyses (see Thompson [2005] for a useful methodology in a non-Oldowan context).

Apart from these two major concerns, there is one, final consideration when making comparisons between fossil and actualistic assemblages. Many of the actualistic studies do not consider bone specimens that are <2 cm in maximum dimension. These specimens must therefore also be removed from the fossil sample, even if they preserve prehistoric surface modifications.

What have bone surface modifications taught us about the Oldowan?

The point of this chapter is to convey the importance of bone surface modifications to understanding the lifeways of Oldowan hominids. So, let us examine what we think we know with a good degree of certainty:

Hominids were using Oldowan stone tools to butcher animals for food. This is the most secure and will probably be the most lasting contribution of bone surface modifications to our understanding of the Oldowan. Although it was long assumed that the mere presence of stone tools was enough to demonstrate that the associated fossil bones were the remains of early hominid meals (e.g., Clark and Haynes, 1970; Leakey, 1971), it was not until the discovery of cut marks at Oldowan sites (Bunn, 1981; Bunn et al., 1980; Potts and Shipman, 1981) that this relationship was confirmed beyond any doubt. Percussion marks and other fracture features show that early artifacts were used as marrow-processing implements as well (Blumenschine, 1995; Bunn, 1981; Oliver, 1994). In fact, the site that boasts some of the earliest evidence in the world for the use of stone tools as butchery implements, Bouri in Ethiopia at about 2.5 million years ago, lacks stone tools entirely: confirmation of carcass processing comes solely from the butchery marks on the bones (de Heinzelin et al., 1999). Butchered bones have been discovered in association with the very earliest evidence for stone tool manufacture 2.6 million years ago at the site of Gona, also in Ethiopia (Domínguez-Rodrigo et al., 2005), which indicates that from its very inception, Oldowan technology was used, at least in part (see below), for processing carcasses.

Hominids were also using Oldowan stone tools for activities unrelated to carcass butchery. Bone surface modifications, or, in this case the lack thereof, also reveal that at many Oldowan sites stone tools were *not* being used to butcher the fossil bones that co-occur with them. This pattern is particularly pervasive in Bed I at Olduvai Gorge, where only the fauna from the 1.8 million-year-old Level 22 at the FLK locality (the *Zinjanthropus Floor* or *FLK Zinj*) is demonstrably anthropogenic, even though stone tools occur at many of the other Bed I sites (Bunn et al., 2010; Domínguez-Rodrigo et al., 2007a, 2010b). For what then were the stone tools at these sites being used? Given the undeniable importance of plant foods in early hominid diets (Peters, 1987; Sept 1992), it is likely that resources like nuts and roots would have dictated when and where hominids chose to concentrate their tool-using activities at Olduvai (Peters and Blumenschine, 1995) and beyond (Sept, 2001). Recent analyses of the Bed I lithics support this contention (Díez-Martín et al., 2010; Mora and de la Torre, 2005).

The menu of Oldowan hominids was relatively diverse. Butchery marks have been documented on animals that range in size from hedgehogs to elephants. This demonstrates that Oldowan hominids were willing and able to acquire carcasses of animals that were in some cases significantly larger than themselves and certainly larger than those procured by any modern nonhuman primate (Boesch and Boesch, 1989; Stanford et al., 1994; Uehara, 1997; Uehara et al., 1992; Watts and Mitani, 2002). Butchery marks are also found on animals that have habitat preferences ranging from open grassland to dense woodland (Blumenschine and Pobiner, 2007). This indicates that Oldowan hominids exploited animals from a wide variety of habitats, a pattern that also contrasts markedly with what is seen among nonhuman primates.

Carnivores were involved, in some form or another, in the formation of nearly every Oldowan site. Although it is often overlooked that early pioneers of paleoanthropology in Africa acknowledged that carnivores might have played some role in the accumulation and/or dispersal of faunal remains at early archaeological sites (e.g., Leakey, 1971; Isaac, 1971), a fuller appreciation of this was gained only with the formal integration of taphonomy into paleoanthropology through the work of researchers like Brain (1967, 1969, 1981). The near ubiquity of carnivore involvement in the formation of Oldowan faunas is attested by the presence of carnivore tooth marks (in addition to other lines of evidence) at nearly every early site with decent bone preservation (granting, of course, that some of the tooth marks might have come from the hominids themselves). Therefore, the question in many cases is not "*Did* carnivores contribute to this bone assemblage?" but rather "*How* did carnivores contribute to this bone assemblage?"

Hominids and carnivores overlapped in their use of space and, in some cases, overlapped in their use of individual carcasses. The co-occurrence of stone tools and/or butchered bones with fossils that bear carnivore tooth marks shows that hominids and carnivores used the same areas on the landscape during Oldowan times. The temporal dimension of this association is less clear, however: that is to say, were hominids and carnivores on site at the same time, or was the overlap in space separated by months, years, or even decades? The answer for both questions seems to be *yes*. For example, there are sites such as FLK North in Bed I Olduvai (approximately 1.7 million years old) where hominid tool-using activities and carnivore carcass consumption appear to have been carried out in largely independent and unrelated episodes, separated by at least months if not longer (Bunn et al., 2010; Domínguez-Rodrigo and Barba, 2007a; Domínguez-Rodrigo et al., 2010a). At this and other Oldowan sites, however, there are examples of the co-occurrence of hominid and carnivore surface modifications on the same bone specimen. Because soft tissue remains nutritionally attractive for only a limited amount of time, this strongly suggests the sequential use of the same individual carcasses by hominids and carnivores likely within days (or less) of each other. This of course does not necessarily mean that hominids and carnivores were vying with each other for control of carcasses in dramatic competitive interactions (although this might have occurred sometimes). For instance, Isaac (1983: 9; see also Binford, 1983; Isaac and Crader, 1981), in his "common amenity" model, suggested that water, food, and other amenities probably drew hominids and carnivores to the same locations, in many cases independently, over many years.

In many cases, hominids enjoyed early access to carcasses. Perhaps no debate has sparked as much controversy as that over the carcass acquisition strategies of Oldowan hominids. Were they active hunters that could monopolize carcasses or timid scavengers relegated to picked-over carnivore kills? As with most debates, such a dichotomy grossly oversimplifies the complexity of the topic. Nevertheless, let us briefly dichotomize here before returning to a more realistic interpretation in a later section. Bunn (1981, 1982, 2001, 2007; Bunn and Ezzo, 1993; Bunn and Kroll, 1986) has argued consistently over the years that the frequency and anatomical location of cutmarks clearly indicate that Oldowan hominids were butchering fully fleshed carcasses. This implies that hominids were getting to carcasses before carnivores could consume them. Blumenschine (1995) and others (Capaldo, 1997; Selvaggio, 1998), based mainly on tooth mark and percussion mark data, contend that hominids scavenged felid-killed prey that they subsequently harvested for the remaining flesh scraps, bone marrow, and brain. Binford (1981; Binford et al., 1988) went even further and suggested that hominids were limited solely to heavily ravaged carcasses that provided nothing more than some marrow and perhaps bits of flesh. It is of note that Oldowan taphonomists have roundly rejected Binford's claims because he paid little heed to the bone surface modification data, which has since contradicted his *marginal scavenger* hypothesis. Although the previously mentioned arguments have been based mainly (although not exclusively) on the evidence from the large and well-preserved FLK Zinj assemblage, steadily accumulating evidence from other Oldowan sites over the past 25 years or so has largely confirmed Bunn's original argument (Domínguez-Rodrigo, 2002; Domínguez-Rodrigo et al., 2002, 2007a; Egeland and Domínguez-Rodrigo, 2008; Ferraro, 2007; Pickering et al., 2004b, 2007, 2008; Pobiner et al., 2008). The general pattern that is emerging involves (1) cut marks on the midshafts of long bones, and more specifically on hot zones; and (2) low tooth-mark frequencies and high percussion mark frequencies on midshaft fragments. These two factors indicate that hominids were gaining access to and butchering fully fleshed carcasses and then breaking open long bones to access marrow. In most cases, carnivores later scavenged the bone refuse.

Where do we go from here?

Now let us examine some issues that need to be worked out: *OK, Oldowan hominids were eating large animals, but how important were meat and other animal products in their diets?* There is no question that Oldowan hominids exploited animals in ways unknown among other members of the Primate order, and it is probably not a coincidence that the earliest evidence for this exploitation coincides with the earliest stone tool technologies. To gauge how significant this dietary shift was for hominid biology and behavior, however, we must be able to say not only that hominids were getting ahold of carcasses but *how often* they were doing so. If Oldowan hominids were accessing carcasses on a regular basis, it is possible that a higher-quality diet based on easy-to-digest animal protein and fat could have fueled changes in brain size, modifications to life history, and expansions in range size (Aiello and Wells, 2002; Antón et al., 2002; Kaplan et al., 2000; Leonard et al., 2007). If so, Bunn's (2007) assertion that "meat made us human" may not be far off the mark.

Although there are several lines of evidence that can and should be used to examine this idea, butchery marks are the most critical because they alone document unambiguously the processing of carcasses by early hominids. We already know from butchery mark data that hominids in many cases were able to gain early access to carcasses, although this does not necessarily mean that animal products were integral components of their diets. What about the number of Oldowan sites that show evidence of butchery? Of the seventy-nine Oldowan assemblages from Africa that have preserved faunal material, thirty-one, or about 39% of them, preserve butchery marks of some sort (Pobiner, 2007: tables 8.1 and 8.2).[1] Given that the faunas from some of those sites that lack butchery evidence suffer from poor cortical preservation, it is likely that this number is actually higher. Butchery evidence is rare at Oldowan sites outside of Africa, although this is likely to change as the sample of sites of this age increases (e.g., Martínez et al., 2010). This suggests that carnivory was a temporally and spatially widespread behavior, at least among Oldowan populations in Africa.

Another way to look at this problem is to determine the actual number of mammal carcasses that were butchered by hominids at Oldowan sites. Data on the minimum number of individuals (MNI) that were butchered are available from only a few sites. Two of the Olduvai sites show relatively high frequencies of butchered animals: FLK Zinj has the most, with a butchered MNI of eighteen (Domínguez-Rodrigo and Barba, 2007b), whereas at BK there is evidence of at least eleven butchered carcasses (Egeland and Domínguez-Rodrigo, 2008), although new excavations at the site will raise this total (Domínguez-Rodrigo et al., 2009). Pobiner (2007) reports butchered mammal MNIs of between nine and eleven at three 1.5-million-year-old Okote Member sites from Koobi Fora. These numbers actually compare quite favorably to those observed among modern hunter-gatherer camps (Bartram, 1993; Bunn et al., 1988; O'Connell et al., 1988a). However, we know that modern camp refuse represents usually less than a year of occupation, whereas the formation times of the Oldowan sites must be inferred. One can see, for example, how different the carcass acquisition rates at FLK Zinj would be if the assemblage formed over the course of no more than three years, as Bunn and Kroll (1987) have argued, relative to what they would be it had taken a decade or more to accumulate (Potts, 1986). The same issue must be worked out at other Oldowan sites as well.

OK, Oldowan hominids were gaining early access to carcasses in most cases, but were they actually hunting? The hunting prowess of Oldowan hominids really hits at the core of their humanity, because many think that proficient hunting of larger mammals (i.e., weighing more than about twenty or thirty pounds) indicates a more human-like adaptation. To discern active hunting from other forms of early carcass access such as mass deaths (e.g., mass drowning; Capaldo and Peters, 1995, 1996; but see Domínguez-Rodrigo et al., 2010b) or *power scavenging* (i.e., aggressively driving

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[1] Pobiner (2007) lists 77 assemblages with 29 preserving butchery marks. Subsequent to her summary, butchery marks have been identified at two additional sites/levels: FLK North Level 4 (Domínguez-Rodrigo et al., 2007b) and Swartkrans Members 1 and 2 (Pickering et al., 2008) for a total of 79 total assemblages with 31 preserving butchery marks.

carnivores off kills; *sensu* Bunn, 1996: 322) is extremely difficult to do with bone surface modifications. To demonstrate this unequivocally with surface mark data, one would need to identify impact marks created by projectiles (Letourneux and Petillon, 2008; Smith et al., 2007). Without this sort of evidence, which is currently lacking for the Oldowan, we are forced to speculate a bit. Bunn (e.g., 2007: 198) favors hunting for the acquisition of smaller animals, because lions and hyenas can consume such carcasses very quickly, and advocates power scavenging as the most likely acquisition strategy for medium-sized animals. Domínguez-Rodrigo and Barba (2006) point out that the ability of modern hunter-gatherers like the Hadza to drive predators off kills is based largely on their use of heavy bows (which are effective up to 40 meters). Given the lack of bow technology during the Oldowan, they therefore argue that confrontational scavenging would have been too dangerous an undertaking. This, in their opinion, leaves active hunting as the most likely alternative. Although no unambiguous hunting implements are preserved at Oldowan sites, Domínguez-Rodrigo et al. (2001: 298) have suggested that evidence for woodworking at Peninj, a 1.5 million-year-old site in Tanzania, might indicate the production of rudimentary spears. Is this all to say that Oldowan hominids never passively scavenged carcasses? Of course not; they certainly did when the opportunity presented itself, and in fact modern hunter-gatherers are always on the look-out for scavengeable carcasses (O'Connell et al., 1988b). Bunn and Ezzo (1993: 388) probably put it best when they state that hominids used a "flexible and sophisticated strategy of carcass acquisition that involved as the dominant methods active, confrontational scavenging to acquire large animals and both active scavenging and opportunistic hunting to acquire small animals. As part of this flexible, broadly based strategy, passive scavenging probably did occur, but not enough for it to be reflected as a significant, dominant factor in the known archaeological record."

Conclusion

The take-home messages from this chapter should be

1. As long as we can identify them correctly, bone surface modifications represent an unambiguous link between a carcass and whatever interacted with that carcass (in our case, hominids and carnivores).
2. Bone surface modifications are absolutely critical to any discussion of the formation of a bone assemblage, and thus to any broader discussion of Oldowan hominid behavior.

3. Familiarity with the diagnostic features of surface marks should be gained through the study of actualistic assemblages of known derivation. These morphological features cannot be applied uncritically to fossil assemblages that have experienced complex taphonomic histories, however. Therefore, a configurational approach to mark identification should always be practiced.
4. Fossil samples must be corrected to account for poor surface preservation and/or differential breakage before they are compared with modern actualistic assemblages.
5. Both the frequency occurrence and anatomical patterning of surface modifications are important factors to consider in determinations of hominid and carnivore access to carcasses.
6. Surface marks have taught us quite a bit about the lifeways of Oldowan hominids. We now know, for example, that (1) hominids used Oldowan tools both to butcher animals and, most likely, to process plant resources; (2) hominids exploited a greater diversity of mammalian prey than any nonhuman primate; (3) carnivores were active in the creation of nearly every Oldowan bone assemblage; (4) hominids and carnivores used the same places on the landscape, and in some cases fed from exactly the same carcasses; and (5) hominids were capable of gaining early access to fully fleshed carcasses.
7. Other issues are a bit cloudier when it comes to the use of surface modification data. For instance, we cannot be sure just how important animal resources were in the diets of Oldowan hominids based solely on surface marks. As it currently stands, the issue of active hunting is also an open question (although conclusive evidence of such is potentially attainable through bone surface modifications).

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CHAPTER 4

On early hominin meat eating and carcass acquisition strategies: Still relevant after all these years?

Karen D. Lupo

Of all the resources consumed by people, meat and animal fat are often given special values among contemporary hunter-gatherers. Meat is shared more often and more widely with unrelated people than other resources, and fatty meat is most often identified as desirable or preferred (so-called meat hunger) by contemporary hunter-gatherers (Abrams 1987). Compared with the diet of nonhuman primates, the human diet has a higher percentage of meat (L. Cordain 2002). Hunting medium- and large-sized game is often revered as a prestigious activity, and good hunters can receive a variety of nonconsumptive social and political benefits such as deference among peers, allies, and increased mating opportunities (Bird and Smith 2005). In human evolution, hunting and meat eating are often viewed as a catalyst for the use of home bases, transport of food, provisioning, and the sexual division of labor (Isaac 1978; Isaac 1971; Kaplan 2000) and is implicated in the evolution of anatomical and physiological features associated with early *Homo* including changes in dental morphology and masticatory abilities, modern gut proportions and digestive kinetics, increased brain size, and a trend toward a slow-paced life history (Aiello and Wheeler 1995; McHenry 1994; Milton 2003; Wood and Collard 1999). Still others view the archaeological evidence of hunting and the use of efficient hunting technologies as an index of modern behavior (Binford 1985; Binford 1988; Klein 1995; Klein 2000; McBrearty and Brooks 2000). More recently, the proportion of meat and animal fat in the diet of ancestral human populations (e.g., Paleolithic or preagricultural diet) is identified as a crucial issue for understanding the current epidemic of cardiovascular and metabolic diseases that plague postindustrial and emerging nations (Colagiuri and Miller 2002; Cordain 2005; Eaton and Eaton 2001). Thus, the evolution, timing, and nature of the consumption of animal products by ancestral hominins have been and continue to be central issues in human evolution.

Inferences concerning how, when, and why early hominins obtained carcasses, and by implication, the quantity, quality, and predictability of animal protein in the diet have generated some of the most intense and controversial debates in paleoanthropology (Binford 1981; Bunn and Kroll 1986). The fallout from the now decades-old hunting-versus-scavenging debate has challenged archaeologists to develop and apply systematic analytical techniques to paloearchaeological assemblages and generated some of the most sophisticated and rigorous actualistic studies conducted to date, especially in taphonomy. Perhaps one of the most enduring contributions of the hunting-versus-scavenging debate is that it exemplified attempts to break away from ethnographic analogy and conceptualize carcass acquisition strategies (and lifeways) that were not recapitulated by contemporary or historic hunter-gatherers. Unfortunately, the tone of these debates at times has obscured the broader behavioral inferences that are often linked to carcass acquisition strategies and meat eating and that provide some of the most compelling reconstructions of hominin behavior currently available.

This chapter reviews the current state of the debate by focusing on recent hominin carcass acquisition models built on zooarchaeological taphonomic evidence. Because these models are rooted in the premises and concepts developed in earlier studies, I begin by briefly reviewing the hunting-versus-scavenging debate over the last few decades. This retrospective is not exhaustive, because some very comprehensive reviews of the debate already exist (Bunn and Ezzo 1993; Domínguez-Rodrigo 2002; Domínguez-Rodrigo, et al. 2007; Marean and Assefa 1999; Rose and Marshall 1996). This chapter highlights how current zooarchaeological models of hominin carcass acquisition strategies are challenged by the high degree of variability displayed by taphonomic phenomenon. Thus far, researchers have traditionally met this challenge using a bottom-up approach. This approach has produced a large body of useful and descriptive taphonomic data that can be used as a backdrop for evaluating the zooarchaeological record. A central weakness to this approach is that it fails to generate theoretically based predictive hypotheses about the range and nature of informative taphonomic phenomenon. Several current ideas derived from life history theory allow researchers to view the emergence of hominin carcass acquisition and meat eating within a larger theoretical context of dietary and technological change and can be used to make testable predictions about the zooarchaological record and taphonomic phenomenon. The chapter concludes by identifying future directions for zooarchaeological and taphonomic research, specifically, the use of theoretical models to evaluate different potential carcass acquisition strategies and reexamine of some of the key inferential premises that underlie and assign importance to different carcass acquisition strategies.

Unpacking the hunting-versus-scavenging debate

From Dart's (1953) vision of savage and cannibalistic killer apes to Washburn and Lancaster's (1968) view based on ethnographic analogy and promoted at the "Man the Hunter" conference, hunting was traditionally identified as one of the pivotal differences that separated ancestral hominins from other anthropoids (Ardrey 1977; Washburn 1957). Meat acquisition by hunting and occasionally scavenging was one of the foundations of Glynn Isaac's (1971, 1978) influential home base model, which emphasized foraging from central locations, hunting, and a sexual division of labor. Isaac's (1978) model was built, in part, on the differences between humans and nonhuman primates and assumed that specific behavioral features were functionally linked and formed a novel adaptive complex.[1] For instance, bipedalism, tool use, hunting, food transport, the use of central locations, meat sharing, and pair bonding were features of a novel and integrated adaptive complex. This important inferential device continues to permeate most recent models of hominin carcass acquisition strategies.

Archaeological evidence from "living floors" at Olduvai Gorge (FLK 22 *Zinjanthropus*, Bed I) and the presence of kill-sites and home bases (Type B and C sites, respectively) at Koobi Fora seemed to support a hunting scenario. Additional support came from Bunn's (1981, 1982, 1986) zooarchaeological analyses of FLK 22 and FLK N1–2, which lead him to conclude that early hominins systematically butchered meat-bearing elements from smaller-sized carcasses acquired by hunting, and from medium- to larger-sized prey taken by confrontational scavenging (Bunn and Kroll 1986; Bunn 2007). Sites such as FLK 22 represented *secondary butchery sites*, where parts were transported, consumed, abandoned, and the refuse ravaged by nonhuman scavengers (Bunn and Kroll 1986; Leakey 1971). Hunting was viewed as an outgrowth of small-game acquisition in the last common ancestor, and consuming large quantities of meat was an adaptation to climate-driven changes in the environment and the spread of savanna habitats.

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[1] This rational is best expressed by Isaac (1978): "I advance the argument that we ought to think of an inseparable set of distinctive behaviors which reinforce each other and which were jointly intensified through the action of natural selection without any one factor being an isolated prime mover."

Despite the appeal of the hunting hypothesis, serious questions concerning the origin of paleozooarchaeological assemblages and the timing and sequence of hominin access to carcasses remained. Several features of the paleoarchaeological record were seemingly discordant with the hunting hypotheses. For instance, the archeofauna from FLK 22 and many other Plio-Pleistocene sites were taphonomically complex, leading some to question the range and sequence of taphonomic processes that influenced the assemblages (but see Binford 1981; Potts 1988). Still other bone assemblages associated with cave sites and containing Australopithecine remains, such as Swartskrans and Makapansgat, displayed carnivore damage and were at least partially accumulated by nonhuman predators that also targeted Australopithecines (Binford and Bertram 1977; Brain 1969, 1981; Pickering, et al. 2004; Vrba 1974; Washburn 1957). Because lithic technology such as Oldowan and early Acheulean tools was simple and did not include any recognizable hunting implements capable of dispatching big game (Bingham 2000; Isaac 1987; Whittaker and McCall 2001), the carcass acquisition abilities of early hominins remained open to question.

Although some viewed stealing prey from other predators as a transitional step toward hunting used by Australopithecines or supplementary strategy used with hunting by *Homo* sp. (Howell 1970, 1965; Leakey 1967; Read-Martin and Read 1975), the scavenging niche was not systematically defined until the late 1970s and early 1980s. The most significant work came from Binford (1981), who used ethnoarchaeological observations of modern Nunamiut Eskimos and actualistic studies of nonhuman carnivores as backdrops for reevaluating faunal remains from Olduvai Gorge, Klaisies River Mouth, and Lower and Middle Paleolithic age sites in Europe (Binford 1981, 1984, 1985, 1988; but see Lupo 1994). Prey anatomical part representation dominated by heads and feet ran counter to the part selection that typified modern hunters and compared favorably to parts abandoned by predators at kill sites. In addition to the carnivore damage displayed by some bones, the placement and positioning of tool marks suggested the removal of remnant meat scraps, stiff carcass butchery, and the exploitation of limb bone marrow. Binford envisioned early hominids as obligate, marginal scavengers who exploited the largely defleshed limb bones of carcasses abandoned by bone-crunching predators to obtain small amounts of marrow and tissue. He extended this model to the Lower and Middle Paleolithic in Europe and Africa and argued that hunting (as displayed by contemporary hunter-gatherers) probably did not emerge until after the spread of anatomically modern humans. The small quantities of food obtained by scavenging undercut and challenged the idea of sharing, provisioning, or transport to central places. Parts of Binford's hypothesis gained further steam from several different sources. Potts and Shipman's (1981; Shipman 1986) analysis of fossil bones from Olduvai Gorge Beds I and II, for instance, identified a handful of bone specimens that displayed carnivore tooth marks overprinted by slicing marks, which many viewed as incontrovertible proof of the scavenging hypothesis.[2] As Pott (1984, 1988) pointed out, however, scavenging did not preclude the use of central places and transport of resources to safe locations for processing and consumption to avoid the possible threat from predators.

Blumenschine's (1986b, 1986a) pioneering actualistic work in the Serengeti further refined and expanded the passive scavenging niche by identifying medium- and large-sized carcasses abandoned by terrestrial felids in wooded and riparian habitats as predictable and largely (but not completely) uncontested resources for scavenging hominins (Behrensmeyer 2007; Blumenschine and Marean 1993; Tappen 1995). Other scavenging opportunities included smaller-sized prey cached in trees by scansorial felids (Cavallo and Blumenschine 1989), animals that had died of natural causes (e.g., disease, starvation, and exposure), or that were killed by accidents (Selvaggio 1998b). This modification of the scavenging niche allowed for the acquisition of intact carcasses as well as partially defleshed body parts. Carcass parts were transported a short distance to more secure or shady locations, processed, the tissues consumed, the bones discarded, and the residue subsequently ravaged by nonhuman bone-crunching predators (Peters and Blumenschine 1995). Fat-rich tissues associated with scavenged carcass parts, such as brains and marrow, were highly valued resources, especially during the dry season when other food resources were limited (Speth 1989). Hunting and confrontational scavenging were characterized as plausible but unsupported alternatives because both strategies required hunting technology or weaponry to displace predators and/or cooperation by a group (Blumenschine and Pobiner 2007). Taphonomically, this scenario called for the complex sequential impact of three different agents.

By the early to mid-1980s, four scenarios for early hominin carcass acquisition strategies had emerged, and some are still actively debated today. The strategies include (1) the obligate marginal scavenger (*sensu* Binford 1981),

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[2] I suggested that such overprinting could also be caused when there was a delay between when the animal dies and the carcass acquired by hunters. Among contemporary hunters it is not uncommon for wounded prey to wander and lie dead for many hours before tracking parties find the carcass. It is also not uncommon for other carnivorous interested parties to take advantage of this delay and scavenge hunted prey before it can be butchered (see Lupo 1994).

(2) the passive scavenger focused largely (but not entirely) on the remains of felid kills (Blumenschine 1986), (3) the confrontational (also known as power or aggressive) scavenger that involved dislodging predators from kills of large animals to obtain fleshed carcasses, and (4) the active hunter. These do not necessarily represent mutually exclusive strategies and many researchers agree that early hominins were probably capable of dispatching and/or collecting smaller-sized prey (Roche et al. 1999). Important variations of these models were applied to different hominins and chronological contexts. Stiner (1994), for instance, proposed that a flexible hunting and scavenging pattern based on the availability of other crucial resources characterized foraging among Neanderthal populations. Klein and colleagues (Klein and Cruz-Uribe 1996; Klein and Cruz-Uribe 2000) argue that Middle Stone Age hominins in South Africa were limited by simple hunting technology and targeted low-risk prey prior to 40,000 to 50,000 years BP. After this date, evolutionary shift involving brain reorganization, enhanced cognition, and the emergence of languages also gave rise to the use of more sophisticated and effective hunting technology that allowed hominins to hunt a different range of more dangerous prey (Klein 1995; 2000).

Much of the current debate still concerns the interpretation of zooarchaeological evidence from Bed I sites at Oldvuai Gorge, FLK 22 (Zinj) and FLKN 1–2, and Koobi Fora (FxJj50). Blumenschine and colleagues marshal considerable taphonomic evidence to support the view that the evidence from FLK 22 is consistent with their original proposition (i.e., felids-hominins-carnivores). Evidence from FLKN 1–2 reflects variation on this theme, with evidence that hominins scavenged more intact carcasses from other opportunities (Blumenschine et al. 2007a, 2007b). The results from both of these analyses fit comfortably within the passive scavenging niche as originally defined by Blumenschine (1987, 1986a, 1991). Domínguez-Rodrigo (1997, 1999b; Domínguez-Rodrigo, et al. 2007) and colleagues maintain that the evidence from FLK 22 and FxJj50 supports the acquisition of fully fleshed carcasses by hunting, confrontational scavenging, or scavenging mass natural death sites. Reanalyses of several Olduvai Bed I and II archeofaunas, including FLKN 1–2, suggest that hominins had very little to do with the formation of the assemblages (Domínguez-Rodrigo, et al. 2007; Egeland and Domínguez-Rodrigo 2008; Monahan 1996).

Beyond these ongoing assemblage specific debates, questions concerning the longitudinal evolution of carcass acquisition strategies and associated technology, and the wider behavioral, political, and social implications of meat eating remain largely unresolved. Clear attempts to build an evolutionary trajectory from the aggregate analyses of the material record are ongoing. For example, Bunn (2007) proposes three temporal phases of carcass acquisition and use for early hominins. From 2.6 to 2.5 MYA, hominins using simple flaked stone tools butchered complete animals at the place of procurement, investing minimal effort in meat acquisition. Between 2.3 and 1.9 MYA, evidence from West Turkana and Koobi Fora indicates transport, delayed processing, and consumption of carcasses, and a higher investment in meat acquisition by hunting and confrontational scavenging. After 1.7 to 1.5 MYA, there is evidence for habitual hunting (also see Monahan 1996).

Despite the differences among researchers and models, there are some general points of agreement. Bipedalism is viewed as an important baseline preadaptation among Australopithecines or their immediate precursors for limb differentiation that ultimately gave rise to tool use and the ability to transport resources to consumption locations. Most identify a dietary shift beginning with the appearance of early Homo (*Homo habilis* and *H. rudophensis*) by 2.33 (Kimbel 1996) or 2.4 MYA (Sherwood et al. 2002) or possibly earlier (Dominy et al. 2008; Laden and Wrangham 2005). The appearance of early Homo coincides with an interval marked by global cooling and drying and a spread of C4 grasslands approximately 2.5 MYA (Vrba 1995), which is widely viewed as a *push factor*, or impetus for evolutionary change. It is during this interval that the earliest known tools and cut-marked animal bones also appear (e.g., de Heinzelin et al. 1999; Domínguez-Rodrigo et al. 2005; Semaw et al. 1997, 2003). Dietary change set in motion with early Homo might have been further amplified in highly mobile *Homo erectus/ergaster* populations. The significant increase in body size and cranial capacity, and the emergence of modern gut morphology in *Homo erectus/ergaster* had a catalytic effect on prolonged offspring dependency and the reproductive costs incurred by females (Aiello and Key 2002; Aiello and Wheeler 1995; but see Helladic and Parquet 2002). All of these anatomical changes and the advent of new technology are viewed as indicators of a shift to a high-quality diet that might have focused on meat or some other resource, and/or the advent of cooking technology (Dominy et al. 2008; O'Connell et al. 1999; Wrangham et al. 1999).

Zooarchaeological and taphonomic evidence

Within the last decade, paleoarchaeological research steadily has pushed back the date for the earliest evidence of hunting, showing that it predates the emergence and spread of anatomically modern humans (contra Binford 1985). Cumulative evidence from different sources including tools and animal bones from archaeological sites (Churchill 1998; Kuhn and Stiner

2006; Schmitt et al. 2003; Speth and Tchenov 1998; Stiner 2002; Villa et al. 2009), isotopic dietary studies (Richards and Schmitz 2008, Richards et al. 2000, 2008), and osteological analysis (Berger and Trinkaus 1995) show that Middle Paleolithic Neanderthal populations were "top-level carnivores" and consistently ate the meat of herbivores. Reconsideration of the carcass acquisition strategies of Plio-Pleistocene and early Pleistocene hominins is less clear-cut and is still hindered by the small number of well-preserved sites (but see de Heinzelin et al. 1999; Domínguez-Rodrigo et al. 2005; Norton and Gao 2008; Pobiner et al. 2008; Stiner et al. 2009; Rabinovich et al. 2008; Villa et al. 2005; Yeshurun et al. 2007)

Although zooarchaeological evidence continues to play a central role in these analyses, some assemblage level attributes such as taxonomic composition, age profiles, and anatomical part representation (e.g., Binford 1981, 1984; Vrba 1975) are strongly influenced by methodological biases and taphonomic processes and can be unreliable indicators of assemblage origin or formation history (see Binford 1984; Blumenschine and Pobiner 2007:169; Lam and Pearson 2005; Marean and Assefa 1999). Skeletal part representation is an especially illustrative example. Several studies show that carnivore ravaging can destroy and alter bone assemblages in more or less predictable ways, producing the now well-known effect of *density-mediated survivorship* (e.g., Brain 1981; Lyman 1994; Marean et al. 1992). Marean and colleagues (e.g., Bartram and Marean 1999; Marean 1998; Marean et al. 1992; Marean and Assefa 1999) argue that the combined effects of carnivore ravaging, biased excavation, collection and curation procedures, and analyst methodology can produce spurious skeletal part profiles that are similar to those associated with scavenging (i.e., dominated by heads and feet). The potential for bias and ambiguity in assemblage level attributes has led some to nominate taphonomic evidence as a more reliable indicator of assemblage formation processes (Domínguez-Rodrigo et al. 2007). As recently stated by Blumenschine and Pobiner (2007:171) "marks on bone surfaces provide a reliable inventory of many of the biotic processes that influenced a zooarchaeological assemblage including those potentially interacting with hominins over carcass foods." Thus, bone damage attributes play a central role in identifying the impacts of multiple taphonomic agents and systematic butchery in the paleoarchaeological record.[3]

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[3] A different line of taphonomic evidence, lithic damage on stone tools produced when projectile weaponry pierces the flesh and hits the bone, is used by Villa et al. (2009; Mussi and Villa 2008) to identify hunting. These important data are also generated by actualistic research but are not discussed here.

Comparative taphonomic data are generated by a variety of sources, but actualistic research, especially experimental and replicative studies, play a central role in the analysis of early hominin assemblages. These studies are used to model the outcome of different potential acquisition and processing scenarios, the timing and sequence of carcass access by hominins and nonhuman carnivores, and the intensity of processing. To apply these data successfully, however, researchers must demonstrate (1) that certain kinds of damage attributes or characteristics are unambiguous indicators of specific activities or agents (so-called taphonomic signatures); (2) the processes/behavior that created the attribute in the present were identically or predictively operative in the past; and (3) a clear match between the actualistic evidence and patterns in the archaeological record (Lupo and O'Connell 2002). These three criteria can be difficult to operationalize because variability from many intrinsic and extrinsic factors can influence the outcome in each step of the analysis. Intrinsic factors are those arising from the design, execution, and analysis of the experiment or archaeological assemblage.[4] Extrinsic factors are those influencing the behavior or mechanics of the taphonomic agent or process under investigation. Extrinsic factors can include prey density and mobility, predator group size, seasonal fluctuations, prey and predator state, and ecological context and can result in behavioral variability in a carnivore's feeding strategy and influence the intensity and range of bone damage inflicted by ravaging carnivores (see Galán et al. 2009; Lansing et al. 2009; Lam 1992; Lupo 1995).

Although zooarchaeologists have been aware of variability in actualistic outcome for a long time (see Lyman 1994), the idea of a taphonomic signature presumes that all sources of variability that influence the process or agent (and the resulting damage attribute) are known and/or can be experimentally controlled. Even as the number of well-controlled taphonomic experiments increases, researchers should assume that they still have only a limited grasp on the range of variability influencing some attributes such as tooth marks, cut marks, and percussion marks (see Lupo and O'Connell 2002). Considering that the range of purported Plio-Pleistocene carnivores that coexisted and interacted with early hominins includes lions, cheetahs, leopards, ancestral hyenas, saber-toothed cats (*Homotherium*, *Megantereon*, and *Dinofelis*), *Lycaon sekowei*, and *Chasmaporthetes*, as well as scavenging birds and carnivorous reptiles, such as *Crocodylus lloydi*, the variability

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[4] Eck (2007), for example, discusses very subtle intrinsic factors that can influence data recovery in an archaeological survey and excavation, such as the length of time spent surveying, number of surveyors, and area excavated.

resulting from these factors is potentially very high (Hartstone-Rose et al. 2010; Lewis 1997, Lewis and Werdelin 2007; Njau and Blumenschine 2006), and analysts can expect an as yet largely undefined variability in bone assemblage attributes, including damage patterns, generated by these predators (e.g., Lam 1992; Marean and Erhardt 1995; Marra et al. 2004).

The unambiguous criterion is also difficult to demonstrate because it implies that equifinality from every other potential taphonomic agent can be ruled out. In most cases, researchers can eliminate only those taphonomic processes that they think probably influenced the assemblage. This approach simplifies the scope of the analysis but constitutes an a priori limitation on the range of processes and potential taphonomic outcomes considered by the researcher based on a preconceived sequence of events (see Egeland et al. 2004 for a similar point). Even when taphonomic phenomena are well-defined, there is a great deal of variability in how different researchers identify and quantify different attributes, and applications of these data to the archaeological record can create inconsistencies in results (see Lupo and O'Connell 2002). Recent contrasting viewpoints on the sequence of processes influencing large mammal remains at FLK 22, based on taphonomic data, demonstrate the problem (Blumenschine and Pobiner 2007). Truly unambiguous taphonomic patterning should consistently yield the same or nearly the same inference, irrespective of the analyst or research group.

Impacts from multiple agents

Early recognition that FLK 22 and some other Plio-Pleistocene sites were taphonomically complex led Blumenschine and colleagues to (1986b, 1988, 1995; Blumenschine and Marean 1993; Blumenschine et al. 1994; Capaldo 1997, 1998; Selvaggio 1994, 1998) to consider how the sequential impact of different agents influenced taphonomic patterns. Up until that time, many (but not all) taphonomic studies focused on the impact of single actors on bone assemblages. A series of experiments in which bovid limb bones in various conditions (i.e., defleshed and marrow removed, or intact) were exposed to nonhuman predators simulated three hypothetical acquisition scenarios: hammerstone only (human first), carnivore only, and hammerstone followed by carnivore ravaging. Cumulatively, these experiments showed that carnivores selectively ravage parts based on the amount and types of nutrients associated with the part when it is discarded, and survivorship largely depended on bone density and size (Binford et al. 1988; Marean et al. 1992). When carnivores ravaged simulated assemblages, in which defleshed limb bones were broken with hammerstones and the marrow removed, epiphyses and near-epiphyses portions showed a higher incidence of tooth marks (or were consumed) compared with midshaft segments associated with denser, cortical bone. The latter are less attractive to ravaging carnivores after the marrow is removed from defleshed bones. Comparison of these experimental data to FLK 22, Bed I showed that the presence of percussion marks on limb bones, indicating hominin involvement in the formation of the assemblages, but tooth mark frequencies on limb midshafts were intermediate to values obtained in carnivore only and hammerstone followed by carnivore simulations (Blumenschine 1995). Blumenschine (1995) concluded that this reflected damage inflicted by carnivores (i.e., felids) before hominins acquired and processed the bones. According to Blumenschine, "The 'extra' tooth marks on the long bone midshafts therefore appear to have been inflicted prior to diaphyseal fragmentation and marrow extraction by hominids" (1995:42).

Subsequent experiments by Selvaggio (1994, 1998) modeled tooth marks and butchery damage for a complex three-agent scenario involving sequential taphonomic processes (felid-hominin-carnivore). She predicted that when hominins had first or early access to carcasses, butchery marks would occur on bones that lacked evidence of carnivore modification, and when hominins followed carnivores, cut marks would co-occur with carnivore tooth marks and damage. In her analysis, the incidence of tooth and butchery marks on large mammals from FLK 22 compared favorably to the felid-hominin-carnivore scenario. Thus far, the three-part model has been applied only to sites in Olduvai Gorge (Blumenschine and Pobiner 2007), but dual-patterning models (i.e., humans followed by carnivores) are widely applied to the analyses of archaeological sites in many different contexts and are often used in conjunction with skeletal part profiles, indicating density-mediated attrition.

One assumption underlying multiple agent models (dual or triple patterning) is that the intensity, range, and sequence of different agents that impacted an assemblage can be confidently inferred by tooth mark attributes, such as mark dimensions, distribution, and frequency. There is disagreement among researchers concerning the reliability and precision of some of these measures, however.[5] Selvaggio and Wilder's (2001) study of tooth marks used actualistic marks created by contemporary African carnivores and latex impressions of the dentition of extinct Plio-Pleistocene

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[5] Additional issues concerning the misidentification of different taphonomic processes that influenced particular assemblages are case specific and are not discussed here (see Domínguez-Rodrigo et al. 2006, 2007; but also Blumenschine et al. 2007).

predators. This study showed that tooth area (length and breadth) varied as a function of bone density. Less durable bone portions such as the metaphyses and cancellous bone displayed larger tooth pits than cortical diaphyseal bone, creating spurious overlap in the pit size among different animals. Mark morphology distinguished bone-crushing specialists from flesh eaters but also showed overlap between the groups in single-dimension measurements. In a separate study, Domínguez-Rodrigo and Piqueras (2003) found pits on cancellous bone to be reliable indicators of carnivore type, whereas pits on cortical bone could only differentiate small and medium from large-sized carnivores. More recently, Delaney-Rivera et al. (2009) examined tooth marks inflicted on bones by sixteen different omnivores and carnivores ranging in size from 1.36 to 159 kg in body weight and found overlap in pit dimensions as a function of body mass and taxonomic affiliation. Human tooth marks, for example, overlapped in pit size with medium- and small-sized predators (see Landt 2007 for the original data). Their study concluded that whereas tooth marks on the epiphyses and metaphyses correlated with animal body mass, those on the diaphyses showed the weakest relationship to body mass. They argue that inferences regarding animal size based on tooth mark dimensions on limb bone diaphyses are not supported, thus undercutting the value of these data in the multiple agent models. Delaney-Rivera et al. (2009) also point out the possibility that tooth marks were created by hominins (or smaller-sized carnivores) is often downplayed (but see Oliver 1994; Landt 2007) or ignored by analysts, but remains a plausible alternative (but see Pobiner et al. 2007; Pickering and Wallis 1997 for recent work involving chimpanzee modifications).

Variability in tooth mark frequencies and distributions are also documented. Domínguez-Rodrigo (1997, 1999a, 1999b; Domínguez-Rodrigo et al. 2007) reported circumstances in which carnivores created high frequencies of midshaft tooth marks on bones processed and discarded by humans and exposed to secondary consumers for prolonged periods, and found that large felids (lions) created very few tooth marks on midshaft bone segments during initial defleshing (contra Blumenschine 1995). A synthetic analysis, comparing existing actualistic results with ethnoarchaeological data from assemblages produced by Hadza hunter-gatherers that were subsequently ravaged, showed a very high range of tooth mark frequencies (between 20% and <95% of the NISP) on midshaft segments in human first assemblages that overlapped with carnivore first assemblages. In general, Hadza assemblages (human first) displayed fewer tooth marks than experimental assemblages where carnivores had first access (Lupo and O'Connell 2002). The relationship between sequence of access and tooth mark frequencies on midshaft segments was not consistently clear-cut among the existing datasets, however.

One difficulty in identifying the sequence of multiple agents is the lack of clear expectations regarding cut-mark frequencies and distribution on fleshed, partially fleshed, and defleshed caracasses (but see Blumenschine 1988; Capaldo 1997; Selvaggio 1994, 1998; Domínguez-Rodrigo 1997). Bunn and Kroll (1986; also see Bunn 2001) asserted that cut marks were the result of unintentional contact between blade and bone, and that butchers, attempting to preserve their tools, only created cut marks when attached flesh obscured the bone's surface. According to this rationale, if hominins obtained defleshed limb bones (as per the scavenging scenario), butchers would be able to see and avoid the exposed bone and any adhering flesh could be easily bitten off. They predicted that the processing of defleshed bones should result in fewer cut marks on midshaft portions than the butchery of fully fleshed body parts. Binford (1984:71, 1986, 1988:131) argued that the frequency of cut marks displayed by bones varied as a function of the effort invested in butchery, not the amount of meat associated with the bone. He argued the removal of small scraps of flesh from scavenged bones resulting in numerous oblique and transverse marks on limb bone shafts. These contrasting viewpoints gave rise to an ongoing controversy concerning the relationship between the placement and frequency of cut marks and the amount of meat attached to the bone at acquisition.

Butchering experiments conducted by Domínguez-Rodrigo (1997, 1999a) on carcass parts acquired in various stages of completeness found that cut-mark frequencies paralleled the weight of meat/flesh on carcasses/body parts at acquisition. Synthetic comparative analysis on the existing data sets, including Hadza assemblages by Lupo and O'Connell (2002), showed an overlap in mean and standard deviation values for cut-mark frequencies displayed by midshaft segments in human and carnivore first assemblages. Although cut-mark frequencies are not consistently predicted by the amount of flesh attached to the bone at acquisition, upper limb bones with more attached flesh generally display more cut marks compared with lower limb bones, and midshaft segments of upper limb bones display more cut marks if the part is acquired intact (also see Lupo 1993). Subsequent experimental work by Pobiner and Braun (2005) found no relationship between the amount of meat attached to a part and the number of cut marks, and Egeland's (2003) experiments found no relationship between processing intensity (as measured by the number of tool strokes) and the weight of the meat attached to the part, and number of cut marks displayed by a bone (also see Braun et al. 2008). Although some of the differences among these studies result from intrinsic factors, cut-mark frequencies recorded in ethnoarchaeological and archaeological contexts show a very high degree of variability as a function of a number of factors including (but not limited to) bone shape, tool type and material, site context, degree of bone fragmentation, carcass size and condition, and desired end product (see Lyman 1994 for a summary; Dewbury and Russell 2007; Lyman 2005).

Systematic butchering

Recent inferences of early carcass acquisition from hunting or confrontational scavenging are based on cut marks reflecting systematic butchering and assemblage characteristics such as the intentional harvesting of prime-age adults indicative of cooperative hunting (Stiner et al. 2009). According to Bunn and Kroll (1986), systematic butchering is reflected by cut marks indicating that carcasses were skinned, dismembered, and defleshed efficiently. If a carcass has been systematically butchered, cut marks relating to specific activities, such as the removal of meat, should be located at key anatomical locations (especially on high meat-bearing elements). For instance, de Heinzelin et al. (1999) describe some of the earliest known cut-marked bones from the Hata Member of Bouri Formation (Ethiopia) dating to approximately 2.5 MYA.[6] The cut marks from this site include incisions possibly resulting from the removal of the tongue on the mandible of a medium-sized alcelaphine; cuts, chops, and hammerstone marks on a large bovid tibia midshaft; and dismemberment and fillet marks on a Hipparion (three-toed horse) femur. At Gona, a nearly contemporaneous site complex (2.58 to 2.1 MYA) located some 96 km from Bouri, Domínguez-Rodrigo et al. (2005) report cut-marked specimens that include an equid calcaneous with a skinning mark, midshaft segments of upper limbs (and high meat-bearing parts), and a rib portion with a mark on the ventral surface resulting from evisceration. Significantly more evidence for the acquisition and processing of largely intact carcasses comes from Middle Pleistocene sites. For example, Pobiner et al. (2008) describe butchery damage (cut and percussion marks) displayed by animals in zooarchaeological assemblages from three sites in Koobi Fora (FwJj14A, FwJj14B, and GaJi14) dating to approximately 1.5 MYA. Rabinovich et al. (2008) document systematic butchery of fallow deer (*Dama* sp.) at Gesher Benot Ya'aqov, an Acheulean

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[6] The recent discovery of tool-marked bones from the Sidi Hakoma member of the Hadar Formation in Dikika, Ethiopia, could pre-date 3.39 MYA (McPheron et al. 2010), but these marks are highly controversial (Domínguez-Rodrigo et al. 2010).

age site in Israel, and Stiner et al. (2009) recently identified cooperative hunting of large game based on systematic butchery and the presence of prime-age adult prey at Qesem Cave, Israel (400 and 200 ka).

The underlying assumptions that guide the inferences of systematic butchery are that (1) there is a predictable relationship between the placement and frequency of cut marks and the amount and kinds of tissue attached to the bone at acquisition; (2) there is a reasonable association between mark location and function; and (3) complete carcass processing will more or less follow a predictable sequence of actions, from evisceration and skinning to filleting and dismemberment, that conforms to a butchery pattern (see Lyman 1994, 2005). As discussed earlier, there is no widespread consensus concerning the first assumption, at least as it relates to the distribution of meat. The second assumption generally is supported by ethnoarchaeological observations of contemporary hunter-gatherers (see Binford's 1981 seminal work) and butchering experiments (e.g., Nilssen 2000). Most analysts realize, however, that there is no necessary one-to-one relationship between mark location and function, and some marks are multipurpose (Lyman 1994). Even so it seems unlikely that all marks could serve multiple functions. Skinning and dismemberment marks, for example, tend to occur in fairly predictable locations. The third assumption is not problematic, but the converse rationale can be misleading. Does the absence of evidence for systematic butchering mean that carcasses were acquired in a defleshed condition or late acquisition? Systematic butchery implies a consistent sequence of butchery actions, yet ethnoarchaeological observations show that carcass butchery does not always follow a set sequence of actions, and the bones do not always display cut marks that conform to systematic butchery or a butchery pattern. Lyman recently (2005) demonstrated this point in a comparison of two different, but culturally and chronologically similar archaeological sites. In his study, cut-mark frequencies and distribution across skeletal elements lacked consistency even among animal remains within the same taxon and size class. Lyman (2005:1730) concluded, "The most pressing problem now . . . is one of determining why tremendous ranges of variation exist in the frequency of striae . . . if we cannot explain that variation then I suggest no amount of reliable identification of cutmarks or new methods of tallying cutmarks will help us." Similar results were found in an intrataxonomic comparison of cut marks across skeletal elements in bone assemblages created by Hadza hunter-gatherers (Lupo 1993). In this analysis, all of these carcasses were acquired by hunting and fully fleshed at acquisition, but the bones displayed a striking lack of consistency in the frequency and distribution marks. These results suggest that many cases of early carcass access might go unrecorded because the bones do not display evidence of systematic butchery.

Variation in field and consumptive processing also could influence and mask manifestations of systematic butchery. Among contemporary hunter-gatherers, prey or whole carcasses are often roasted intact with very little to no preprocessing (i.e., skinning). Hadza hunter-gatherers, for instance, routinely transported and processed whole zebra limb segments and the cranium with the skin still attached to the part. Similarly, among contemporary Bofi and Aka forest foragers in Central Africa, the skin is often left attached to limb segments of medium- and larger-sized prey (>25 kg), and most smaller-sized animals are roasted whole (Fancher 2009). Among historic and ethnographic hunter-gatherers, larger-sized animals were often roasted whole in pits or simple earthen ovens (see Wandsnider's 1997 review). Thermal processing causes meat to easily separate from the bone (in some cases it falls off), thus obviating the need for extensive cutting. This kind of processing results in very little to no burning on the bone's surface, and few if any cut marks. If, as suggested by Wrangham et al. (1999), early *Homo* sp. used fire, then thermal processing of this kind represents a plausible carcass-processing activity.

It should be clear from the previous discussion that analysts can no longer assume that taphonomic attributes are any more reliable or less ambiguous signatures of specific processes than some assemblage-level attributes (see Lupo and O'Connell 2002). This fact does not diminish the value of taphonomic phenomena, and additional research on variability in taphonomic traces is clearly warranted. Even as taphonomic analyses become more fine-grained, however, and the number of studies increases, analysts are still unsure how to consider variability in taphonomic attributes fruitfully and productively. This underscores the importance of using top-down approaches as a guide to developing predictions about the zooarchaeological and taphonomic record.

Life-history profiles, meat acquisition, and dietary change

Several recent models concerning the emergence of meat eating place special emphases on the unique life-history profile that separates modern humans from other hominoids. Life-history theory (LHT) is the study of how energy is allocated toward somatic growth, physical maintenance, reproduction, and raising offspring (e.g., Hill and Kaplan 1999; Smith and Tompkins 1995). These unique features include helpless offspring with long periods of dependency, a juvenile growth spurt, and prolonged and postreproductive lifespan. More recently, some researchers include less hard-wired behavioral traits as part of the human life-history profile. These include male provisioning of offspring and mates, complex systems of communication, reliance on social learning, technologically assisted lifestyles, and a high degree of cooperation (Hill et al. 2009; Kaplan et al. 2000).

Questions raised by these models concern when these unique life-history traits emerged and whether these traits appear as a functional suite (e.g., Robson and Wood 2008). One key assumption that underlies these (and other) models is that the appearance of *Homo* sp. (*Homo erectus/ergaster*) in tandem with increased body size and cranial capacity are markers for relatively helpless young having prolonged juvenile dependency. The predictable acquisition of meat by hunting or confrontational scavenging either played a supplementary role (O'Connell et al. 2002) or directly supported the derivation of these features (Kaplan et al. 2000). Inferring the life-history profiles of extinct hominins presents a central difficulty for these models. Although general life-history parameters of extinct hominins can be inferred from body mass, brain growth and dental development (Dean 2006; Ramirez-Rozzi and Bermúdez de Castro 2004; but see Robson and Wood 2008), the emergence of longevity and the prolonged postreproductive life span remains highly contentious (e.g., Hawkes et al. 1997, 1998; Kennedy 2003; Martin 2007). Caspari and Lee (2004), for example, suggest that longevity (as measured by the ratio of older to younger adult skeletal remains) does not significantly increase until the early Upper Paleolithic (but see Bello et al. 2006). Nevertheless, these models provide very compelling reconstructions and bring together different lines of inquiry that clearly reach beyond a specific artifact class, damage attribute, or site.

Big-game acquisition as a competitive display

O'Connell and colleagues (1999, 2002; Hawkes et al. 1991, 1997, 1998) turn conventional arguments upside down by arguing that female foraging and food sharing, not meat provisioning and acquisition, drove many of the physiological and anatomical changes associated with *Homo* sp. According to this view, aggressive scavenging emerged as a costly signal or competitive display by males, in tandem with shifts in female foraging and food-sharing patterns. Climate-driven reductions in foods that juvenile creatures could handle on their own beginning approximately 2 to 2.5 MYA initiated a dietary expansion involving a change in foraging range and broadening of the diet with an emphasis on resources that were difficult to acquire, but predictable and had a high return. In this model, the diet breadth expands and costs for searching out new resources increases. This in turn fuels technological innovations designed to reduce the handling costs of resources. In this model, adult females provision their offspring by collecting tubers (so-called underground storage organs [USOs]), which are viewed by some as fallback resources (see Dominy et al. 2008). As provisioning by females and a heavy dietary reliance on tubers is established, older females assisted junior kin, favoring the survivorship of grandchildren and giving rise to a slow-paced life-history profile that underwrites increased life spans, decreased juvenile mortality, larger body sizes, and higher population densities (O'Connell et al. 2002; Hawkes et al. 1997, 1998). These characteristics, especially larger body size and increased sociality, gave *Homo erectus/ergaster* a competitive advantage in aggressively scavenging prey from other Plio-Pleistocene predators. The amount and rate of meat acquired from aggressive scavenging was highly unpredictable, however, and did not support transporting prey body segments to central places or provisions for offspring.

This scenario is based on research that integrates rationale derived from human behavioral ecology with ethnographic observations of contemporary hunter-gatherers. Observations of Hadza hunter-gatherers show that big-game hunting can yield large quantities of meat, but the daily returns are highly unpredictable, with men having a 97% chance of failure on any given day (Hawkes et al. 1991). Relative to other available hunting opportunities, such as small prey capture, big-game hunting appears to be inefficient. Ethnographic studies among hunter-gatherers in other parts of the world show that men often pursue wasteful and/or seemingly expensive hunting opportunities that are high risk, inefficient, or unpredictable (see Bliege Bird et al. 2001; Lupo and Schmitt 2004; Sosis 2000). Moreover, prohibitions and customs governing the consumption and redistribution of meat often limit the caloric rewards that hunters and their families can garner from these opportunities (e.g., Testart 1987). Hadza big-game hunters, for instance, on average take home only 10% (or less) of the meat they acquire, and their shares are no larger (and sometimes substantially smaller) than those received by others (Hawkes 2000). Thus, among some contemporary hunter-gatherers certain kinds of hunting opportunities do not appear to be aimed at provisioning families with consistent and reliable resources or rate maximization. A related explanation recasts inefficient or high-risk hunting activities as costly signals. *Costly signaling theory* promotes the view that certain kinds of activities are costly because they impose a handicap on the possessor and are impossible to fake. These activities can work as honest signals to others about the signaler's underlying qualities or intentions (Bird et al. 2001; Bliege Bird and Smith 2005). Successful hunters who engage in costly hunting activities might reap nonconsumptive benefits, which can include access to younger and/or harder-working wives, larger networks of allies and/or trading partners, increased prestige, and political deference (e.g., Bliege Bird and Smith 2005; Hawkes 2000; Smith 2004).

According to this view, strategies of male competition involving big-game acquisition were already in place among early *Homo* populations before the emergence of behaviorally modern hunter-gatherers. If the acquisition of big game via aggressive scavenging initially emerged as a competitive display among early hominins, then one might question when and how provisioning (or rate maximization strategies) ultimately emerged? Hawkes (2000; also see O'Connell et al. 2002) argues that competition in mating effort as manifested by big-game acquisition undercut male dominance hierarchies and gave rise to conventions (e.g., generous sharing) that displaced contestation as a means of solving disputes. Males provision and bond with their mates as a form of mate guarding that reduces the costs of male-to-male competition and monopolizes women's fertility (Blurton Jones et al. 1996).

Hunting and embodied capital

Kaplan et al. (2000) argue that a prolonged period of growth and experiential learning were a requisite for a reliance of skill-based subsistence technology in early *Homo*. According to this view, hunting emerged among early *Homo* sp. as a strategy for provisioning helpless offspring with high-quality but technologically demanding resources during prolonged periods of juvenile dependency. Highly variable climate conditions favored an environmental and dietary shift, emphasizing cognitive solutions and intellectually demanding extractive technology (Kaplan et al. 2000; Walker et al. 2002). Natural selection favored experiential-based capital that increased returns from high-skill demanding resources. As juvenile periods became more prolonged, life span increased, shifting higher periods of productivity into later life. The resulting shift in feeding niche increased the encounter rate with large-sized but difficult-to-find resources, including meat. Several consequences of this shift include delayed growth and maturation associated with longer periods of cognitive and intelligence development. A reliance on high-quality resources, such as meat, increased nutritional status and decreased mortality by food sharing among these populations. As longevity increased and population size grew, the selective pressure from predatory animals associated with smaller groups sizes was released.

A more recent version of this model stresses the role of cumulative social learning for the transfer of extrasomatic information and cooperation as central features of uniqueness in modern humans (Hill et al. 2009). In this model, early *Homo* sp. used large home ranges and focused on technologically demanding but high-quality food resources, including hunted prey, by 2 MYA. Provisioning dependent offspring and food sharing among adults was not sufficient enough to decrease adult mortality or increase juvenile dependency, however. The slow maturation pattern that typifies modern life-history profiles emerged well after the shift toward hunting, possibly by the Middle Pleistocene. Additional features such as population growth, reliance on social transmission, and cumulative cultural capacity probably occurred later, with the emergence of anatomically modern humans (Hill et al. 2009).

Both embodied capital models assume that foraging, and especially hunting, are activities that require large amounts of time to gain the skills and experience necessarily to be successful. Among contemporary hunter-gatherers, the foraging activities of children are largely limited by body size and age (Blurton Jones et al. 1997), but most boys can hunt on their own by the time they are adolescents, and most are hunting large prey by their midteen years (MacDonald 2007). Walker et al. (2002) point out that competency in hunting, as measured by caloric returns, is at its highest for most men between the ages of 30 and 50. Men in this age range are generally more skilled at capturing animals, have acquired a large pool of knowledge pertaining to animal behavior, and have higher encounter rates with prey. Even so, some hunting strategies are more technologically complex or demanding of skill than others. For example, the use of slings, stone projectiles, simple traps, and hand collecting is less demanding of skill than other hunting technologies. Depending on how skill- and experienced-based acquisition strategies are defined and measured, there is very little archaeological evidence to support the emergence of skill-demanding extractive technology dating to 2.5 to 2 MYA. Most of the earliest stone tools (2.5 MYA at Gona, Ethopia; Semaw et al. 1997 and 2.33 ± 0.07 Myr in the Hadar, Ethiopia Kimbel et al. 1996), are simple technologies and lack skill-demanding hunting implements. Other kinds of purported high skill–based hunting activities such as cooperative hunting date to approximately 400,000 years ago (Stiner et al. 2009) but could date even earlier if Plio-Pleistocene hominin groups cooperated in the acquisition of large-sized animal carcasses, as many have suggested (e.g., Rose and Marshall 1996; O'Connell et al. 1999). Although cooperative carcass acquisition implies planning, cooperation, and stealth, it apparently does not require long periods of learning or cognitive development, because modern chimpanzees (e.g., Boesch 2001; Stanford 1996) and baboons (see Hausfater 1976 and the references therein; Strum 1983), with more rapid life histories than humans, acquire prey by cooperative hunts. If technological complexity (as measured by tool diversity and specialization) is any measure of skill-based tasks, then evidence for complex carcass acquisition strategies requiring sophisticated technology does not become widespread until after 50,000 years ago. Similarly, evidence for other kinds of experience-based food extraction tasks, such as complicated processing sequences associated with some toxic plant foods, do not appear to predate 50,000 to 40,000 years ago (Barker, et al. 2007).

Where do we go from here? Moving beyond stone and bones

Decades after actualistic and taphonomic studies became important in the field of zooarchaeology, researchers are still debating some of the same carcass acquisition scenarios first posited more than thirty years ago. Equifinality and ambiguity in taphonomic attributes stemming from variation is not new, and neither is a call for theoretically guided research (e.g., Bird and O'Connell 2006). What is new is our ability to combine strong theory with diverse data sets to address expanded sets of questions about subsistence and behavior. I suggest that future zooarchaeological research address variation in taphonomic attributes by combining traditional actualistic inquiry with theoretical models. Traditional approaches based on actualistic research can be used to identify the range and nature of variation associated with different taphonomic phenomenon. The growing number of studies conducted thus far allows analysts to perform more synthetic and comparative studies of specific taphonomic attributes (see Cleghorn and Marean 2007; Lupo and O'Connell's 2002; Pickering 2002). These kinds of analyses can be used to: (1) identify important new sources of variability, and (2) test how significant those sources are likely to be in shaping experimental and inferential outcome. By testing the significance of different sources of variability, analysts can more reliably separate those that should be accounted for from those that are less likely to create ambiguity.

Future zooarchaeological research can go one step further by using theoretical models to (1) identify the circumstances that create high levels of variability in taphonomic attributes, and (2) the kinds of taphonomic damage expected under different dietary scenarios. For instance, several different datasets from paleoenvironmental reconstructions (Ségalen et al. 2007) to isotopic analyses (e.g., Lee-Thorp et al. 2000; Sponheimer et al. 2005) suggest an expansion of the diet breadth for Plio-Pleistocene hominins. Expanding diet breadth is usually associated with increased search costs. If big game were encountered at low densities, then lower-ranked and smaller-sized prey composed the bulk of the animal protein consumed by these populations, and taphonomic evidence of intensive processing should be expected (e.g., Lupo 2007). If the acquisition of big game was not aimed at rate maximization but constituted a competitive display, then the usefulness of cut marks as an indicator of the amount of meat attached to the bone at acquisition is undercut. A similar approach can be used to evaluate different plausible carcass acquisition strategies within a larger context of dietary change. In addition to the models discussed here, several alternatives for hunting without the use of sophisticated projectile weaponry are on the table (e.g., Bingham 2000; Bramble and Leiberman 2004; Carrier 1984; Guthrie 2007; Isaac 1987; Liebenberg 2008; also see Krantz 1968). How do we evaluate these different possibilities? One method might be to compare the energetic costs and benefits of pursuing different plausible strategies (see Lupo 2007 for a review).[7] Some attempts have been made to model the return rate of different carcass acquisition strategies, but most focus on demonstrating whether specific strategies yield adequate energetic returns (e.g., Blumenschine and Madrigal 1993; but see Lupo 1998). A more informative approach is to compare the energetic returns from competing and plausible strategies to evaluate the opportunity costs associated with pursuing different strategies. These models not only allow the comparison of different acquisition strategies but also the evaluation of the trade-offs between the different foraging strategies of men and women (e.g., Zeanah 2004). Research along these lines is already emerging and in its nascent stages (e.g., Steudel-Numbers and Wall-Scheffler 2009), but similar approaches need to be applied to different carcass acquisition strategies as an adjunctive method to be used with paleoarchaoelogical analyses.

This still leaves open the question in the title of this paper: how relevant are some of the existing models of early carcass acquisition? Recently Villa et al. (2005) not only reject Binford's obligate, marginal scavenging model for Lower and Middle Paleolithic sites in Europe and Eurasia, but present a postmortem for scavenging models. They argue that scavenging probably occurred opportunistically (as it does among modern hunter-gatherers), but most meat was acquired by hunting. It is not entirely clear how early hominins obtained medium- and large-sized carcasses before the

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[7] Energy is not the only currency that one could use; other currencies are possible.

appearance of recognizable hunting technology, but it is entirely plausible that all of the conventional scavenging and hunting models discussed here played some role among different early hominin populations at different times. Modern humans and their immediate precursors are characterized by extreme omnivory, however (Ungar et al. 2006). Omnivory is usually favored by natural selection in highly volatile and changeable environments and allows the organism to tolerate a wide range of conditions within and between generations (Wells 2007). Omnivory is generally not associated with specialization in acquisition strategies. Obligate scavengers are specialists who can tolerate only a narrow range of conditions and are selected for when the environment is more stable in the long run (DeVault et al. 2003). This is why scavenging prey is a supplementary carcass acquisition strategy used by most contemporary nonhuman omnivores (DeVault et al. 2003; Hamiton 1973) and human hunter-gatherers (O'Connell et al. 1988a). Thus, researchers investigating Plio-Pleistocene hominins might rephrase the question by asking under which circumstances do we expect specialized rather than generalized strategies to become the dominant mode of carcass acquisition?

If current versions of passive and aggressive scavenging and hunting allow for the acquisition of complete (or nearly so) carcasses, then researchers need to revisit the significance of distinguishing among these acquisition techniques. Much of the significance of different hominin carcass acquisition strategies is elevated in part by a series of inferential steps built on the premise that certain anatomical, physiological, and behavioral characteristics are functionally interrelated (after Isaac 1978). Several models, for example, assert a functional and mutually reinforcing link between increased juvenile dependency, provisioning, a dependable meat supply from big-game hunting, and meat sharing among ancestral hominins (e.g., Kaplan et al. 2000). Although these characteristics may be concordant and serve a mutually supporting role among contemporary human populations, whether a functional and reinforcing relationship always existed among these features is seldom questioned. One might begin by questioning, for example, if there is any demonstrable relationship between carcass acquisition strategies and the proportion and reliability of meat in the diet? The proportion of meat in the diet of contemporary hunter-gatherers appears to be determined by a variety of different environmental factors and is not directly linked to carcass acquisition strategies (e.g., Binford 2001; Keeley 1995; Waguespack 2005). Another example is the long-presumed evolutionary link between increased meat eating and brain development. Some argue for a long history of meat eating because docosahexaenoic acid (DHA), a fatty acid made from long-chain polyunsaturated fatty acid (LCPUFA) precursors found in meat, is the only *n*-3 fatty acid implicated for the functional development of the human brain (Finch 2007; Martinez 1992). Shellfish and fish have higher concentrations of DHA than animal tissue, however, leading some to conclude that these resources were an important part of early hominin diets (Cunnane et al. 1993; but see Langdon 2006). Nutritional constraints on the growth and development of modern humans are informative but should not be confused with selective factors. More recent research suggests that other selective factors, such as reproductive and mating systems, and sociality, may play an especially important role in the encephalization process (Schillaci 2008).

Conclusion

Meat is a calorically dense resource that was (and continues to be) favored by many historic and ethnographic hunter-gatherers. Among all human populations, however, meat is part of an array of resources exploited in conjunction with other kinds of foods. Modern humans are omnivores, and the proportion of meat in the diet is highly variable. In a recent cross-cultural survey involving thirteen hunter-gatherer populations, the percentage of meat in the diet ranged from 26% to 96% (Cordain et al. 2002). In an even larger data set, covering some 390 ethnographic cases, the percentage of meat in the diet varied from less than 5% to 90% (Binford 2001). Given the wide range in meat consumption among modern people, we might imagine a scenario in the past where the proportion of meat in the diet among ancestral hominin populations varied in a similar range or on a continuum over time and across space (also see Ungar et al. 2006).

Similarly, understanding how carcasses were obtained by early hominins remains an important question, especially because most contemporary hunter-gatherers use a wide range of techniques to acquire meat. Researchers clearly need to combine traditional approaches with theoretical tools and paleoarchaeological data from several different and independent material sources (also see Gifford-Gonzalez 1991). Additional actualistic research could help to further identify the range of variability in different taphonomic agents and damage attributes but by itself will not further our understanding of the trade-offs in carcass acquisition strategies made by early hominins. Furthermore, recent results of findings in several different fields of inquiry invites a reexamination of what different carcass acquisition strategies might or might not be telling us about other aspects of hominin lifeways.

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CHAPTER 5

Meat foraging by Pleistocene African hominins: Tracking behavioral evolution beyond baseline inferences of early access to carcasses

Travis Rayne Pickering and Henry T. Bunn

There are many reviews of the evidence and debate over hunting and scavenging by early Pleistocene (formerly, Plio-Pleistocene) hominins from the last 20 years available to the reader (e.g., Bunn, 1991, 2007; Bunn and Ezzo, 1993; Blumenschine et al., 1994; Domínguez-Rodrigo, 2002; Domínguez-Rodrigo and Pickering, 2003; Pickering and Domínguez-Rodrigo, 2006; Blumenschine and Pobiner, 2007). Thus, we briefly summarize our determination that the weight of the relevant available zooarchaeological and taphonomic data indicates that by ~1.8–1.6 Ma (mega annum) hominins were gaining access to the nutritionally and energetically choicest parts of large ungulate carcasses and butchering them for edible meat and marrow. Bunn (2007: 207; see also, Bunn, 1982, 1997; Bunn and Kroll, 1986; Bunn and Ezzo, 1993; Domínguez-Rodrigo et al., 2002, 2007, 2009) summarizes that archaeological sites from this interval, including FLK 22 *Zinjanthropus* and BK (Olduvai Gorge, Tanzania), FxJj 50 (Koobi Fora, Kenya) and the ST Site Complex (Peninj, Tanzania),"[yield large] concentrations of thousands of stone tools and MNI [minimum number of individual estimates], ranging up to four dozen large animals in temporally restricted assemblages. Combined with extensive evidence of butchery for meat and marrow, this demonstrates regular access to mostly intact carcasses and repeated transport of portions to favored, central locations (i.e., intermittent, daytime home bases)."

The strong evidence that underpins these conclusions is zooarchaeological and taphonomic data on ungulate skeletal part profiles and modifications on fossil bone surfaces. Modern wildlife research documented a consistent sequence by which large African carnivores, including prominently hyenids and felids, consume prey animals (e.g., Schaller, 1972; Kruuk, 1972; Blumenschine, 1986; Domínguez-Rodrigo, 1999). Because ungulate faunas from the early Pleistocene sites listed above are dominated by the once meaty upper (humerus and femur) and intermediate (radioulna and tibia) long limb bones, the flesh of which is consumed first by carnivores, and because those same fossil bone specimens exhibit extensive defleshing cut marks, we argue that a predominant pattern of early access to intact carcasses is established beyond reasonable doubt. This in turn means that the passive scavenging hypothesis of early hominin carcass foraging is effectively falsified (see also, Domínguez-Rodrigo and Barba, 2006, 2007). As discussed below, we showed recently that the mortality profiles of ungulate prey from the very assemblages yielding the best and most butchery evidence add a decisive new dimension to the case for early access to meat-laden carcasses by hominins (Bunn and Pickering, 2010a,b).

This is not to suggest that hominins never scavenged passively from abandoned carnivore kills – but only that early access to carcasses is now demonstrated empirically to be the dominant taphonomic signature in Pleistocene zooarchaeological samples from across Africa (see also, e.g., Asfaw et al., 2002; Pickering et al., 2004, 2008; Fiore, 2004). It is also the signature that presents the greatest (and most interesting) challenge at the next inferential level above simply discerning early versus late access to carcasses. This is because an inference of early carcass access prompts consideration of several alternative methods to gain that early access, which probably can be tested using paleoanthropological data. In contrast, once inferred, late access to carcasses is almost by definition synonymous with passive scavenging. How passive scavenging was accomplished is no mystery. There is only one choice: wait until a carnivore abandons its kill and only then move in and scrounge the remaining scraps of edible soft tissues.

Thus, the most interesting question for paleoanthropology about hominin carcass foraging is now: how was early access to carcasses – indicated zooarchaeologically and taphonomically – gained by Pleistocene hominins? The four reasonable choices in answer to this question are: (1) access to whole carcasses from natural deaths caused by factors other than predation; (2) access to whole and/or nearly whole carcasses by aggressive confrontational (or power [Bunn, 1996, 2001]) scavenging; (3) access to whole carcasses by hunting; and (4) access to carcasses through a combination of some or all of these processes. In this chapter, we strive to advance investigation of early hominin diet and subsistence strategies at this next inferential level of inquiry.

Perspectives of the first indications of hominin meat eating

Early hominin meat eating is not demonstrated, in the form of co-occurring butchery-marked animal bones and flaked stone tools derived from fine-grained sedimentary contexts, until 2.6 Ma at Ethiopian archaeological sites from the Gona and Bouri localities (de Heinzelin et al., 1999; Semaw et al., 2003; Domínguez-Rodrigo et al., 2005). The hominin and panin lineages separated at least (and probably earlier than) four million years ago (e.g., Bradley, 2008; Fabre et al., 2009). In our opinion, it is extremely unlikely that the Gona and Bouri data represent the *actual* first occurrences of hominin meat eating, at least (and probably more than) one and a half million years after that split. There is nothing about the morphology or about the inferred behavior of pre-stone tool hominins and those hominins assumed (uncritically) by some researchers to have been materially acultural (e.g., the robust australopithecines) that precludes their regular pursuit and attainment of meat eating opportunities (Pickering, 2006, 2010). In fact, isotopic data for *Australopithecus* suggest strongly that even extremely derived (and assumedly dietarily specialized) species, like *A. robustus*, consumed meat at least occasionally (e.g., Sponheimer et al., 1999; 2005; 2006; van der Merwe et al., 2003; see also discussion in Pickering and Domínguez-Rodrigo, Chapter 6). More marginally relevant is the skeletally based diagnosis of possible brucellosis (a disease contracted through contact with and/or consumption of infected soft tissues of mammals) in an *A. africanus* partial skeleton from Sterkfontein (South Africa; D'Anastasio et al., 2009).

We do *not*, however, think that recent claims of two butchered animal bone fragments from the ~3.4 Ma *A. afarensis* site of Dikika (Ethiopia) (McPherron et al., 2010) are relevant to this summary of potential paleoanthropological indications of hominin meat eating >2.6 Ma. Domínguez-Rodrigo et al. (2010), in response to these claims, concluded that the published evidence does not, in fact, support the identification of bone surface marks on the two fossils in question as unequivocal stone tool butchery damage. We further asserted that any equivocation surrounding butchery claims of this great antiquity (~800 ka older than oldest known butchery marks from Gona, where marked animal bones are derived from fine-grained sediments and in spatial association with hominin-flaked stone tools) should lead to rejection of such claims. The equivocation stems from the facts: (1) that the Dikika fossils derived from a potentially abrasive sedimentary context, and (2) that the Dikika fossils show surface damage that is indistinguishable from that imparted randomly (by trampling and/or other incidental movement) in such deposits. Although it might be possible to use unmodified stones cutting against bone to mimic the morphology of the Dikika bone surface marks (as McPherron et al. speculate was how the Dikika marks were produced),[1] such results would in no way nullify the fact that randomly imparted striae can also mimic the Dikika marks, nor does it change the abrasive sedimentary context of the fossils (Domínguez-Rodrigo et al., 2011). Thus, the null hypothesis of non-anthropogenic origin(s) for the marks remains unfalsified.

Concluding a similar review of the earliest zooarchaeological traces at Gona and Bouri, Bunn (2007: 206) was struck by, in comparison to the rich records of hominin meat eating at more recent sites like FLK 22 *Zinjanthropus*, the minimalist nature of the early samples, opining that the Gona and Bouri butchered bone assemblages "could not be much smaller and still exist." Because the samples are so small, analysts are unable to draw the same kind of inferences about them as are drawn from the larger ~1.8–1.6 Ma samples from FLK 22 *Zinjanthropus* (and other sites mentioned above) of the transport and concentration of animal carcasses at central locations by hominins. Bunn (2007: 206) does qualify his assessment of Gona and Bouri, however, stressing that it "does not imply a marginal interest in meat and marrow [by hominins >1.8 Ma] just a marginal ability to obtain it." Bunn's (2007) long-range perspective is that, given the major biological changes that were evidently being selected for over the course of Pleistocene hominin evolution (e.g., increase in body size, modification of body proportions, greater encephalization), hominin carcass-foraging capability was also probably enhanced over time.

In response, Pickering (2010; see also Pickering and Egeland, 2009) contended that there is a different – perhaps more "generous," but certainly still "honest" – way to interpret the Gona and Bouri data: that is, by focusing on the anatomical patterning of butchery mark distribution in the fossil samples (for reviews of the use of butchery mark anatomical patterning to infer timing of hominin access to carcass resources, see e.g., Bunn [1991]; Domínguez-Rodrigo [2002]; Domínguez-Rodrigo and Pickering [2003]; Pickering and Domínguez-Rodrigo [2006]). Pickering argues that cut marks on meat-bearing ungulate long limb bone (i.e., humeri, radioulnae, femora, tibiae) midshaft sections, on the lingual surface of a bovid mandible, and on the ventral surface of a bovid rib in the Gona and Bouri assemblages (de Heinzelin et al., 1999; Domínguez-Rodrigo et al., 2005) must also indicate early access to carcass resources by hominins at

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[1] In our own experiments, using the humanly unmodified edges of sharp rocks to cut flesh from bones, we were unable to reproduce most of the Dikika bone surface marks in question (Domínguez-Rodrigo et al., 2012).

these sites, just as the identical patterning of butchery marks indicates those same carcass acquisition capabilities at the relevant 1.8–1.6 Ma sites, which are approximately one million years younger than Gona and Bouri.

In broad view, early Pleistocene archaeological samples are rare enough generally, and even rarer in the extreme early Pleistocene (i.e., 2.6–2.0 Ma), the interval within which the Gona and Bouri sites fall (see summaries of early sites and their archaeological contents in Domínguez-Rodrigo et al., 2005; Domínguez-Rodrigo and Martínez-Navarro, 2011). Following Domínguez-Rodrigo (2009), Domínguez-Rodrigo and Martínez-Navarro (2011) reason that "the uncertain depositional history of some of these sites, poor preservation of cortical bone in some sites where stone tools and bones occur in the same space, [a relatively] abundant number of sites [among the small overall total number of 2.6–2.0 Ma sites] in undisturbed or minimally disturbed depositional contexts without faunal remains, and the fact that a total of only 15 cut-marked bones (as complete elements [i.e., estimated minimum number of elements]) have been retrieved, create uncertainties about the functional association of stone tools and bones at many of the earliest sites . . . " In this context, Pickering certainly agrees that it behooves paleoanthropologists to consider that some of these early sites were formed by hominin behaviors "that may have been equally (or maybe even more important) than butchery, including plant processing [with stone tools]" (Domínguez-Rodrigo and Martinez-Navarro, 2011). But, Pickering also agrees that the butchered Gona and Bouri faunas (and possibly that from the ~2.0 Ma Kanjera, Kenya, site [Plummer et al., 1999; Plummer, 2004; Ferraro, 2007]) are exceptions to this uncertainty about site function.

With this perspective, the totality of the 2.6–2.0 Ma archaeological sample is not really that distinct from the expanded African sample spanning the longer interval of 2.6–1.0 Ma. Both periods are dominated by sites of ambiguous function and/or palimpsests composed of the residues of the taphonomic actions of hominins *and* carnivores (and/or other biotic agents and nonbiotic processes). And, for both periods, FLK 22 *Zinjanthropus*, BK, FxJj 50 and the ST Site Complex are the *only* sites where zooarchaeological and taphonomic data support *systematic*, centralized butchering of carcasses by hominins, who obtained those carcasses early through hunting and/or aggressive scavenging (Bunn, 1982, 1997; Bunn et al., 1980; Bunn and Kroll, 1986; Bunn and Ezzo, 1993; Domínguez-Rodrigo et al., 2002, 2007, 2009). Pickering's point here is that the evidence from Gona and Bouri (and other more recent zooarchaeological assemblages, like Swartkrans Cave [South Africa] [e.g., Pickering et al., 2004, 2008]) is, at least on one important level, *functionally* equivalent to that from the larger Tanzanian and Kenyan samples, in that both sets of data indicate early access to animal carcasses by hominins. This long-range view of the total evidence holds that the carcass acquisition capabilities of hominins did not change significantly between 2.6 – 2.0 Ma and 1.8 – 1.0 Ma. In other words, “it might have been the *regularity* with which [hominins] acquired carcasses and not necessarily the *method* of access that differed” between these two intervals (Pickering and Egeland, 2009: 175). By extrapolation, this probably means that hominins were hunters of some sort long before Gona and Bouri (see, Pickering, 2010; Pickering and Domínguez-Rodrigo, Chapter 6).

Lessons from extant hominoids: chimpanzees and human hunter-gatherers

For our hypothesis of hunting by >2.6 Ma hominins – in which extant chimpanzee (*Pan troglodytes*) behavior provides a model of Mio-Pliocene hominins as hand-capture predators of small animals, who then processed carcasses manually and/or orally for consumption – we refer readers to Pickering and Domínguez-Rodrigo (Chapter 6). Of special relevance from that chapter to this chapter is the contention that that kind of hunting might have been assisted by the use of simple wooden spears. Pickering and Domínguez-Rodrigo refer to the important work of Jill Pruetz and Paco Bertolani (2007), who reported on spear hunting by a population of savanna chimpanzees at Fongoli (Senegal). Using their teeth to sharpen the ends of sticks into points, the Fongoli chimpanzees fashion what are essentially small thrusting spears, the longest of which are only a couple of feet in length. The chimpanzees poke the spears into hollows in trees in efforts to stab and extract galagos (*Galago galago*), small nocturnal primates who sleep in the holes.

Some specialists downplay the importance of these observations, arguing that galagos are small and essentially defenseless, and so their capture by (predominantly female and juvenile) chimpanzees should be considered gathering, rather than hunting. From this point-of-view, the Fongoli behaviors are equated with other types of extractive foraging, like digging up the underground storage organs of plants or probing open a termite mound. True, there is a measure of mechanical equivalency to all these actions, but the radically divergent payoff and risks of the different resources involved reveals that that equivalence is of lesser importance in the broader view.

Demoting the Fongoli behavior to mere gathering or extractive foraging obfuscates the essential – and wholly disparate – characteristics of animal versus plant prey. Unlike a plant, which is sessile and *can* be *truly* gathered without retaliation against its gatherer (poisonous and thorny plants excepted, of course), most animals are likely to give some fight for their lives if unable to flee and unintentionally afforded the opportunity by their hunters. Chimpanzees, like those at Gombe (Tanzania), who hand-hunt monkeys, face real peril in doing so; monkeys are large and have substantial canine teeth, they are aggressive in counterattack and would rather not be killed. But, even a bite from a less formidable prey animal, like the galagos that the Fongoli chimpanzees hunt, is painful enough to be avoided if possible and also holds the potential to become lethally septic to a predator. The Fongoli chimpanzees thus give themselves an advantage in using crude spears to gain at least minimal physical separation between them and galagos during the capture/kill phase of predation. In a relevant cross-hominoid comparison, we have often observed Hadza (a group of traditional human foragers from Lake Eyasi, Tanzania) hunters capturing and killing galagos in essentially the same way as do the Fongoli chimpanzees. A Hadza man will poke an untipped wooden arrow into a galago sleeping hole, stab the hiding animal and pull it out on the end of the arrow, and then crack it repeatedly over the head with another arrow until it dies. The galago fits comfortably in the man's hands once dead, but, like the Fongoli chimpanzees, that man knows a galago bite would, at the very least, smart.

Another salient resemblance between the Fongoli chimpanzees and modern human foragers is that hunting weaponry – and its potential to enhance distance (if even minimally, as in the case of the stabbing spears of the chimpanzees) – imbues each, compared with other hand-hunting chimpanzee populations, with a relatively cool, composed approach to predation (Pickering, 2012). In contrast, most chimpanzee monkey hunts culminate in brute force brawls, much like the intraspecific, no-holds-barred fights among male chimpanzees that Jane Goodall (1986: 317) described as "hitting, kicking, stamping on, dragging, slamming, biting, scratching and grappling" matches.

Pickering (2012) expands on these points and their implications for human evolution in a book-length treatment. That extended discussion builds, in part, on the work of comparative psychologists Brian Hare and Michael Tomasello (e.g., 2004; Hare, 2007), who concluded that the social skills of chimpanzees (and other studied nonhuman apes) are hampered by their social emotions, those feelings that are produced in the presence of another living being. In contrast, hominins gained control of their "emotional reactivity" sometime during the course of our evolution, probably since our lineage split with the panins.

The possibility does exist, however, that emotional control preceded that split. Like humans, bonobos (*P. paniscus*), the other closest living relative of chimpanzees, are comparatively docile, cooperative, and just generally more agreeable than are chimpanzees. The implication that can be drawn – that human-like emotional control was a feature of the most recent common ancestor of bonobos, humans, and chimpanzees – is intriguing but remains to be tested in any serious way. If true, it would mean that the more extreme emotional reactivity displayed by common chimpanzees is a relatively recently evolved characteristic, unique among these three closely related primate species. Efforts to increase our presently poor knowledge of bonobo psychology should someday aid in testing this hypothesis.

Although the Fongoli chimpanzee predation data do nothing to shed light on this larger issue, they do again highlight the utility of emotional control in a primitive hunting context. As argued here, hunting weaponry is probably a key factor in allowing that emotional control. With their predator-prey distance-enhancing capability, weapons mitigate against possible injury from counterattacking prey. Hunting weapons would have provided prehistoric hominins with the same advantage, whether or not an overarching, general management of emotional reactivity had yet evolved.

Stabbing, thrusting, and casting

Of course, once hunting with weaponry became a fixed pattern of behavior, it is likely that hominin predation also became more efficient and was directed at increasingly larger prey animals. It is not difficult to envisage the evolution of hominin hunting techniques from a Fongoli-type of probing/stabbing "spear" use to effective spear thrusting and/or short-distance spear casting, and finally to tool-assisted (e.g., atlatl, bow) long-range propulsion of javelins, darts, arrows, and other projectiles. In Chapter 6, Pickering and Domínguez-Rodrigo hypothesize how the earliest hominin hunting was accomplished using hands and perhaps stabbing "spears." The efficiency of modern bow-hunting people is also well documented ethnohistorically and ethnographically. It is, however, the midpoint of this proposed technological evolution and its possible application in hunting that remains elusive, although strides are being made to reveal it.

There is debate about the way in which hominins wielded the earliest known spears from the 400 ka site of Schöningen (Germany). Even though the three Schöningen spears are, in form, very similar to the aerodynamic javelins hurled by modern track athletes (i.e., each of these 6–8 ft. long spears has its center of mass positioned toward its intentionally tapered "business-end"; Thieme, 1997), some analysts nonetheless interpret them, and other penecontemporaneous spears from Lehringen, (Germany; Movius, 1950) and Clacton-on-Sea (England), as thrusting spears (Oakley et al., 1977). This alternative view of the archaeological spears' function rests largely on the fact that they are thicker and heavier than are ethnographically known javelins. But, extrapolating from the inferred large body masses of non-modern, Pleistocene *Homo* (Ruff et al., 1997), there is every good reason to suspect that the brawny individuals of these species were much stronger than are modern *H. sapiens* (see also, e.g., Ruff et al., 1993). Thus, non-modern hominins could presumably hurl effectively heavier spears than can modern people.[2] To their credit, even some doubters of the Schöningen-spears-as-javelins do admit that "with respect to hand-delivered spears, the distinction between thrusting and throwing spears is largely artificial: recent (historically known and extant) hunter-gatherers use hand-held spears in both manners" (Schmitt et al., 2003). Also to their credit, these researchers acknowledge the general capabilities of early hominins to hunt using spears, at least in some manner.

The opinion of others, who flatly deny spear use before Schöningen, is probably based on at least two reasons. First, is the very real archaeological absence of spears before Schöningen. There is no getting around this lack of empirical evidence, but we do note two pieces of indirect data that hint inferentially at hominin spear production in the early Pleistocene. First, microwear analysis of some stone artifacts from ~1.6 Ma sites at Koobi Fora (Kenya) revealed polishes attributed to wood-working activities, which could have included spear manufacture and modification (Keeley and Toth, 1981). Second, phytoliths, which are probably derived from acacia trees, have been identified on several ~1.5 Ma large bifacial artifacts from Peninj (Domínguez-Rodrigo et al., 2001). Acacias are sturdy trees, and their phytolithic traces on ancient stone tools likely reflect hominin chopping activities, perhaps including the production of spears.

The second argument against pre-Schöningen spears is predicated on the (at least) tacit assumption that *substantial* distances between an early hominin predator and its prey were necessary for safe hunting. And, because it is likely, in the doubters' opinion, that the very first (as-yet undiscovered spears) were thrusting weapons, it is hard for some to imagine that that distancing was possible. In many cases this latter assumption is based on

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[2] Further, the Clacton-on-Sea wood fragment might not even be a spear; alternative interpretations of the artifact posit it was a digging stick or possibly even a snow probe (Gamble, 1986, 1987).

appeal to an interesting study of the global pattern of Neanderthal (*H. neanderthalensis*) skeletal trauma. Suspicious that common and often ruinous Neanderthal bone injuries might have resulted from the intimate contact with large dangerous prey that is required by spear thrusting,[3] Berger and Trinkaus (1995) sought to confirm this suspicion by comparing Neanderthal skeletal damage to that of modern people who also interact regularly, closely, and physically with imposing hoofed beasts. The frequency and patterning of Neanderthal bone breakage matches that seen in rodeo athletes. Said package quickly and convincingly became the "Neanderthal story": an all-in, wrecking-ball approach was how they hunted, and for this they paid a high bodily price, regardless of their hyper-rugged physiques. The story concludes with the cautionary advice to imagine how much worse – probably deadly – close-range, spear-thrusting hunting would be for smaller, more gracile early *Homo*.

There is, of course, a middle ground: the issue at hand is not necessarily complete physical contact with prey using thrusting spears versus long distance separation from prey using efficient throwing spears, but perhaps instead short- or medium-distance projectile hunting – an idea that is seldom seriously considered for prehistoric hominins. Such a technique would have provided minimal – but crucial – distancing between predator and prey, just as simple stabbing spears allow the Fongoli chimpanzees to hunt their minimally dangerous galago prey.

Anthropologists have, at various times and in various ways, tested the stopping power of spears, but no one has yet determined the distance at which an untipped spear penetrates prey skin and hair less effectively than a tipped one. Nonsystematic experimentation by Manuel Domínguez-Rodrigo (personal communication, 2010) shows that zebra hides and those of other

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[3] Much of the image of Neanderthals as close-range, large-game hunters also probably stems from their life-reconstructions as brawny, muscular people. In addition, much detailed analysis of their shoulder girdle and upper limb combined to indicate a possible spear thruster (see e.g., Churchill et al., 1996; Schmitt et al., 2003; Rhodes and Churchill, 2009). For instance, Neanderthals have increased humeral retroversion compared with most modern people, but analyses conclude that, in this case, the retroversion has more to do with overall Neanderthal body form and activity than with habitual throwing. Tentatively corroborating that conclusion, the three known Neanderthal skeletons with paired humeri also lack bilateral asymmetry in the angles of their right and left humeral retroversion. Neanderthal humerus shafts are asymmetrical, however, with right humeri thicker anteroposteriorly than are left humeri. But, compared with modern humans from the Upper Paleolithic, both right *and* left humeri of Neanderthals are relatively wide in this plane compared to their thicknesses mediolaterally. The anteroposterior thickness of Neanderthal humerus diaphyses might have functioned to resist "large bending moments engendered by *thrusting* spear use" (Schmitt et al., 2003), perhaps supporting the notion of them as physically rugged, close-killing, big-game hunters.

medium-sized ungulates can be pierced effectively with untipped spears at ranges up to fifty feet (see also, Smith [2003], who documented experimentally diagnostic patterns of wooden spear damage on ungulate bones – types of modifications that might also be detectable on fossil remains). Of course, that is with modern human throwers, and although we have good reason to infer that our early ancestors were much stronger than we are, the throwing ability of early *Homo* might have been poorer than ours.

Among early hominins, only Upper Paleolithic *H. sapiens* show use-related humeral retroversion angles (in their presumed throwing arms) and significant degrees of bilateral asymmetry in inter-humeral retroversion angles (leading to inference of right- or left-handedness, rather than ambidexterity, in throwing; Rhodes and Churchill, 2009). Early modern human humeri also display well-rounded diaphyseal cross-sections, which, among other possible functions, might have acted to resist the torsion that accompanies a throwing motion (Churchill et al., 1996; Schmitt et al., 2003).

The sample of *H. erectus* shoulder girdle bones is small, with some of the best preserved belonging to the ~1.5 Ma Nariokotome Boy (KNM WT-15000) skeleton (West Turkana, Kenya; Walker and Leakey, 1993). The skeleton's clavicles are short relative to those of modern humans, and this may have had a significant negative impact on the boy's throwing abilities. Larson (2007, 2009: 72) explains that a long clavicle "forces" the scapula closer to the vertebral column than is the case in hominins with short clavicles: "Such a shift in scapula position would have dramatically increased the range of upper limb motion, particularly in the posterior direction. One potential factor stimulating such a change is throwing, which entails a significant component of posterior motion of the abducted arm during the cocking phase." The implication is that *H. erectus*, as characterized by Nariokotome Boy, was a weak thrower.

Clearly, more fossils are needed to verify this argument. Nariokotome Boy was a single individual, and he could have been afflicted with a pathology known in modern humans as short clavicle syndrome. It does seem more likely, however, that Nariokotome Boy represents the typical morphology of *H. erectus*, especially considering that three new *H. erectus* clavicles from the ~1.8 Ma site of Dmanisi (Republic of Georgia), display his same anatomy (Lordkipanidze et al., 2007). However, there is no reason that this means *H. erectus* was necessarily incapable of hunting with spears – especially when considering the proposal of short- or medium-distance casting. *Homo erectus*' upper body strength would have been advantageous if it were a close-range spear thruster or caster. Even chimpanzees, with their ape shoulders and arms, throw rocks and sticks sufficiently well to drive away prowling leopards (*Panthera pardus*; e.g., Nishida, 1968; van Lawick-Goodall, 1968) and even lions (*P. leo*; Hiraiwa-Hasegawa et al., 1986).[4]

In addition, fire treating a wooden spear strengthens its tip, thereby amplifying its potential force of impact. Several claims of evidence for hominin-controlled fire in East and South Africa as early 1.5 – 1.0 Ma, within the known time range of *H. erectus*, are hotly contested (see review in Klein, 1999). Even if that evidence is someday verified, critics of spear hunting will simply assume the fallback position that the required closeness to prey for thrusting or short-distance hurling of a spear disqualifies it as a hunting option for premodern hominins.

True, proximity to large wild animals can *sometimes* be perilous. Opponents of early spear hunting point to Berger and Trinkaus's Neanderthal study and argue that injuries similar to those incurred by modern rodeo performers – an occupation that more than any other places its practitioners in intimate, dynamic contact with large animals – would have been fatal for early hominins, who obviously lacked access to medical care. There are, however, safer (and smarter) ways to get close to large, gregarious ungulates, minimizing injurious risk. One of those ways is by slow, steady approach on foot. Anecdotally, it seems that small, solitary antelopes are much more vigilant, and this natural skittishness makes them more difficult to approach closely than larger, social ones (personal observations).

Another technique that puts modern human hunters in relatively safe proximity to large game is ambush hunting. Ambush hunting requires stealth but not much more than rudimentary observational skills to find a game trail or waterhole and set up beside it. Recently, Bunn and Pickering (2010b) argued that age-at-death profiles of ungulates (based on dental eruption/wear stages of the teeth) from FLK 22 *Zinjanthropus* that are inferred taphonomically to be early *Homo* prey remains (see e.g., Bunn, 1982; Bunn and Kroll, 1986; Domínguez-Rodrigo et al., 2007) resemble those of prey remains known to be created by modern ambush predators, like some African felids, but are more enriched in prime adult animals. These authors went on to propose that short-range spear-casting from trees was possibly the specific type of ambush hunting undertaken by early *Homo*, an animal that presumably (like us, its descendants) inherited good climbing

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[4] We also recognize that it is possible to kill with spears *without* throwing *or* thrusting them. For instance, they can be employed as pikes braced in the bases of pit traps, but this is a decidedly more cognitively sophisticated approach to "hunting" than we might want to ascribe a priori to *H. erectus*.

capabilities from its primate forebears. Modern human hunters still exploit this ability when hunting in tree stands. Hares and other diminutive prey tend to be hyper-vigilant because they can be attacked from many directions: a large raptor diving from above is as likely a predator of these small animals as a canid coursing through the underbrush. Medium and large ungulates, on the other hand, are most often attacked from ground level, so it is unsurprising that the focus of their watchfulness is at that level.

"Man the ambush predator": unthinkable, unknowable, or unavoidable?

We anticipate that some anthropologists will be unwilling to even consider our hypothesis of ambush predation by early *Homo*. Perhaps this is because some colleagues seem to view such four-letter words as *meat*, *hunt*, and *kill* as distasteful, unwanted, and basically unthinkable elements in our ancestry. In historical perspective, this resistance is somewhat understandable: Raymond Dart's (e.g., 1957) dramatic, visceral, but ultimately fanciful (see Brain, 1981), hypothesis of a bloodthirsty "killer ape" parentage for humanity, and the highly influential but tacitly gender-biased "man the hunter" paradigm (e.g., Lee and DeVore, 1968), both soured many to any suggestion of hunting's import in our evolutionary past. We argue, however, that the hypothesis of early *Homo* ambush predation should be evaluated on its own merits.

The reasonable scientific approach to learning how we evolved into humans requires that we examine the available evidence and try to determine from it what happened in the past. What were the pivotal foraging adaptations that enabled some ancestral hominins to succeed? Many aspects of the behavior and diet of early Pleistocene hominins are certainly unknowable, and some would argue that meat foraging methods and capabilities are on that list. We disagree and discuss here our reasons why.

As indicated earlier, there is a finite and actually very short list of general methods by which hominins could have acquired whole or nearly whole large ungulate carcasses. This includes early access by (1) managing to find carcasses of animals that died of natural causes other than predation before competing carnivore scavengers; (2) power scavenging from large predator kills; (3) hunting; and (4) combination(s) of the above methods. The specific methods (1) through (3) have proved difficult to distinguish using the conventional combination of zooarchaeological data on ungulate skeletal part representation and bone surface modifications. Mortality profiles of prey animals, however, have the potential to break this impasse and provide decisive leverage for achieving more definitive reconstructions of the dominant method(s) of carcass foraging by ancient hominins.

Research on modern predator-prey dynamics has documented that while predation is complex, even in modern settings with direct observation, there are patterns in prey selectivity according to prey size for different carnivores, and according to prey age for different hunting methods (for archaeologically relevant summaries of modern wildlife studies, see e.g., Stiner [1990]). For example, lions preferentially kill large, size class 3 animals (for ungulate size classes, see Brain [1981]; Bunn [1982]), whereas leopards kill smaller size class 1 and 2 prey. These ambush predators (lions and leopards) kill a representative sample of different aged animals in a population, but cursorial predators (e.g., spotted hyenas [*Crocuta crocuta*]) kill mostly the weaker, more vulnerable juvenile and old adult animals, rather than prime adults. In both modern and fossil contexts, the approximate age at death of prey animals can be determined from eruption and occlusal wear patterns of their teeth, and any resulting patterns, or mortality profiles, can then be analyzed in relation to prey selectivity of different predators, hunting/scavenging methods, and specifically, the four potential methods of carcass foraging outlined above.

In a recent study, Bunn and Pickering (2010a,b) conducted this type of analysis using the fossil teeth of butchered bovid prey from the FLK 22 *Zinjanthropus* archaeofauna. Test predictions for the different foraging methods and a summary of results are as follows. If early *Homo* at FLK 22 *Zinjanthropus* was scavenging carcasses resulting from events other than predation, then those dead animals should have been predominantly the weakest, most vulnerable juvenile and very old adult individuals, who would have been most susceptible to death from natural accidents or other stressful conditions. If, instead, early *Homo* was aggressively scavenging from large carnivore kills, then depending on the carnivore and hunting method, different mortality patterns would be predicted. Specifically, if scavenging from cursorial predators (i.e., hyenas), then the prediction would again be for a predominance of the vulnerable young and old, but for smaller prey, there was likely very little to be scavenged from hyenas. If scavenging from ambush predators, then the prediction is that the mortality profile should match what these are known to kill; that is, for leopards or arguably other leopard-sized felids in the early Pleistocene, the prediction is for smaller bovids and an unselective mortality profile (called a *living-structure* or *catastrophic profile* in the literature). For lions or other lion-sized felids in the early Pleistocene, the prediction is for large, size class 3 bovids and an unselective, living-structure mortality profile. If early *Homo* hunted by endurance running, as hypothesized recently by Bramble and Lieberman (2004), then the prediction is the same as for other cursorial predators, a predominance of young and old rather than prime adults. Finally, if early *Homo* hunted by an ambush method, then the prediction is for a living-structure profile, unless hunting capability was sufficient to enable further selection for a particular subset of the prey population, or unless prey socioecology increased vulnerability of some subset of the population. Stiner (1990), for example, developed the argument that only human hunting produces what she termed *prime-dominated mortality profiles*, in which weaponry enables human hunters to select the largest, most energetically valuable prime adults.

Using the modified triangular graph approach of Steele and Weaver (2002), we analyzed the bovid mortality data from FLK 22 *Zinjanthropus* in comparison with modern carnivore data from the wildlife literature, and we obtained some interesting, statistically significant results. For smaller bovids, FLK 22 *Zinjanthropus* ($n = 7$) is dominated by old adult males, which contrasts completely with what modern leopards are known to kill. At a 95% confidence level, early *Homo* did not scavenge from tree-stored leopard kills (as was suggested by Cavallo and Blumenschine [1989]) to obtain these carcasses. This result is confirmed by a two-tailed Fisher's exact test, comparing prime to old adults from modern leopard kills in the Serengeti to FLK 22 *Zinjanthropus*, which yielded a statistically very significant p value of 0.0034. The finding of old adults is partially consistent with the endurance running-hunting hypothesis writ generally but is *completely inconsistent* with the on-the-ground reality of tracking animals in savanna mosaic habitats that characterized the places where early *Homo* evolved. Hadza foragers, who are skilled, lifetime trackers, routinely lose arrow-shot, bleeding prey in such habitats, and they would not even consider trying to track healthy animals through heavy vegetation at a jogging pace. Attributing to early *Homo* tracking skills well beyond those of modern foragers is simply unrealistic (e.g., Pickering and Bunn, 2007). In all probability, early *Homo* hunted small bovids by ambush methods to acquire the carcasses represented at FLK 22 *Zinjanthropus*.

For large, size class 3 bovids, FLK 22 *Zinjanthropus* is dominated by prime adults. At a 95% confidence level, the pattern at FLK 22 *Zinjanthropus* is distinct from what Serengeti lions produce from killing wildebeest (*Connochaetes taurinus*). A two-tailed Fisher's exact test comparing large bovids at FLK 22 *Zinjanthropus* with Serengeti lion-killed wildebeest yielded statistically significant differences ($p = 0.0439$). Early *Homo* at FLK 22 *Zinjanthropus* was probably not scavenging from lions or other lion-sized felids, nor is the predominance of prime adults consistent with predictions of the endurance running-hunting hypothesis. The predominance of large prime adults is also inconsistent with hypothesis (1), a variant of the passive scavenging hypothesis. Thus, we are left with ambush hunting as the likely predominant cause of the mortality profiles documented at FLK 22 *Zinjanthropus*.

Charting the course forward

Discussions in the previous sections serve to illustrate that for our science, paleoanthropology, to progress it needs not only firm rooting in empiricism but also the courage to lean on imagination and metaphor in hypothesis building. There is nothing inherently "unscientific" about this approach, and importantly, this is *not* the same as recommending wild conjecture as a course forward. As Bunn and Pickering (2010b: 403) state in similar context: "How can science ever determine when this behavior actually began, if we allow our investigations to be guided by ideology rather than evidence and if we do not ask such provocative questions and then explore ways of answering them?"

Currently, we are devising tests of our self-provoking hypotheses sketched out above. These tests include experimentation with spear manufacture and use/efficiency/penetrating power and simulated spear hunting from trees in modern African contexts that replicate reconstructed hominin paleohabitats. Pickering (2012) explores the implications of his proposal that early hominin hunting and common aggression need to be decoupled in light of what we now know about ape psychology and the composing affect of hunting with weapons. As small-scale (i.e., non-societal) violence often concedes emotional control, it is an inept way for a human to hunt large, wary, and sometimes dangerous prey. In that light, hypotheses – both antique (e.g., Raymond Dart's [e.g., 1957] "killer apes") and recent (e.g., Richard Wrangham's [e.g., Wrangham and Peterson, 1996] "demonic males") – that equate the real propensity of male apes (hominins included) for startling interpersonal violence with hunting success are revealed, instead, as conflations.

It is only recently that this next-order level of hypothesis building and testing has become a justifiable pursuit for paleoanthropological zooarchaeologists, because debate since at least the early 1980s has been more concerned with simply establishing the dominant taphonomic pattern of early versus late access to carcasses by hominins. Secondary, tertiary, and popular literature still lags behind the shifting majority opinion of most specialists, which now accepts early access as the dominant signature, whether assessing early Pleistocene evidence from Africa or Neanderthal materials in Europe. This new plane of investigation will require just as much rigor and perhaps even more creative thinking to reveal a more refined understanding of early hominin behavioral patterns. But, if hunting hypotheses emanating from the opening decade of this new century (e.g., persistence hunting by endurance running [Bramble and Lieberman, 2004]; ambush hunting from trees [Bunn and Pickering, 2010b]) are indication of the commitment of researchers in this direction, paleoanthropologists seem well up to meeting these invigorating challenges.

Acknowledgments

We thank our colleague and dear friend Manuel Domínguez-Rodrigo for the invitation to contribute to this book. Pickering thanks his family for their continued support and encouragement.

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CHAPTER 6

Can we use chimpanzee behavior to model early hominin hunting?

Travis Rayne Pickering and Manuel Domínguez-Rodrigo

In a recent article (Pickering and Domínguez-Rodrigo, 2010), we answered yes to the question posed in the title of this chapter. In our opinion, extant chimpanzees (*Pan troglodytes*) can be carefully employed as a useful referent taxon for modeling hunting by the earliest hominins (>2.6 Ma [mega annum]), who presumably lacked the modified lithic cutting technology that characterized more derived species that existed ≤2.6 Ma. This opinion contrasts with that of others who reject models that are explicitly "referential" in paleoanthropology and instead argue that the only suitable models of human evolution are those that are "conceptual."

The fundamentals of referential modeling in paleoanthropology

In their canonical 1987 paper, "The reconstruction of hominid behavioral evolution through strategic modeling," J. Tooby and I. DeVore were among the earliest voices to stigmatize the use of chimpanzees as early hominin referents. Most recently, chimpanzee referential modeling has come under intense critical scrutiny by K. Sayers and C.O. Lovejoy (2008). It is important to note, though, that (unlike Tooby and DeVore) it does not seem that these more recent authors are necessarily arguing a hard-line version of conceptual modeling. But, within their more expanded critique, Sayers and Lovejoy do express serious qualms about the utility of chimpanzee hunting for modeling early hominin hunting, the approach we specifically recommend in this chapter. And, it is important to note at the outset that this is, indeed, *all* that we are arguing in this contribution. We hold no illusions that chimpanzees can serve as useful referents for modeling all, most, or perhaps even few other paleoanthropologically salient aspects of early hominin evolution. Further, we also recognize that using chimpanzee data to model early hominin hunting is not the same as simply overlaying *all* aspects of chimpanzee hunting onto Mio-Pliocene hominins. Specifically, we never contend that the mechanics of early hominin hunting were the same as that typically employed by arboreally acrobatic, superhumanly strong extant chimpanzees when they dispatch monkey prey high in the forest canopy. It is not merely unrealistic to suggest this; it is folly. It is no subtle detail to establish a priori the parameters of any analogy if it is to have maximal explanatory power. This is true at every level: across different categories of behavior (e.g., locomotion, tool use, hunting) *and* within individual behavioral complexes (e.g., hunting, writ complete).

Especially relevant to these critical points is the carefully articulated view of J. Moore, who among very few others in the wake of Tooby and DeVore's diatribe, argued quietly but cogently for the continued utility of chimpanzee data in building testable hypotheses of the most recent common ancestor (MRCA) of humans and chimpanzees and the very earliest hominins. He advocated

> the use of a referential approach in which the model is not a single typological modern species [chimpanzee] *per se*, but the set of differences observed between populations of that highly variable species . . . I believe that such an approach has great potential . . . I do not believe the method can stand on its own, though, any more than can the simple analogical, cladistic, or strategic modeling approaches; and none of these approaches to behavioral scenarios will get far without careful attention to the fossil and archaeological evidence (Moore, 1996: 285–286).

Moore's closing point was prescient in light of the recent interpretation of the functional morphology of 4.4 Ma *Ardipithecus ramidus* (White et al., 2009a and references therein).[1] If *Ar. ramidus* is indeed an early

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[1] Moore's point is echoed consistently in the numerous admonishments of T.D. White (e.g., 2006: 103) and his colleagues (such as especially C.O. Lovejoy [see, e.g., Sayers and Lovejoy, 2008]), who stress that "the rich detail of the modern world compared to the paucity of the prehistoric world can serve to obscure the recognition and analysis of evolutionary novelty. The present illuminates the past in myriad ways. However, the unwary paleobiologist can easily misinterpret past organisms by using inappropriate interpretive constructs based solely on modern form and function. For example, the dichotomization of locomotor modes as 'bipedality' versus 'quadrupedality' works well to describe modern humans and their close African ape relatives. But what about now-extinct organisms who lived during the Upper Miocene, near the base of the [hominin] clade? Does this neontological dichotomization actually serve to obscure evolutionary novelty that is accessible only through the paleontological record? This is a fundamental issue for the study of [hominin] origins and early evolution." We understand this point and are definitively sensitive to it, as exemplified by our study of tooth marks on fossils from Swartkrans Member 3 (South Africa). Provoked by the statement "The [inferred] ability of [modern] leopards to kill and

above-branch arborealist/terrestrial biped *and* a hominin, then it demonstrates not only the highly divergent evolutionary pathways taken by extant African apes and hominins after those lineages split sometime >6.0 Ma but also that no contemporary referent species could have been used to predict or model its unique positional and locomotor adaptations (White et al., 2009a). But, does that mean by extension that data on extant chimpanzees are also useless for helping to reconstruct *every* aspect of early hominin behavior?

In response, we first emphasize that biochemical (e.g., Goodman, 1963), molecular (e.g., Ruvolo, 1997), and genomic (e.g., Bradley, 2008) data converge to conclude that extant chimpanzees and humans are sister taxa, which shared an MRCA 8–4 Ma, more recently than existed the MRCA that either shared with *Gorilla* (e.g., Bradley, 2008; Fabre et al., 2009). The phylogenetic proximity of chimpanzees and hominins is one critical characteristic that renders the former an appropriate referent for modeling at least *some* aspects of early hominin behavior. This is the case because, as discussed in the next section, the habitats occupied by the earliest hominins were, in at least some cases, similar to those inhabited by some extant chimpanzee populations. Taxa that are phylogenetically close and subjected to similar selective forces (e.g., because they reside in similar ecosystems) are predicted to "respond" more similarly morphologically, physiologically, and behaviorally than are more distantly related taxa.

We think that those who malign chimpanzee referents obfuscate this principle when they reject chimpanzees as referents that are useful for modeling early hominin hunting. This obfuscation is accomplished, in part, by appealing to the fact that other more distantly related taxa also hunt in groups (e.g., social mammalian carnivores, hawks). Some critics of chimpanzee referents seem to think that the vast phylogenetic distance between those appealed-to, non-hominoid species and hominins can be leveraged

cache prey many times their own body weight would imply that perhaps we need not invoke the activity of sabre-tooth cats in the accumulation of bones found in the hominin bearing caves of South Africa" (de Ruiter and Berger, 2000: 680), we found that quantitative analysis of the actual tooth marks on bones from Swartkrans Member 3 showed many to be outside the range that leopards are capable of producing and thus concluded that "our results highlight the hazards of blanket statements based on the application of inadequate actualistic criteria in complicated taphonomic settings, such as that at Swartkrans. The indirect evidence of modern leopard killing and carcass transport capabilities is by itself not sufficient to explain the deposition of variably sized animals during the Plio-Pleistocene in South African cave sites. More particularly, direct paleontological evidence, in the form of taxon specific carnivore tooth marks, better informs investigators on this matter in the Swartkrans Member 3 fauna. We suggest that the same will probably hold true for other assemblages as well" (Pickering et al., 2004: 601).

to demonstrate the trifling nature of analogical reasoning generally and of chimpanzee referential approaches specifically. But, highlighting the trivial nature of the shared feature (in this example) of group hunting in hominins and non-hominoid taxa ultimately reduces to an intellectual diversionary tactic. Rather than grapple with critical chimpanzee-hominin continuities, this tactic merely directs attention away from those important, *non-trivial* similarities between chimpanzees and hominins – the first of these non-trivial similarities being phylogenetic proximity and the second being a greater degree of shared functional anatomy than either chimpanzees or hominins share with the Carnivora and raptors. Thus, chimpanzee models of hominin evolution have two advantages that separate them clearly from the red herrings thrown out by conceptual modelers when they appeal, for example, to lions and hawks in attempts to invalidate chimpanzee referents. Chimpanzee models of human evolution are based on *both* homology (similarities from recent common descent) *and* homoplasy (similarities from common adaptations) (Moore, 1996).

An appropriate model in any science is never based on trivial (or, in the parlance of theoretical archaeology, formal [e.g., Gifford, 1981; Gifford-Gonzalez, 1991]) analogies. Instead, the best scientifically testable models are fashioned through the dialectic use of groups of linked non-trivial analogies. Non-trivial analogies are called structural analogies by theoretical archaeologists and are distinguished from trivial/formal analogies in that they – non-trivial/structural analogies – are inferences built on analogical reasoning, rather than simply being transcriptions of observed analogs onto the subject of inquiry (that subject being the past for historical sciences, such as paleoanthropology; e.g., Gifford-Gonzalez, 1991; Aronson et al., 1995; Bunge, 2006).

The linkages between these non-trivial analogies must be forged by consideration of close phylogenetic relationship (discussed above) and corresponding ecological context, the two essential variables that also just happen to form the core of evolutionary behavioral ecology. Staunch conceptual modelers may reject this principle – but, it is worse still when poor referential modelers simply fail to recognize it (or choose to ignore it) and instead simply overlay their own trivial analogies on to the hominin fossil record, with no effort to transform sets of non-trivial analogies into proper models.[2] With this contribution, we hope to make a break from that indolent practice in chimpanzee referential modeling. Having earlier synopsized the rather

[2] See Pickering and Domínguez-Rodrigo (2010) for specific examples of models of human evolution that simply overlay trivial chimpanzee analogies onto the hominin fossil record.

uncontroversial inferred close phylogenetic relationship of chimpanzees and hominins, we now move on to a specific consideration of the ecological context of early hominin evolution and then focus on chimpanzee hunting data from populations occurring in similar habitats.

Hominin habitats

Currently, there are three late Miocene hominoid species, *Ar. kadabba* (Haile-Selassie, 2001; Haile-Selassie and WoldeGabriel, 2009), *Orrorin tugenensis* (Senut et al., 2000), and *Sahelanthropus tchadensis* (Brunet et al., 2002), argued to be the earliest known hominin. It is also possible that the three species might actually belong to one genus or even compose subspecies of the same species (e.g., Haile-Selassie et al., 2004). Geologically, *Ar. ramidus* is a more recent species than are *Ar. kadabba*, *O. tugenensis*, and *S. tchadensis* (WoldeGabriel et al., 1994). *Ardipithecus ramidus* also samples a larger hypodigm and is more derived anatomically than those earlier occurring taxa (White et al., 1994; Haile-Selassie et al., 2004). Its paleoenvironment is extensively described both at Aramis (Ethiopia), the locality at which it was originally discovered (WoldeGabriel et al., 1994; White et al., 2009b; WoldeGabriel et al., 2009; Louchart et al., 2009), and at Gona (Ethiopia; Semaw et al., 2005; Levin et al., 2008), from which additional fossils of the species have been recovered. Assuming that one (or all of these proposed taxa) does indeed represent the earliest known hominin, what can we say about its ancient environment(s)?

Reconstruction of early hominin habitats is complex, drawing on paleobotanical, faunal, and isotopic datasets, themselves of varying relative completeness and accuracy and occupying multiple levels of resolution. We do not intend to provide here an exhaustive review of those datasets for the Miocene/early Pliocene species listed earlier but will instead summarize them. The preponderance of the evidence for all relevant taxa indicates that they are sampled from more closed paleohabitats (e.g., WoldeGabriel et al., 1994, 2001, 2009; Pickford and Senut, 2000; Vignaud et al., 2002; White et al., 2009a,b; Louchart et al., 2009; Haile-Selassie and WoldeGabriel, 2009) than predicted by various permutations of the "savanna hypothesis" of human origins, which envisaged a sharp transition between a tropical forest-dwelling protohominin and the first hominins living in open grasslands (e.g., Leakey, 1934; Dart, 1957; Cole, 1965; Washburn, 1973; Coppens, 1991; Pickford, 1991; Senut, 1991).

Extant chimpanzees do not live in open grasslands, but the paleontological falsification of the savanna hypothesis, strictly understood (i.e., hominization as an adaptive response to open grasslands), means that the possibility of ecological similarity still exists between some chimpanzee populations and the newly reconstructed paleohabitats of putative early hominins. Each of those newly reconstructed paleohabitats is a type of savanna. Modern ecology uses the term *tropical savanna* to encompass a variety of habitats, the unifying feature of which is that the groundcover is dominated by C_4 grasses (Harris, 1980; Huntley and Walker, 1982; Bourlière, 1983; Mistry, 2000); other than this basic requirement, tropical savannas range from densely wooded to wide open country (see Moore [1992] for discussion of savannas as they are relevant to chimpanzee referential models).

Debate has ensued over the type of savanna and/or habitat component(s) within a savanna mosaic that early hominins preferred. For example, White et al. (2010: 1105e), responding to a critique by Cerling et al. (2010) of the Aramis research team's reconstruction of *Ar. ramidus* habitat preference, state: "We made it clear that the regional and local environmental mosaics [of ancient Aramis] included grasslands as potential habitats available to *Ardipithecus* but concluded that 'Its ecological habitat appears to have been woodland-focused' . . . rather than grassland-based." Similarly, the paleontologically and isotopically based conclusion that *Ar. ramidus* and *Ar. kadabba* at Gona "may have inhabited a variety of landscapes and were not as ecologically restricted [to woodlands] as previous studies suggest" (Levin et al., 2008: 232; see also Semaw et al., 2005) is contested by the Aramis research team, which argues instead that the Gona data "show only that a range of habitats was present, and [thus] the attribution of *Ardipithecus* to any particular set of the available biotopes is problematical in this mixed assemblage" (White et al., 2009b: 92).[3]

We do not wish to enter the dispute between research teams to which we do not belong, but our understanding of the published data from Aramis, Gona, and other early hominin sites (i.e., Lothagam, Tanzania [Leakey et al., 1996]; Lukeino, Kenya [Pickford and Senut, 2000]; Toros-Menalla, Chad [Vignaud et al., 2002]) is that they, by and large, sample ancient savanna mosaic ecosystems, each of which contained open habitat biotopes. Even if hominins preferred the woodland components of these ecosystems,

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[3] Based on our field and laboratory experience at Gona and on our understanding of the relevant literature, we do not agree that Gona faunas in question are mixed. The depositional contexts from which they derive are composed of fine-grained sediments, associated with lakeside, pond, or possibly spring settings, and the recovered fossils show no cortical abrasions or polish indicative of transport (personal observations; Semaw et al., 2005; Levin et al., 2008).

the essential nature of those woodlands would have been different than the dense tropical forest habitats *preferred* by *most* extant apes. For instance, the relatively cool high-altitude forest/woodland context reconstructed for *Ar. kadabba* from the Western Margin of the Middle Awash (Ethiopia) (Haile-Selassie and WoldeGabriel, 2009) would have likely been similar to modern Afromontane forests at high latitudes, which have pronounced seasonality. Indeed, by definition, all savannas experience seasonality. In turn, seasonality determines intra-annual resource availability and thus conditions the adaptive matrix of a savanna versus that in which occupants of dense tropical forests are embedded.

Modern chimpanzee populations range in distribution across the mid-latitudes of Africa from relatively uniformly dense rainforest to areas of more open savanna mosaic. We submit that those living in habitats clustered at the latter end of that environmental continuum experience a similar degree of seasonality as that that would have affected Mio-Pliocene hominins in seasonal woodlands.

Harkening back to Moore (1996), we thus contend that understanding intraspecific variation in the hunting behavior of savanna and forest chimpanzees (and its potential triggers) is the key to modeling the hunting behavior of basal hominins in wooded, seasonal habitats. Importantly, this process is *not* equivalent to weak formal analogizing, which simply overlays trivial analogies on to the fossil hominin record. Instead, a referent species (chimpanzee) has first been identified based on its phylogenetic proximity to hominins. Second, its role as a referent has been further refined by isolating an important, shared aspect of ecology: seasonality. Further, it is still not sufficient, at this point, to simply overlay the features of savanna woodland chimpanzee hunting onto *Ardipithecus*, *Orrorin*, *Sahelanthropus*, and early *Australopithecus*. Rather, we need to compare and identify contrasts in hunting across all chimpanzee populations for which we have relevant data and look for the underlying, ultimate evolutionary causes of hunting – be it/they universal across populations or instead rooted in specific ecologies.

Chimpanzee hunting

Newton-Fisher (2007) provides a useful summary of chimpanzee hunting. We draw heavily on that work here and direct interested readers to his paper for a comprehensive list of primary references on the topic.

The underlying evolutionary question of the ultimate cause(s) of chimpanzee hunting is unresolved, but worth consideration here. First, it is important to stress that chimpanzee hunting frequency varies among populations; some hunt rarely, whereas others are seemingly committed predators. Based on their impact on prey populations, some of the best-studied chimpanzee populations hunt quite frequently and successfully. The extreme of chimpanzee hunting dedication is documented at Gombe (Tanzania), where their predation on red colobus monkeys (*Piliocolobus badius*) over the years 1972–1991 killed 8% to 42% of that prey population annually (Busse, 1977; Wrangham and van Zinnicq Bergmann Riss, 1990; Stanford et al., 1994). But, since chimpanzee hunting rates vary so radically among populations then it seems entirely possible that triggers of chimpanzee predation might also vary intraspecifically.

Hypotheses of what causes chimpanzee hunting fall into two major groups: one group posits social impetuses as ultimately underlying predation; the other group sees ecological factors as driving the behavior. Mitani and Watts (2001) have forwarded one of the most prominent recent hypotheses in the first group, arguing that male chimpanzees who participate in hunts create alliances around that activity, and more importantly, around the consequent meat-sharing of successful hunts. Indeed, data from the Tanzanian sites of Gombe and Mahale, as well as from Taï Forest (Côte d'Ivoire) chimpanzees, show that the number of adult male chimpanzees in a group is positively and strongly correlated with hunting frequency (Mitani and Watts, 2001). In addition, at least at Ngogo (Uganda), larger male groups hunt more often, with a greater success rate of multiple kills per hunt, than do smaller male groups (Mitani and Watts, 2001; Watts and Mitani, 2001).

Large male group size at Gombe, Mahale, and Ngogo – which predicts the elevated rate and success of hunting in these populations – is in turn positively correlated with fruit abundance at those sites (e.g., Wrangham, 1977; Uehara, 1997; Takahata et al., 1984; Stanford et al., 1994; Watts and Mitani, 2001, 2002; Newton-Fisher, 2007). This latter correlation falsifies earlier ecologically based hypotheses that suggested chimpanzee hunting at those sites was linked to decreases in fruit availability, meaning that meat was being sought to mitigate shortfalls in energy sources.

Mitani and Watts (2001) also argue that hunting might be a "luxury activity," engaged in only when fruit availability is at its peak. Ripe fruit attracts aggregations of chimpanzees. Those aggregations can grow expansive because of the sheer abundance of fruit available to support them. With daily energetic needs easily met, large male subgroups can turn to high-cost predation without the risk of energy shortfalls that would be incurred by unsuccessful hunts when fruit is scarce. The evolutionary benefits thus obtained are not immediate energetic returns but potential long-term gains delivered by coalition membership (e.g., reproductive advantages).

The posed reproductive advantage of group hunting for males might be accurate *but* without it actually being the ultimate motivation for hunting. Alternatively, reproductive gain could be just one of multiple, linked ultimate motivations underlying hunting. Recognizing these potential complexities also acknowledges that edible animal product (hide, skin, meat, marrow, bone grease, and brains) is more than simply a source of energy. It is also a highly valuable nutritional resource – an especially excellent supply of protein. In fact, for meat to be used as the commodity (or tradegood) in male-male coalition building and solidification (e.g., Mitani and Watts, 2001; Watts and Mitani, 2001) or in scenarios of direct meat-for-sex (male-to-female) exchange (e.g., Stanford, 1998a), meat must hold some important intrinsic value (Newton-Fisher, 2007). Sensibly, the most obvious value of meat is nutritional, occurring as it does across an animal carcass as discrete packages of dense, easily digestible, and readily metabolized protein.

We might predict that seasonally annual shortfalls in protein availability are most acute in the most seasonal of the habitats that chimpanzees occupy. That chimpanzee hunting peaks in the late dry season at Gombe (Stanford, 1998a) and Mahale (Takahata et al., 1984) seems to support this contention. Regarding this point, it is important to emphasize that at Gombe (for instance) chimpanzee hunting of red colobus monkeys (their primary prey) is in part determined by encounter rate (Stanford, 1998b). Chimpanzee-monkey encounter rate is in turn linked to the daily travel distance of a chimpanzee party and the coincidence of chimpanzees and monkeys feeding on the same plant food sources during any particular day (Stanford, 1998b). It is reasonable to conjecture that expanded chimpanzee day range is ecologically based and that this wider movement might be instigated by nutritional/protein needs because typical sources of those dietary constituents are exhausted within the core of the home range. If so, the hypothesis that ecology/seasonality underlies hunting frequency is still maintainable.

A weak positive test of this hypothesis is found in data on chimpanzee hunting at Taï, a rainforest habitat that is markedly less seasonal than the Tanzanian and Ugandan sites discussed above, but that still does experience some seasonality in resource availability. Peak hunting by chimpanzees takes place at Taï during the wet season, which is the time of year when coula nuts (*Coula edulis*), a major source of protein and fat, are *not* in season (Boesch and Boesch, 1989; Boesch and Boesch-Achermann, 2000).

Using the impact of chimpanzee hunting on prey populations is probably not the best way to measure (or, more accurately, extrapolate about) hunting's frequency, but it is a proxy that is published commonly enough to have some limited heuristic value in addressing the topic. Moving from the least seasonal to most seasonal habitat: Taï chimpanzees killed 3% to 8% of the red colobus monkey population in any given year during the 1980s (Boesch and Boesch-Achermann, 2000); Ngogo chimpanzees killed 6% to 12% of the red colobus monkeys annually 1998–1999 (Watts and Mitani, 2002); Gombe chimpanzees killed 8% to 42% of the red colobus monkeys each year across several intervals during the 1970s, 1980s, and 1990s (Busse, 1977; Wrangham and van Zinnicq Bergmann Riss, 1990; Stanford et al., 1994).[4]

We also contend that if seasonally based resource depression is the major ecological factor underlying chimpanzee hunting, then hunting is likely to be more expansive and/or multifaceted at the most seasonal chimpanzee sites. Some field data provide tentative support for this proposal. First, woodland chimpanzee populations hunt a wider range of prey species (including several, non-arboreal/non-primate mammalian species) than do those living in forests (e.g., Uehara, 1997; Newton-Fisher, 1997). Good observational data on chimpanzee hunting at some of the most open-country and seasonal chimpanzee sites (e.g., the Bafing region [Mali], Mt. Assirik [Senegal], Ugalla [Tanzania], Semliki [Uganda]) are lacking, but McGrew et al. (1988) argue that dry-habitat-adapted savanna chimpanzees eat less meat than those at forest and denser woodland sites. McGrew (1983) suggests this apparent difference might arise from the fact that savanna chimpanzees face potential competition from a more taxonomically diverse and numerous carnivore guild than do other chimpanzee populations. That situation not only applies consistent non-chimpanzee pressure on prey species, removing many otherwise potential chimpanzee "victims" but also might make prey generally warier and render hunting more dangerous for chimpanzees, possibly bringing them in contact more frequently with large predators pursuing the same edible opportunities (McGrew, 1983).[5]

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[4] It must be noted, however, that chimpanzee predation pressure on red colobus monkeys at Mahale during the 1980s was minimal (Boesch et al., 2002; interestingly, as at Taï, chimpanzee hunting at Mahale is also more common during the rainy season [Watts and Mitani, 2002]), and conversely, that there was relatively heavy pressure exerted by chimpanzee hunting on guenon (*Cercopithecus miti*, *C. lhoesti*) populations at the tropical forest site of Kahuzi-Biega (Democratic Republic of Congo; Basabose and Yamagiwa, 1997).

[5] In addition, at sites like Bafing, much game has been extirpated through human hunting and population pressure (Jim Moore, personal communication, 2009).

However, the recent and best data on hunting by savanna chimpanzees comes from the site of Fongoli (Senegal), where chimpanzees fashion sticks into spears by chewing their ends into sharp points (Pruetz and Bertaloni, 2007). Twenty-two individual spear-hunts of galagos (*Galago galago*) were observed between March 2005 and July 2006, with majority of them ($n = 13$) occurring during the transitional wet season months of June and July, 2006. Similar to Ngogo, where chimpanzee hunting peaks with greatest fruit availability, it is during this transitional interval between the late dry season and early wet season that fruits eaten by chimpanzees at Fongoli are in their greatest abundance (Pruetz, 2006). Data are not yet available, but again the correlation between fruit abundance and apparent intensification of hunting at Fongoli might be linked to seasonal variation in the protein (and/or other non-caloric/energetic constituent[s]) content of the consumed fruit and/or the scarcity of protein-rich, non-fruit plant parts during the late dry season and early wet season. As a test of this general suggestion – that chimpanzee faunivory increases with the seasonal dearth of plant protein – it would be informative to determine whether chimpanzee predation on insects also increases as plant protein becomes scarcer at various sites across chimpanzee range. In general, termite fishing occurs at most sites most often in their wet seasons (McGrew et al., 1979; Nishida and Uehara, 1980; McBeath and McGrew, 1982; Collins and McGrew, 1987; Sugiyama and Koman, 1987; Fay and Carroll, 1994; Newton-Fisher, 1999; but see, contra, McGrew et al., 1979; Suzuki et al., 1995; Sanz et al., 2004; Bogart and Pruetz, 2008), but for any given site it remains to be shown if this is also a period of plant protein scarcity. It is interesting, however, that fruit abundance at Fongoli appears to coincide with frequent termite consumption there (Bogart and Pruetz, 2008), mirroring the positive correlation between increased galago hunting frequency and greatest fruit availability at that site. Bogart and Pruetz (2008: 610) speculate that: "A possible explanation for termite fishing during high fruit availability might relate to prey species available to the Fongoli chimpanzees . . . Owing to low mammalian prey species diversity at Fongoli compared with other chimpanzee sites, it may be that termites provide necessary protein."

Based on this brief review, it is obvious that the protein-compensation hypothesis that we favor to explain the underlying cause of chimpanzee hunting currently lacks many supporting data. However, in the field-demonstrated absence of any convincing correlation (much less an established causal relationship) between chimpanzee hunting peaks and the scarcity of non-meat resources that are rich in energy, protein (and/or other nutrient) capture seems the most plausible motivation that catalyzes chimpanzee hunting. Even if one favors a sociobiological explanation for chimpanzee hunting (i.e., meat-sharing among hunters builds strong male-male coalitions; direct, male-to-female exchange of meat-for-sex), the "possibility that chimpanzees achieve nutritional benefits directly from hunting cannot be easily dismissed . . . for either of the tradegood hypotheses to operate, there must be a nutritional gain to the individuals who receive and consume parts of the carcass. If there were not, the carcass would hold no value and could not be traded" (Newton-Fisher, 2007: 1313). The logic of this assessment is inexorable.

But, does this mean, in turn, that a provisional hypothesis of early hominin hunting is imminent? We believe the answer is yes, but with the caveat that currently the model is very simplified. We anticipate its elaboration with more data from studies of chimpanzee ecology and ethology and from the early hominin fossil and archaeological records.

A simple hypothesis of Mio-Pliocene hominin hunting

We begin our model with seven propositions:

(1) The earliest hominins occupied seasonal savanna habitats, probably at least as seasonal as the most seasonal of modern chimpanzee habitats.
(2) Such paleohabitats would have experienced seasonal fluctuation of hominoid-edible resources, as do similar modern ones inhabited by chimpanzees. Thus, energy and protein/nutrient availability would have also fluctuated intra- and interannually.[6]
(3) Animal carcass resources contain protein. Hominoids metabolize animal proteins more efficiently than they do plant proteins (e.g., Milton, 2002).
(4) Studies of carcass persistence in modern African savanna habitats show that scavengeable meat is rarely available. Thus, yields from passive scavenging in these habitats are limited largely to brains, bone

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[6] A recent report on tool-assisted plant foraging by savanna chimpanzees at Ugalla illustrates that the relationship between foraging and seasonality does not always meet the simple prediction of a positive correlation between seasonal decrease in overall food abundance and increased foraging intensity, elaboration (e.g., tool-use) and/or dietary breadth expansion (Hernandez-Aguilar et al., 2007). This example highlights why understanding the intra-annual dietary needs of consumers and intra-annual dietary content of their edible resources is so crucial to constructing accurate models that wish to harness variables like dietary/nutritional motivation and seasonality.

grease, and marrow (e.g., Blumenschine, 1986; Domínguez-Rodrigo, 2001).

(5) Prior to 2.6 Ma (de Heinzelin et al., 1999; Domínguez-Rodrigo et al., 2005), there is no evidence in the form of modified hammering stone tools or percussion damage on bones to indicate that hominins possessed stone percussion technology, which they could have used to break open skulls and ungulate long limb bones to scavenge brains and marrow.[7] Further, early hominin teeth – compared, for instance, with the specialized bone-cracking dentitions of hyenids and canids – would have been relatively ineffective for opening bones to access internal edible soft tissues.[8]

(6) Prior to 2.6 Ma (de Heinzelin et al., 1999; Domínguez-Rodrigo et al., 2005), there is no evidence in the form of flaked stone tools or cut

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[7] Our recent critique of claims for hominin butchery from the ~3.4 Ma Dikika (Ethiopia) site (McPherron et al., 2010), concluded that the published evidence does not, in fact, support the identification of bone surface marks on two fossils as unequivocal stone tool butchery damage (Domínguez-Rodrigo et al., 2010). We further asserted that any equivocation surrounding butchery claims of this great antiquity (~800 ka older than oldest known butchery marks from Gona, where marked animal bones are derived from fine-grained sediments and in spatial association with hominin-flaked stone tools) should lead to rejection of such claims. The equivocation stems from the facts: (1) that the Dikika fossils derived from a potentially abrasive sedimentary context, and (2) that the Dikika fossils show surface damage that is indistinguishable from that imparted randomly (by trampling and/or other incidental movement) in such deposits. Although, it may be possible to use unmodified stones cutting against bone to mimic the morphology of the Dikika bone surface marks (as McPherron et al. speculate was how the Dikika marks were produced), such results would in no way nullify the fact that randomly imparted striae can also mimic the Dikika marks, nor does it change the abrasive sedimentary context of the fossils. Thus, the null hypothesis of non-anthropogenic origin(s) for the marks remains unfalsified.

[8] Szalay (1975), however, argued: “It appears that the morphotype of the [early Pleistocene hominin] dentitions evolved in response to strong positive selection for increased incisivation and increased molar ability to withstand compressive forces. It is argued that these features, given the phylogenetic heritage of the first [hominins] from their pongid ancestry, are particularly appropriate to meat tearing and bone crushing . . . ” In addition, Binford (1984) baldly asserted hominin tooth marks on a bovid metapodial from the Middle Stone Age site of Klasies River Mouth (South Africa). More convincingly, White and Toth (2007) make the point that hominin tooth marks should not be unexpected in paleoanthropological faunas that they had a role in generating and predict a high degree of equifinality in the morphologies of tooth marks produced by hominins and carnivores. They also illustrated possible Neanderthal tooth marks on specimens in the cannibalized hominin bone sample from Krapina (Croatia), and, paralleling Szalay’s suggestion of hominin bone gnawing, point to “[A] probably female Swartkrans A. *robustus* maxilla (SK 65+67+74) features prominent step fractures on the labial surfaces of the canines. Such damage, with post-fracture wear polish and striae superimposed to show that the trauma was incurred *in vivo*, indicates forceful biting of a hard surface. Such damage is frequently encountered in different early [hominin] taxa. When found in hyaenids such damage is usually interpreted as evidence of bone chewing” (White and Toth, 2007: 290).

marks on bones that indicates that hominins possessed stone cutting technology.[9] In addition, early hominins did not possess carnassials or tearing claws and thus would have found it difficult or impossible to open the thick skins of large ungulates without using a sharp tool or having the skin opened by some other previous consumer of a carcass.

(7) Regardless of points (5) and (6), there are paleontological data indicating, to various degrees of certainty, meat-eating by early *Australopithecus*. The most convincing of these data are from isotopic studies of *A. africanus* and *A. robustus* teeth that show δ^{13}C enrichment, comparable to the isotopic signatures of grazing mammalian herbivores and carnivore consumers of grazing herbivores (e.g., Sponheimer et al., 1999; 2005; 2006; van der Merwe et al., 2003).[10] Also, possible traces of brucellosis, which might have been contracted "through contact with (or consumption of) infected tissues of other mammals, such as parturient discharges, fetal membranes or meat of young antelopes or other Ungulata," have been identified on a partial *A. africanus* skeleton from Sterkfontein Cave (South Africa) (D'Anastasio et al., 2009).

Combining these propositions leads to the construction of a simple hypothesis of basal hominin hunting. The hypothesis states that basal hominin hunting was conducted in seasonal savanna woodlands to offset regular shortfalls in plant protein availability. Because early hominins were not equipped somatically to open large animal carcasses or to slice away meat from bone or to break open marrow-bearing bones, and because there is no archaeological indication that they possessed extrasomatic tools to accomplish these tasks prior to 2.6 Ma, hunting likely concentrated on small mammals (perhaps predominantly immature animals), which were obtained and eaten by hominins using their hands and mouths and/or

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[9] See note 7.

[10] We note here that some researchers have suggested that the enriched δ^{13}C signatures in some sampled early hominins might be the result of the consumption of grass-eating termites, although Sponheimer et al. (2005) present relevant data on modern termites that renders this possibility extremely unlikely. In addition, Dominy et al. (2008) showed that some modern African hard-brittle corms and hard-tough tubers (foods with mechanical properties that match the craniodental specializations and inferred masticatory capabilities of *Australopithecus*) have elevated, "grass-like" δ^{13}C enrichment. Finally, van der Merwe et al. (2008) and Cerling et al. (2011) argue, based on stable carbon istopic data, that *A. boisei* was a specialized grass eater; a most surprising conclusion given the singularity of such results among the known, sampled early hominin species.

simple (presumably perishable) tools.[11] Scavenging opportunities were probably not ignored, but because of the unreliability of abandoned carcasses to provide flesh in savanna habitats, positing a "scavenging phase" (e.g., Blumenschine, 1986; Sayers and Lovejoy, 2008: 95) from which hominin hunting evolved is unrealistic.

Testing this hypothesis remains a challenge. It seems unlikely that perishable (wooden?) hunting and butchering tools would have imparted abundant surface modifications on small carcass remains, but this topic deserves more experimental investigation to confirm or reject this prediction.[12] Tooth mark analysis is an approach that seems to hold more potential for identifying Mio-Pliocene hominin meat-eaters, who rendered carcasses in the absence of and/or using only very rudimentary technology. Techniques for capturing tooth mark morphology analytically are still rudimentary, and thus taxonomic assignments for mark creators are necessarily gross, but there is promising progress in the direction of defining taxonomically diagnostic features of tooth pits and scores (see, e.g., Haynes, 1983; Selvaggio and Wilder, 2001; Domínguez-Rodrigo and Piqueras, 2003; Pickering et al., 2004); a hominoid/hominin-typical mark morphology and/or pattern of chewing damage might be within reach (see, e.g., Pickering and Wallis, 1997; Plummer and Stanford, 2000; Elkin and Modini, 2001; Pobiner et al., 2007; Landt, 2007; White and Toth, 2007; Martínez, 2009; Saladié, 2009; Fernández-Jalvo and Andrews, 2011). Another worthy actualistic pursuit is to document perimortem damage to small prey skeletons caused by the type of battering to death that characterizes much of chimpanzee hunting at sites other than Fongoli. One of us (TRP) is currently pursuing this type of research experimentally using raccoon (*Procyon lotor*) carcasses of roughly the same size as those of colobus monkeys.

Discussion

The model of early hominin hunting that we present here is necessarily general and certainly tentative at this stage. But, we emphasize again that it

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[11] Importantly, we do *not* hypothesize that early hominins, like chimpanzees in most places, most of the time, concentrated on hunting arboreal monkeys. In light of the radically dissimilar locomotor adaptations and relative strength and climbing capabilities of chimpanzees and those inferred for early hominins, to suggest so would be sloppy, formal analogizing.

[12] We do note that a few studies have been conducted on the use and taphonomic consequences of Asian bamboo as a cutting butchery tool (e.g., Spennemann, 1986, 1987; West and Louys, 2007) and that Smith (2003) reported on bone damage inflicted by untipped, experimental wooden spears, but we are unaware of any experimental work with simpler stick tools, comparable to the Fongoli spears and other known chimpanzee wooden technology (for reviews, see McGrew, 1992, 2004).

is *not* based on a simple trivial analogy drawn from chimpanzee research. Instead, it is a true explanatory (non-trivial) model (as differentiated from a descriptive [trivial] model; see, Harré, 1986; Aronson et al., 1995; Bunge, 2006) – or, in the vernacular of theoretical archaeology, a structural analogy (as opposed to a formal analogy; see, Gifford, 1981; Gifford-Gonzalez, 1991). Like less preferable descriptive models, explanatory models are also based on analogical reasoning, but contrary to intimations of some "conceptual" or "strategic" modelers, this is not a weakness of an explanatory model based on a referent. Indeed, some philosophers of science would contend that all scientific reasoning is based on analogy at some level or levels (e.g., Aronson et al., 1995; Holyoak and Thagard, 1995; Bunge, 2006). Moore (1996: 277) states the same sentiment, if a bit more cautiously: "Analogical or metaphorical models can be important ways of representing phenomena . . . To the extent that (some) people think metaphorically, [analogical] models of one sort or another are simply unavoidable . . . "

Importantly, though, this is not the same as claiming that all analogical models are equally valid. Testable explanatory models are stronger because, like the one presented here (and unlike a simple descriptive model), they are fashioned through the dialectic use of groups of linked, non-trivial analogies. We argue that in paleoanthropology this linkage is accomplished only through incorporation of two essential variables: ecological context and phylogenetic relationship. Relevantly, it is those two variables that also form the core of evolutionary behavioral ecology. It thus vexes us that the overt resistance of "strategic" modelers to chimpanzee-based models is presented, at least in part, as some kind of principled stand against the perceived theoretical naiveté of referential modelers. Good explanatory, chimpanzee-referent based models of human evolution incorporate the very evolutionary and ecological principles esteemed by "strategic" modelers. But, the creators of these referential models *also* recognize – and embrace – the fact that these principles are ultimately gleaned from data on specific biological taxa. In our opinion, that means an a priori bias against chimpanzees – our closest living relative, with whom we share a relatively recent MRCA and who are represented by some populations that inhabit ecosystems similar to that of Mio-Pliocene hominins – nonsensically diverts research focus from the very area to which it should be directed most intensely. In fact, we venture a step further. When some proponents of "strategic" modeling take pains to display "taxonomic objectivity" and cast widely by employing diverse referents residing in radically different ecologies and/or that are phylogenetically distant from the extinct study subject, in support of overarching evolutionary principles, they not only labor in defiance of pragmatism but also are apt to produce analogies that are trivial and substantively uninformative.

Acknowledgments

As should be apparent in this chapter but is also acknowledged explicitly here, TRP's view of chimpanzee (especially savanna chimpanzee) models of human evolution has been heavily influenced by Jim Moore. I am most grateful to Jim for the invitation to work at Ugalla in 1994 and for his (along with LSB Leakey Foundation's) financial support of that work; my brief time spent there has had a disproportionate impact on my thinking about early hominin behavior. We also thank Jim for his specific comments that improved this chapter. TRP thanks Craig Stanford, Bill McGrew, Jill Pruetz, and Janette Wallis for advice and suggestions (and patience) over the years regarding my proletarian dabbling in "chimpology." Finally, great thanks to TRP's family for their continued unconditional support.

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PART II

On the use of analogy II: The earliest stone toolmakers

CHAPTER 7

The origins of the Oldowan: Why chimpanzees (*Pan troglodytes*) still are good models for technological evolution in Africa

Susana Carvalho and William McGrew

To model or not to model?

A recent search of the internet using three key words, "chimpanzees model humans," yielded 27,200,200 pages of results, in less than half a second.

Recently, chimpanzees (*Pan troglodytes*) have been labeled as unsatisfactory models in understanding human evolution (Sayers and Lovejoy 2008; White et al. 2009). Highlighted differences between the anatomy of *Ardipithecus ramidus* (a hominin species living in East Africa at 4.4 Ma) and of the extant African apes suggest that the last common ancestor (LCA) of humans and African apes was morphologically more primitive than previously assumed (Pilbeam and Young 2004). This suggests that several primitive traits common to extant African apes and early hominins might have been a result of convergent adaptations rather than phylogeny (Stern and Orgogozo 2009). The reconstructed paleobiology of Ardi emphasizes behavioral (e.g., facultative bipedality) and morphological (e.g., reduced canine teeth) divergences from the extant chimpanzee. According to Lovejoy et al. (2010, p. 410), these conclusions "were based on intensive review of homologous traits in other primates," using a method known as *strategic modeling* (cf. Whiten et al. 2010). The aim of our paper is not to seek to prove that the chimpanzee is the best or only model for human evolution, compared with other living nonhuman species. Nor do we argue about the implications of vague behavioral terms, such as *facultative bipedalism* (e.g., how much bipedal locomotion does a facultative bipedal perform?), or *broadly omnivorous* (e.g., how much fruit needs to be in the diet for a species to be classed as a *ripe fruit specialist*, given that diets vary considerably with ecological context?), or *terrestrial and arboreal feeder* (e.g., what percentage of food must come from the ground versus above it?). Chimpanzees are insufficient models, if they are presented as the only useful species for reconstructing the hominin fossil record, nor are they time machines, whereby chimpanzee behavior precisely mimics LCA or hominin behavior. As we share various features with all primates (Fleagle 1998; Martin 1990), different research topics are better approached with a comparative approach, using pertinent species as models. Research that uses comparative primate modeling covers a wide range of human behavioral patterns, from tool use to contagious disease (Boyd and Silk 2009; Chapman et al. 2005). Nevertheless, recent publications that have direct implications for the understanding of the evolution of technological behavior continue to focus mostly on research of wild chimpanzee behavior as it pertains to tools. For example, hunting with spears (Pruetz and Bertolani 2007), digging up underground storage organs with sticks (Hernandez-Aguilar et al. 2007), sleeping in caves for thermoregulation (Pruetz 2007), using complex tool sets (Boesch et al. 2009), reusing tool composites (Carvalho et al. 2009), and revisiting areas of tool-use activity (Carvalho et al. 2008) and of sleeping sites (Hernandez-Aguilar 2009; see McGrew [2010] for an extensive review on relevant findings).

Shortly after modern research started in Africa in the 1960s, behavior once thought to be unique to humans (e.g., tool use and making) was reported for chimpanzees (Goodall 1963, 1964). More information accrued from long-term studies of chimpanzee populations in contrasting ecological settings strengthens the utility of chimpanzees as models for understanding human origins (McGrew 1992, 2004, 2010). The chimpanzee is the only species of living nonhuman primate that customarily uses and makes a wide variety of tools. Moreover, although some chimpanzee and hominin tools match in function (e.g., pounding tools), others seem to match in the modes of action inferred for early hominins (e.g., extraction, hunting). Several tools that chimpanzees make from perishable materials are suitable for activities that are part of the supposed behavioral repertoire of hominins, but the low durability of these raw materials makes them rare in the archeological record. Nevertheless, the few exceptions suggest that chimpanzee tool kits might have broad similarities with early hominins, regarding tool function or dietary components (Backwell and d'Errico 2001; d'Errico and Backwell 2003). To model hominin technology using chimpanzee research requires a holistic-interdisciplinary approach (McGrew 1992, 2004, Haslam et al. 2009). Single traits are not enough to characterize either a species or its technology. For example, chimpanzee technology could fit several features of different hominin industries. Regional diversity is a characteristic of

Middle Stone Age (MSA; e.g., Tryon et al. 2006), and tool composites are characteristic of the Later Stone Age (LSA; e.g., Khalidi 2009), but this does not mean that chimpanzee stone tools resemble MSA or LSA industries.

Archaeologists express an increasing interest in nonhuman primate tools and in seeking more comparisons of percussive technology (Goren Inbar 2002, Carbonell et al. 2009; de la Torre 2009; Toth and Schick 2009). Lack of firsthand knowledge can sometimes lead to inaccurate assumptions, however; chimpanzees modify the properties of some materials that they use to achieve new *capacities*, namely, functions (cutting is a function). For example, chimpanzees transform leaves into sponges to absorb water to drink (Sousa et al. 2009), modify stones through continual reuse (yielding wear patterns), and fracture tools that are later reused with a new function (e.g., fragment of anvil reused as hammer; Carvalho et al. 2008). Any theory presented as a scenario with an evolutionary bifurcation separates the capacity for using versus producing objects, maintaining the assumption that chimpanzees only *use* tools (here referring to stone tools only), whereas hominins show tool production as a derived behavior (Carbonell et al. 2009). Chimpanzees (but not bonobos, who do not use tools customarily in the wild) have a large repertoire of tool making and use sequentially complex and specialized tool sets after making these tools (Boesch et al. 2009; Sanz and Morgan 2009; Sanz et al. 2010).

The origins of the Oldowan: When, who, and how?

The Oldowan industrial complex comprises the earliest stone tool industries recognizable in the archaeological record (Leakey 1971; Isaac and Harris 1997; Semaw et al. 1997; Roche et al. 1999). Morphotypes of characteristic Oldowan assemblages include flaked pieces (e.g., chopper, polyhedron) detached pieces (e.g., whole or broken flake, flake and core fragment), retouched pieces (flake) and pounded pieces (e.g., hammerstone, anvil, battered piece) (Isaac and Harris 1997). This may seem to be a very limited tool kit, but it represents the first material evidence of the so-called cultural revolution: Man as tool maker (Oakley 1965). This core flake technology of multitask tools lasted for more than 1.5 Ma in the hominin repertoire and likely was efficient, advantageous, and adaptative (de la Torre and Mora 2009; Plummer et al. 2009). From a Darwinian perspective, it is reasonable to think of stone tools as advantageous for the survival and reproduction of earlier humans. Do Darwin's main postulates – struggle for existence, variation in fitness, and inheritance of variation – also extend to a technological scenario? That is, (1) The ability of technology to expand is infinite but the ability of any environment to support technology is finite; (2) Technology varies within populations, and this variation affects the ability of individuals to survive and reproduce; (3) This technological variation (abilities and techniques) is transmitted from parents to offspring (Darwin 1859).

In the understanding of the variables that might have influenced the first emergence of technology, it remains unclear *when*, *who* and *how* the use and production of stone tools emerged. Defining *intentionality* in technological behavior might also play a key role in this process through the use of archaeological data to detect and define the first technological transitions (and diagnostic features) regarding the (1) use of stone tools (e.g., use of natural stones with sharp edges); (2) use of stone tools produced unintentionally by other percussive techniques (e.g., use of a flake produced accidentally during nut cracking; reuse of a fractured anvil as an hammer); (3) intentional making and use of stone tools by nonknapping techniques (e.g., bashing, pounding); (4) making of stone tools through goal-oriented knapping. Except for point 4 and taking recent developments into account (e.g., Davidson and McGrew 2005; Marchant and McGrew 2005; Stout et al. 2008, 2010; Toth and Schick 2006, 2009; Schick and Toth 2009), answers to these questions are lacking still.

Oldowan: When? Following the discovery of the oldest stone tools at Gona, Ethiopia (Semaw et al. 1997), the Oldowan was redated back to c. 2.6 Ma. Now, the Oldowan may be backdated to the 3.39 Ma findings from Ethiopia based on bones with cut marks made by the sharp edges of stone tools (McPherron et al. 2010). Even in the absence of evidence directly linking stone tool use to fossil bones, we may infer from cut marks that hominins were using stone tools 700,000 years before the oldest Gona evidence. Lucy, who was semiarboreal, bipedal, and had a brain size similar to living great apes, was using tools to access carcasses of animals. Does Dikika offer the first indirect evidence of the Oldowan, or is it the oldest evidence of stone tool use before the Oldowan? The new findings may be evidence of tool use by hominins without the modification of raw materials, and therefore the ongoing debate on the significance of tool use *versus* tool making continues.

Oldowan: Who? When Louis Leakey worked in Olduvai Gorge, *Homo habilis* was the first fossil hominin to be recognized for the ability to make tools and so was named the “handy man” (Leakey 1964). Olduvai Gorge then had one of the few complete cultural sequences with continuous stratigraphy and reliable dates (Leakey 1975). The general idea was that each industrial complex correlated with one hominin species, and in the Oldowan case, with one part of the world, Africa. Almost forty years later, the evolutionary story of human technology is under major review: *Australopithecus afarensis* was using stone tools (McPherron et al. 2010), A. *garhi* was a candidate as a tool maker (de Heinzelin et al. 1999), and A. *aethiopicus*, A. *africanus*, A. *robustus* or A. *boisei* were all contemporaneous in the Early Stone Age, between 3.39 and 1.4 Ma (Delson et al. 2000). The genus *Homo* appeared at 2.3 Ma with *Homo rudolfensis*, followed by *habilis* and *ergaster* (plus early *erectus* in Eurasia). These later taxa, especially *Homo ergaster/erectus*, likely were more complex and successful tool makers than their ancestors (Whiten et al. 2009). As hominin fossils and novel taxa accumulate, paleoanthropologists must find new branches of the evolutionary tree to accommodate current findings, and archaeological assemblages need to be reinterpreted in a broader evolutionary context.

Oldowan: How? This earliest stone tool repertoire includes one particular category of artifacts, pounding tools. Archeologists catalogue them into utilized and modified materials (Leakey 1971), or pounded pieces (Isaac and Harris 1997) or unflaked pieces (Rogers 1997). Hammerstones and anvils are quasi-universal in the archaeological record (de Beaune 2004) and currently are still present in the tool inventory of some modern human populations, like the Manon and Guerzé of Guinea-Conakry (Biro et al. 2010a). Pounding tools were seen as the poor relations of lithic technology, because they were materials modified by use but not modified before use. Recently, a new research focus dedicated to the study of these hominin artifacts has emerged (Goren-Inbar 2002, de Beaune 2004; de la Torre 2004; Mora and de la Torre 2005). From these studies, we now know that (1) hominin nut-cracking sites exist in the archaeological record; (2) identifiable nut species were consumed; (3) accidental flaking during hammer-anvil percussion can be mistaken for intentional knapping; and (4) these pounding accidents could have "shown the way" to produce one stone object from another. In recent years, with the expansion of primatological studies, it has become evident that the earliest hominins and living nonhuman primates share at least one category of lithics, pounding tools, and this overlap could be an important link to understanding the origins of technology.

Nut cracking by wild chimpanzees: Bossou and West Africa

Nut cracking by wild chimpanzees has been reported since 1844 (Savage and Wyman 1844) and is exclusive to West African populations. Since 1976, systematic research in Bossou, Guinea, has been studying nut cracking continuously, both in natural and experimental conditions (at the so-called outdoor laboratory in Bossou forest; Matsuzawa 1994). The long-term study of nut cracking at Bossou has contributed important insights into our understanding of tool use in wild chimpanzees, mostly in the ontogeny of behavior, social learning processes, and transmission of traditions (Biro et al. 2003, 2006, 2010a). Matsuzawa revealed the hierarchical structures of behavior in tool use and described the long-term *master-apprenticeship* process that chimpanzees undergo to use stone tools proficiently to crack open nuts (Matsuzawa 2001, but see also Matsuzawa et al. 2011 for a full review of nut-cracking studies in Bossou). In 2006, Matsuzawa brought archeology into studies of stone tools, using an approach that focused on behavioral observations (Carvalho et al. 2008, 2009; Matsuzawa et al. 2011). The chimpanzees of Bossou offer a unique opportunity for investigating technological evolution, because they are the only population that customarily uses portable stones as hammers and anvils (Sugiyama and Koman 1979; Sugiyama 1997). Certain physical properties of objects constrain the basic requirements needed for technological innovation to occur (e.g., outcrops do not allow the use of a wedge; boulders are unlikely to fracture and produce by-products to be reused and transported; log or fallen tree trunk anvils will not yield serendipitous flakes that could be employed in a different task). Such factors could limit the rate of innovation in different populations of wild chimpanzees using stone tools (see van Schaik 2006 for rates of invention in different populations of orangutans). Conversely, much of the data from Taï Forest, the other chimpanzee field site where nut cracking has been studied long term (see Figure 7.1 for a summary of diversity in nut cracking at different sites) was published between the 1980s and early 1990s, when the Taï chimpanzees were not fully habituated (Boesch and Boesch 1983, 1984, 1990). This made the accurate behavioral recording of known individuals difficult, especially regarding observations of direct transport, learning processes, reuse of tools, or differences in site use.

The following is a summary of the known nut-cracking behavioral variety recorded in nine West Africa chimpanzee populations and charted in Figure 7.1 (based on the original diagram of Whiten et al. 1999):

Guinea – Bossou and **Diecké** forests (Sugiyama and Koman 1979; Carvalho et al. 2007): At Bossou, chimpanzees (*Pan troglodytes verus*) crack open oil-palm nuts *(Elaeis guineensis)* year round, but consumption peaks when other wild fruits are scarce (Yamakoshi 1998). This contrasts with Taï, where nuts are a staple food of chimpanzees, and 12% to 15% of their

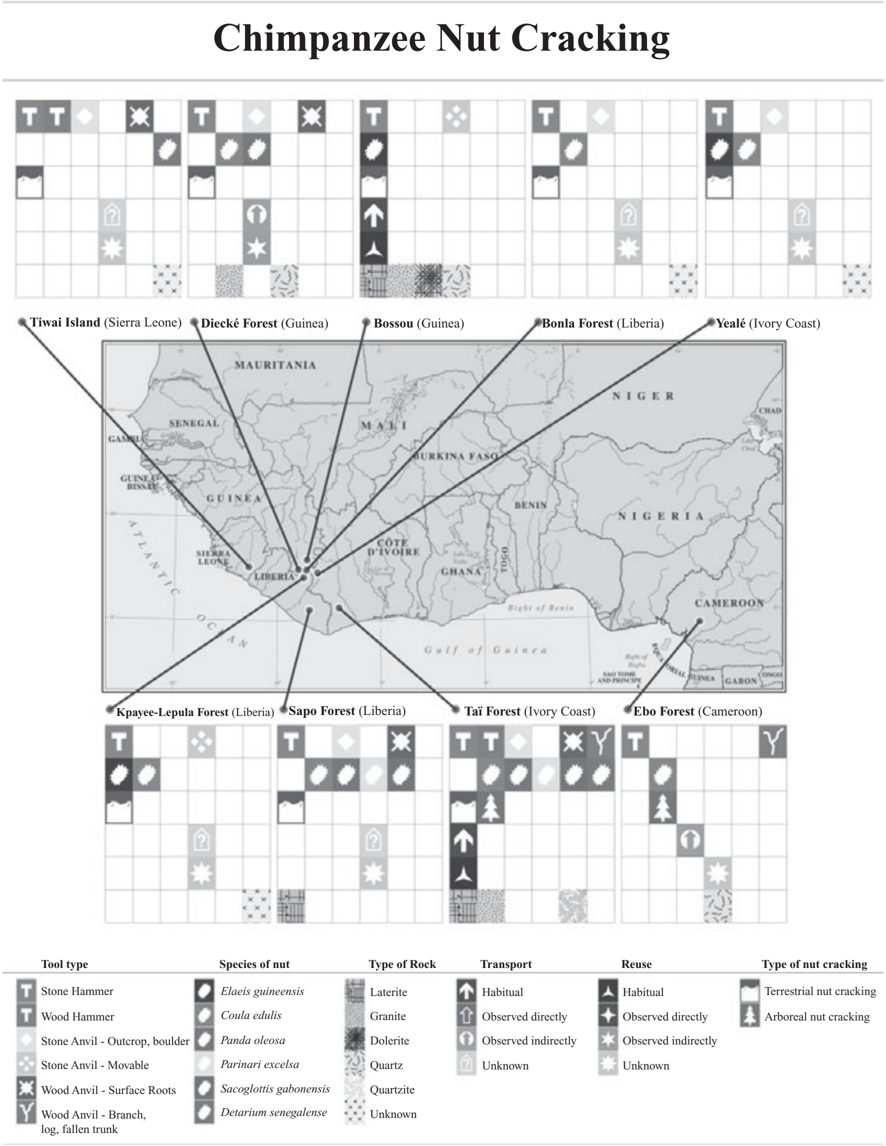

FIGURE 7.1. Diversity in nut cracking across the nine chimpanzee populations where this extractive technology was directly or indirectly recorded (design based on Whiten et al. 1999): 1 – Tiwai Island, Sierra Leone (Whitesides 1985); 2 – Diecké Forest, Guinea (Carvalho 2007); 3 – Bossou Forest, Guinea (Sugiyama and Koman 1979, Carvalho et al. 2008, Matsuzawa et al. 2011); 4 – Bonla Forest, Liberia (Ohashi 2011); 5 – Yealé (Nimba Mountains), Ivory Coast (Humle and Matsuzawa 2001); 6 – Kpayee-Lepula Forest, Liberia (Ohashi 2011); 7 – Sapo Forest, Liberia (Anderson et al. 1983); 8 – Taï Forest, Ivory Coast (Struhsaker and Hunkeler 1971, Boesch 1978); 9 – Ebo Forest, Cameroon (Morgan and Abwe 2006).

feeding time is spent cracking nuts (Yamakoshi 2001). Bossou's apes use movable stones as hammers and anvils and consume only one species of nut. They do not practice customary arboreal nut cracking, although it has been seen twice (by immature individuals, when the terrestrial nut-cracking site on the ground was crowded with adults, Matsuzawa et al. 2011). Occasionally, the chimpanzees use one or two wedge stones to stabilize and to level the anvil's working surface (Matsuzawa 1994; Carvalho et al. 2008). The available raw materials are laterite, granite, dolerite, and quartz. Transport and reuse of tools is seen to be customary. At **Diecké**, chimpanzees use movable stone hammers, stone outcrops, and on occasion root surfaces serve as anvils (Carvalho 2011a). No arboreal nut cracking has been seen. Two species of nuts are cracked: *Panda oleosa* and *Coula edulis*. Stone materials are mainly granite and quartz. Transport and reuse of tools was indirectly confirmed through monitoring known nut-cracking sites.

Ivory Coast – Taï Forest (Struhsaker and Hunkeler 1971; Boesch 1978) and **Yealé** (Humle and Matsuzawa 2001): Chimpanzees (*Pan troglodytes verus*) use movable hammers (wood or stone) and fixed anvils (97% are surface roots but also some embedded stones or tree branches; Boesch and Boesch 1983). They are never seen to transport anvils but customarily carry hammers between nut-cracking sites, often for more than 100 meters (Boesch and Boesch 1984). At Taï, chimpanzees do both terrestrial and arboreal nut cracking and consume five species of nuts (*Panda oleosa*, *Parinari excelsa*, *Sacoglottis gabonensis*, *Coula edulis*, *Detarium senegalense*). Stone hammers are of laterite, granite, or quartzite. At **Yealé**, chimpanzees use stone hammers and stone anvils (outcrop or boulder) to crack open *Coula edulis* and *Elaeis guineensis* nuts. This behavior has not yet been directly observed, however.

Liberia – Sapo (Anderson et al. 1983), **Bonla**, and **Kpayee-Lepula** forests (Ohashi 2010): At Sapo, forest chimpanzees (*Pan troglodytes verus*) use stone hammers to crack open four species of nuts (*Panda oleosa*, *Coula edulis*, *Parinari excelsa*, *Sacoglottis gabonensis*), using mainly (71%) stone anvils but also (29%) root anvils. As at Diecké, most of the stone anvils are embedded rocks. Nut-cracking sites are close to nut trees. Laterite hammers are reported, but the transport and reuse of the same tools have not been seen. At Bonla, chimpanzees use stone hammers and fixed stone anvils (outcrop or boulder) to get the kernels of *Coula edulis* nuts. Recent surveys reported remnant signs of terrestrial nut cracking. The Kpayee-Lepula forest revealed nut-cracking patterns identical to those of the Bonla forest, but chimpanzees there also consume *Elaeis guineensis* nuts.

Sierra Leone – Tiwai Island (Whitesides 1985): Chimpanzees (*Pan troglodytes verus*) seem to use both stone and root surfaces as fixed anvils, and stones and wooden clubs as hammers to open *Detarium senegalense* nuts. Only terrestrial nut cracking is mentioned. Nut-cracking sites are near nut trees, and transport is not suggested to occur.

Cameroon – Ebo Forest (Morgan and Abwe 2006): At Ebo Forest, the recent discovery of nut cracking in Cameroon challenges the *ecological frontier* model (i.e., absence of nut cracking east of the N'Zo-Sassandra River), based on riverine barriers to the cultural diffusion of nut-cracking behavior. Chimpanzees (*Pan troglodytes vellerosus*) were seen using stone hammers to crack open the nuts of *Coula edulis*. Only arboreal nut cracking was reported (at about 5 m to 8 m above the ground) with quartz hammers. *Pan troglodytes vellerosus* is one of the least-studied and most endangered subspecies of chimpanzee (Kormos et al. 2003). As reported by Wrangham (2006), their culture zone is now uncertain, but populations once living in between West and Central Africa might have gone extinct, severing the chain of transmission (Wrangham 2006).

Density of artifacts and probability distribution function in chimpanzee nut-cracking sites

Study site and methods

Previous studies (e.g., Panger et al. 2002) have attributed difficulties in recognizing nonhuman primate assemblages to the low density of artifacts in the archaeological record. The only previous archaeological study of an ancient chimpanzee nut-cracking site presented the density of artifacts, but with no horizon differentiation (Mercader et al. 2007). This chapter describes a preliminary finding that could allow comparison with archaeological data and contributes to defining a "signature" for chimpanzee nut cracking: local density of artifacts and the probability of finding tools when moving away from the food source.

The forest of Bossou (5–7 km^2 of core area) harbors one chimpanzee population (*Pan troglodytes verus*). From 1976 to 2004, the group maintained a stable composition of about twenty animals. Because of respiratory disease, the number of chimpanzees declined in 2004 (Biro et al. 2010b) and remained at thirteen or fourteen individuals during our study period (November 2008 to November 2009). The data presented here were collected through monitoring of natural nut-cracking sites used by the chimpanzees. Archaeological methods and equipment had to take in account two unusual variables: (1) areas of interest to be studied were still in use; that is, not abandoned; (2) *tool users* came and went from the site during data collection. An earlier archaeological reconnaissance of the forested area allowed us to characterize the landscape and to recognize that the proximity of oil-palm trees increases the probability of finding nut-cracking-sites. To have a *representative sample*, we selected nut-cracking sites from different hills around Bossou, at different altitudes and presenting tools of different raw materials. These selected nut-cracking sites were intensively surveyed to check the tools' spatial distribution (see Carvalho 2010b for details on survey methods). For this survey, we selected seventeen nut-cracking sites for monitoring of tool movements. The monitoring of nut-cracking sites was based on nonrandom sampling, in which the researcher selects units of study based on the potential productivity of the chosen areas. Before recording each site, a radiocentric survey was done in the nut-cracking area around the oil palm tree (c. 15-m radius) and in the surrounding forest a (c. 30-m radius) to search for tools. This method allowed confirmation of transport of new tools to the site area. During each monitoring visit, we noted alterations in the position and orientation of the archaeological materials and drew plain views. Because the chimpanzees using these sites could appear any time, our method uses a minimum of equipment, so that we can leave the site as soon as an ape appears. Therefore, the topographic triangulation technique was applied based on two fixed points: magnetic North and the site datum point (nut-bearing tree). A sequential number, a function indicator, and an area marker were assigned, with permanent ink, to all the detected tools (e.g., 2 H M = Hammer number two from Mobli).

Direct observation of nut cracking and the identification of the individuals using tools were recorded at all seventeen nut-cracking sites. The chimpanzees used a total of 201 tools. To maintain independence of data points, only the first spatial distribution of each of the 201 tools was used, and tool reuses were excluded. A nut-cracking site at Bossou was a defined area with one clear accumulation of hammers and anvils focused on the food source, the oil-palm tree. Monitored sites were always separated by distances ranging from about 20 m to about 2 km, with only one palm tree at each site. (Other authors used different units for defining a nut-cracking site, for example, each combination of anvil with nutshells was considered one atelier, Boesch and Boesch 1983). During surveys, we recorded tools around each oil-palm tree, at distances of 0-m to 15-m radius. Therefore, we considered the approximate area of each nut-cracking site in Bossou to be 707 m^2 (3.1416[15]^2). Mean density of tools per site was calculated by dividing the nut-cracking area by the number of tools found. To see if there were differences in hammer and anvil distribution, density was first calculated for all tools present at a site, and then for hammers and anvils separately. To investigate differences in matched hammer and anvil pairs, we used a paired t-test. The probability of finding stone tools (hammers or anvils) when moving away from the food source toward the external (peripheral) limit of the nut-cracking site was calculated using the probability distribution function. To create the histogram, a program was written in Fortran 90 (see later). Intervals of the nut-cracking circumference (Bin width) were set at 0.5 m. Variables were the number of sites ($N = 17$), number of tools ($N = 201$), tool distance from the oil-palm tree and number of bins (30 bins with 0.5 m width = 15 m radius), that is (Formula:h(i)/(dble(sumtools)*binwidth)

h(i) = number of times that tools were found in bin (i)
sumtools = total number of tools
binwidth = r/number of bins

The probability distribution was calculated first for the total number of tools at site (regardless of function) and then for hammers and anvils separately. Data were analyzed using XMGRACE and SPSS17.0.

Results

Figure 7.2 shows the density of tools (hammers and anvils) per nut-cracking site in Bossou. The density of tools per square meter was very low, ranging from 0.002 (numbers 1, 7, 11) to 0.04 (number 6), and 0.05 (number 12). Thirteen sites presented densities below 0.02 (76%), but the density of hammers and anvils did not differ across nut-cracking sites ($N = 17$): $t(16) = -0.442$, $p = 0.63$ (2-tailed).

Figure 7.3 shows the probability of finding hammers and anvils in relation to the distance from the oil-palm tree. The probability of finding any tool gradually decreased as distance from the palm tree increased. Anvils were more likely to be found closer to the tree (1 m–2 m), whereas hammers had a wider distribution (1 m–10 m). Beyond 10 m, it was rare to find tools. Overall, Figure 7.3 shows a regular distribution in the seventeen sites.

These results support predictions by Panger et al. (2002) and Haslam et al. (2009) that chimpanzee sites have very low densities of artifacts (Figure 7.2), which makes an assemblage hard to identify in archaeological surveys.

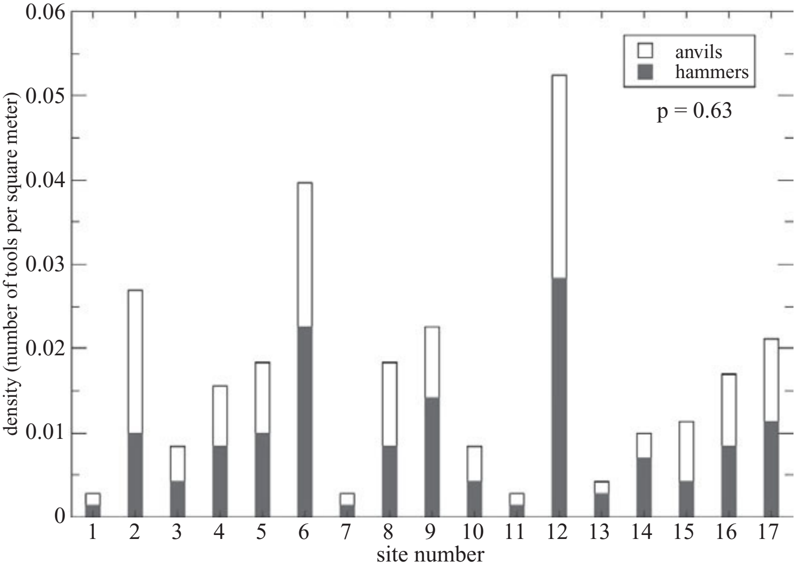

FIGURE 7.2. Density (number of tools per m^2) of anvil and hammer tools across seventeen nut-cracking sites at Bossou (Guinea).

We cannot say now whether these results differ from the earliest Oldowan sites or excavated nut-cracking sites. Few published data allow exact comparison (Potts 1991, Plummer 2004), because the density of artifacts per site is not published, is published with no reference to the excavated area, or is published in a format that does not reveal exact densities of different categories of artifacts from a given period (i.e., densities presented for the total excavated volumes, without separation by horizons). Isaac (1989: 211) estimated that "the minimum density of objects per unit area would be less than one piece per 10,000 m^2 and superimposed on this background scatter it is common to find patches where objects occur in much higher densities (e.g., 1–100 per m^2). It is these patched or concentrations of materials that are commonly called 'sites' and archaeological attention has been focused almost exclusively upon them." Plummer's (2004) review has updated data that allowed us to calculate the mean density of artifacts in relation to the total area excavated for twenty-three of the earliest Oldowan sites, dated between 2.6 Ma and 1.75 Ma (Figure 7.4). (Of course, the excavated areas are but a tiny fraction of the areas used by the hominins). Figure 7.4 appears to mirror the high variability that has been reported to exist between sites.

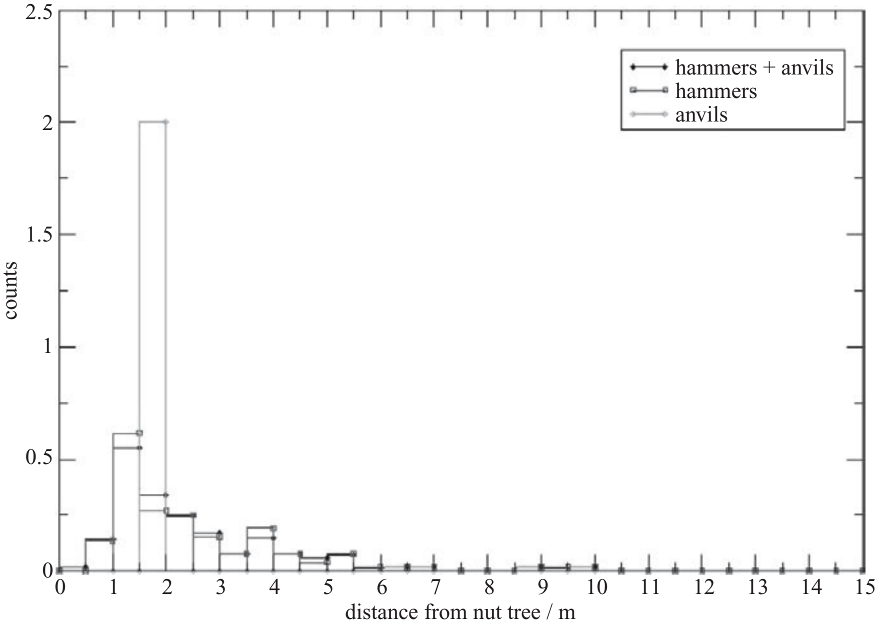

FIGURE 7.3. Probability distribution of anvils alone, hammers alone, and anvils and hammers together at varying distances from the nut tree at seventeen sites combined.

It would be useful to do this comparison for hominin, chimpanzee, and capuchin sites using data for hammerstones and anvils.

Discussion

Even if the chimpanzee signature is a hard one to recognize, these preliminary data show that one can quantify and measure these variables. In future, it may be feasible not only to identify a chimpanzee nut-cracking site via excavation but also to refine the reconstitution of the used sites to the point of predicting mean distances from tools to food sources. Chimpanzee nut-cracking sites are close to nut-cracking trees or in them (Boesch and Boesch 1983, Mercader et al. 2002, Morgan and Abwe 2006, Carvalho et al. 2008) Even if, during archaeological excavation, the food source cannot be identified, it is possible to predict this distance from the assemblage. Further data are necessary (e.g., Diecké and Taï forests) to compare with Bossou, to see how much if at all these distances vary across ecological settings. Previous studies have shown that chimpanzees reuse tools over long periods (months, years; Carvalho unpublished data, n.d.) and have tool-composite preferences at the individual and population levels (Carvalho et al. 2009). If reusing and having preferences for particular tools or composites are characteristic, then further questions can be raised by these results: Can the density of stone tools in a given territory be related to group size in chimpanzees? Does a smaller group have an equivalently small number of stone tools that are reused within their home range? In the case of Bossou, where we find the anvil we normally find the hammer. This tight fit also could reflect the abundance of raw material available, that is, the luxury of having enough material to have a "resident" tool composite in every activity area and thus removing the cost of transporting tools between sites. In Taï, many "ateliers" lack hammers, and the forest lacks movable stones (Boesch and Boesch 1984); chimpanzees therefore transport stones for up to 500 m from one nut-cracking site to another. Bossou chimpanzees often transport tools inside the activity areas, but the rate of such transport between sites is unknown and is part of an ongoing study. In the archaeological record, some sites lack hammer stones in the assemblages or have more anvils than hammer stones (Isaac and Harris 1997).

When the "manifesto" of primate archaeology was published (Haslam et al. 2009), convergence was pinpointed as a key aspect of technological evolution. Technological convergence might explain the differing technologies of various species of primates (Panger et al. 2002), but it also could occur within a species. This suggests that convergence occurs on different scales. In light of recent findings (McPherron et al. 2010), multiple inventions and extinctions of technological behavioral patterns throughout space and time might parsimoniously explain the technological variability detected in some of the oldest contemporary assemblages from Gona, or Lokalalei (Semaw et al. 2003; Delagnes and Roche 2005).

Previous examples of this discipline, studying nonhuman primate tools, include the application of the theoretical concept of operational sequences to chimpanzee nut cracking, addressing the questions of tool function and regional diversity in stone tool use (Carvalho et al. 2008, 2009). In fact, the study of current communities of wild chimpanzees using different types of tools to crack open a variety of nut species reveals technological and typological diversity in these contemporary assemblages (Boesch and Boesch 1990; Carvalho et al. 2008).

In summary, this case study further exemplifies how primate archaeology is a useful tool to better understand the emergence of technology in human and nonhuman primates, if incorporating interdisciplinary research from firsthand knowledge in archaeology-paleoanthropology and

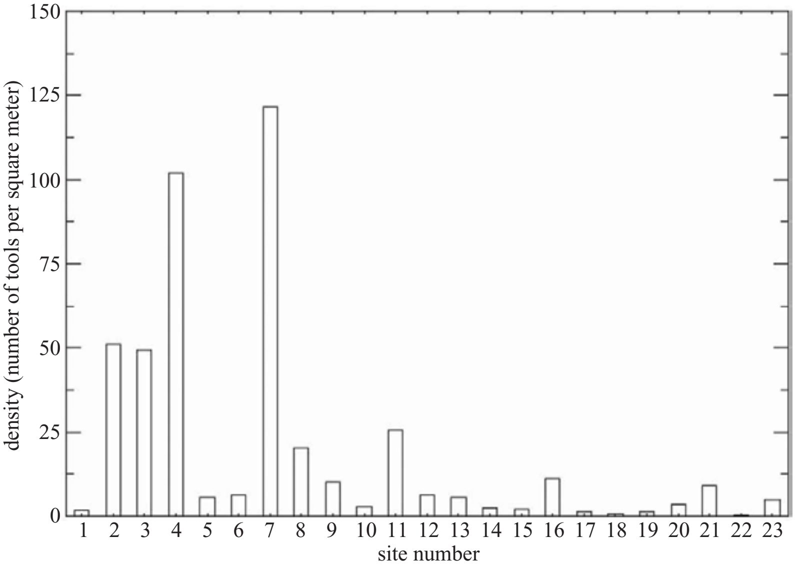

FIGURE 7.4. Density (number of tools per m^2) of excavated artifacts across twenty-three Oldowan sites. Only sites with data for total excavated area and total number of artifacts were included; data are presented with no horizon or category separation. 1 – West Gona 1, Ethiopia (2.4Ma); 2 – EG10 Gona, Ethiopia (2.5–2.6Ma); 3 – EG12 Gona, Ethiopia (2.5–2.6Ma); 4 – OGS7 Gona, Ethiopia (ca. 2.6Ma); 5 – AL666 Hadar, Ethiopia (c. 2.33Ma); 6 – Lokalalei 1, West Turkana, Kenya (2.34Ma); 7 – Lokalalei 2C, West Turkana, Kenya (2.34Ma); 8 – FtJi 1 Omo, Ethiopia (2.3–2.4Ma); 9 – FtJi 2 Omo, Ethiopia (2.3–2.4Ma); 10 – FtJi 5 Omo, Ethiopia (2.3–2.4Ma); 11 – Excavation 1 Kanjera, Kenya (ca. 2.0Ma); 12 – FxJj 82 Koobi Fora, Kenya (ca. 1.77Ma); 13 – FxJj 10 Koobi Fora, Kenya (1.9Ma); 14 – FxJj 3 Koobi Fora, Kenya (1.9Ma); 15 – FxJj 1Koobi Fora, Kenya (1.9Ma); 16 – FLK N 1–2 Olduvai Gorge, Tanzania (1.75Ma); 17 – FLK N 3 Olduvai Gorge, Tanzania (1.75Ma); 18 – FLK N 4 Olduvai Gorge, Tanzania (1.75Ma); 19 – FLK N 5 Olduvai Gorge, Tanzania (1.75Ma); 20 – FLK N 6 Olduvai Gorge, Tanzania (1.75Ma); 21 – FLK 1 22 Olduvai Gorge, Tanzania (1.76Ma); 22 – FLK NN 3 Olduvai Gorge, Tanzania (1.76Ma); 23 – DK 2and3 Olduvai Gorge, Tanzania (1.86Ma). (Data from Plummer 2004).

primatology-ethology. Hominin tools were more than flaked tools, just as chimpanzee tools are more than stone tools used to crack open nuts.

Acknowledgments

We thank the Direction National de la Recherche Scientifique et Technique, République de Guinée, for permission to conduct field work at Bossou. The research was supported by Grants-in-Aid for scientific research from the Ministry of Education, Science, Sports, and Culture of Japan: MEXT-16002001, JSPS-HOPE, JSPS-gCOE-Biodiversity(A06), and F-06–61 of the Ministry of Environment, Japan, to Tetsuro Matsuzawa. We thank B. Zogbila, H. Camara, J. Doré, P. Goumy, M. Doré, P. Cherif, J. M. Kolié, J. Malamu, L. Tokpa, A. Kbokmo, Cé Koti, O. Mamy, and F. Mamy for essential field support. SC thanks M. Domínguez-Rodrigo for the kind invitation to contribute to this book. We thank K. Hockings and V. Carvalho for helpful comments on the manuscript. We are extremely grateful to V. O. Batista, D. Biro, P. Fuentez and P. Gonçalves for essential research assistance. SC is supported by a Cambridge European Trust (RIB00107), Fundação para a Ciência e Tecnologia (SFRH/BD/36169/2007), and The Wenner-Gren Foundation; WCM is supported by the National Science Foundation, Researching Hominid Origins Initiative, grant to T.D. White and F.C. Howell.

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CHAPTER 8

What does Oldowan technology represent in terms of hominin behavior?

David R. Braun

For the past 2.6 million years, humans and their ancestors have left behind traces of their behavior that record the variation in biological and cultural changes throughout our evolutionary history. Most of our understanding of the behavior of hominins is derived from evidence from stone artifacts. The study of Early Stone Age stone artifacts has a long history that is intimately intertwined with evolving ideas of what material culture represents and the ways in which this information can be interpreted. This has included a generalized focus on cultural historical changes through to more process-oriented approaches to ancient behavior. The last decade of research on the Oldowan has opened analysts' eyes to greater diversity of behaviors expressed in the Oldowan. Unfortunately there is not a lot of consensus about how the evidence informs our perception of hominin behavior (De la Torre and Mora 2009). Here we will discuss various aspects of artifact manufacture and use in the early Pleistocene. We will investigate the current debates regarding the behavioral meaning behind stone artifact manufacture and review current approaches. In particular, this chapter reviews the current dual approaches to the study of the earliest archaeological traces. We describe the strengths and weaknesses of these perspectives and consider how it might be possible to combine these techniques in a way that provides a greater understanding of the earliest toolmakers.

Most scientist currently studying Oldowan behavior agree that the cultural historical approaches of the 1970s focused too heavily on description and did not include enough explanation of variation. Attempts to simplify these descriptions to allow for greater comparison across assemblages (Isaac 1977; Toth 1985) has provided some insights to behavioral adaptations associated with early hominin behavior. Some have suggested that these simplifications overlook the detailed aspects of artifact production, however, and therefore are not useful indicators of the full scope of technological behaviors that Oldowan hominins possessed (De la Torre and Mora 2009). Investigations of stone artifacts cover a broad spectrum from formal analogies between tool forms and psychological states (Wynn and McGrew 1989) through to extremely detailed investigations of fracture mechanics that might have very little to do with hominin behavior (Braun et al. 2009b). It is vital that Oldowan archaeologists begin to approach tool use in the Pleistocene in ways that are firmly linked to the empirical data but yet still applicable to higher-order inferences (Torrence 1989).

Different methodologies for the analysis of Oldowan stone artifacts have paralleled competing ideological approaches to the study of the earliest material culture. These different strains of thought are mirrored by similar ideological differences in the study of stone artifacts in general (Geneste et al. 1990; Shott 2003; Van Peer and Bar-Yosef 2009). Some have even suggested that the two approaches are so diametrically opposed as to completely prevent comparison between data sets derived using different methodologies. This has resulted in somewhat of a stalemate in Oldowan studies. There are many debates regarding what Oldowan tools mean for the behavior of their makers; however, none of these debates are likely to be settled given the current framework of investigation.

What do we want to know about Oldowan behavior? Why should anyone care?

Although there are certainly disputes among Oldowan archaeologists, all would most certainly agree that a unifying basis behind studies of these early industries is insight into the evolution of our earliest ancestors. Almost all studies of Pleistocene technology use models of evolutionary change as a basic framework (de la Torre et al. 2003; Delagnes and Roche 2005; Kimura 1999). At a very basic level, we want to know why early hominins started making stone tools. We also want to know if the variation that we see in the archaeological record represents adaptive shifts that were important in the evolution of our ancestors. For these questions to be answered we must accept the possibility of a null model in which evolution plays a very small role in Oldowan stone artifact manufacture. Although the null model is a plausible hypothesis, it seems unlikely that hominins invested time and energy in the development of a complex series of behaviors that had no impact on their eventual genetic fitness.

Thus, if we begin with an assumption that the artifacts found in these early sites do represent evolutionarily significant adaptations of our earliest ancestors, then perhaps we can investigate the evolutionary pressures that shape Oldowan behavior. Unfortunately, different schools of thought diverge. Some approaches to the study of the Oldowan conceptualize artifacts as part of a subsistence system that requires stone artifacts for the acquisition of resources (Blumenschine et al. 2008; Tactikos 2005). These approaches often invoke environmental variation to explain differences. In many behavioral ecological approaches, the evolution of behaviors is in some way related to the capabilities of hominins to access certain resources (Stout et al. 2010). Artifacts become an extra-somatic parallel to biological evolution. Natural selection controls certain behaviors that guide the decisions that hominins make about when and how to manufacture artifacts and when to discard them (Kelly 1988, 1995; Nelson 1991). The exact method of how natural selection effects these decisions is not well understood; however, the details of this are beginning to be addressed (Mesoudi and Lycett 2009; Shennan 2009). This approach seems to oversimplify the forces that shape cultural variation if artifacts are viewed as purely a phenotypic expression of hominin genetic predispositions. Hominins that produced Oldowan technology were not driven only by requirements to produce sharp edges from cobbles. Studies of Oldowan behaviors could be enhanced when they integrate the social framework with which all tool use is associated (Stout 2002). Indeed, it is exactly this social framework that is the basis behind approaches to the Oldowan that are usually associated with Leroi-Gourhan's emphasis on technical actions (Leroi-Gourhan 1964). These models incorporate mechanisms of cognition as well as transmission of skilled behaviors, and thus they are directly applicable to models of cultural evolution that emphasize transmission processes (Mesoudi et al. 2006). Followers of this approach envision natural selection acting on the details of individual cognition and the role that this plays in a social context (de la Torre et al. 2003). This perspective does not disregard the ecological and economic constraints but sees the cultural aspects of technical production as equally as important. By isolating individual variation as the smallest unit of analysis, this approach has many advantages over behavioral ecological approaches. Behavioral ecological approaches largely investigate patterns at the assemblage level and rarely investigate patterns at the level of the individual knapper. Investigation of evolutionary mechanisms, using techniques often described as *chaîne opératoire*, have equal difficulties, however. This perspective places an emphasis on understanding the intentions of the knapper. It is obviously impossible to independently test whether these intentions were correctly identified. In fact, the *chaîne opératoire* method often directly rejects the concept of hypothesis testing and instead depends on the details of the collection to direct the analysis (Pelegrin et al. 1988). The focus of these socially bound models is on the cognitive requirements of different knapping methods. These are usually expressed in poorly defined terms of elaboration, degrees of utilization, and acquisition of techniques (Roche 2005). The direct application of these concepts to models of biological evolution is very difficult (how do you define when a technique is "elaborate" enough to affect the genetic fitness of an individual person?).

Although an evolutionary background guides both of these approaches, the pragmatic application of these methods is less straightforward. We still have not answered the question of why the study of human evolution as a whole should be concerned with these different methods. It is increasingly apparent that the unique condition of humankind is directly linked to our social mechanisms and cultural capabilities. In that sense, material culture records a series of cultural adaptations that humans have made over the course of the last 2.6 million years. These behavioral adaptations have modified the context of our biological adaptations. They have done this through a manner in which selective environments of hominins are modified by the behaviors of the hominins themselves (Laland and O'Brien 2010). The most obvious case is the use of stone tools to gain access to resources that were otherwise inaccessible. The best evidence we have of this comes from the zooarchaeological record (de Heinzelin et al. 1999; Domínguez-Rodrigo et al. 2005). Access to key resources like fat and protein were clearly important factors in the biological evolution of our species. Current narratives of the evolution of the genus *Homo* often point to these adaptations as vital to the expansion of the brain and expanded social networks (Reader and Laland 2002; Wood and Collard 1999). We may need to rethink the relationship between brain size and tool use, based on new evidence suggesting that smaller-brained hominins used tools and had access to higher-quality dietary resources (Brown et al. 2004; McPherron et al. 2010). We clearly need further research on this topic, considering the vociferous debate surrounding these issues (Domínguez-Rodrigo et al. 2010). More quantitative methods of identifying the earliest tool use are necessary. However, the general pattern throughout the Pleistocene is one of increasing dependence on stone tools for the acquisition of key resources (Ambrose 2001). Although tools clearly played some role in the subsistence adaptations of Oldowan hominins, the possibility certainly exists that tool production played some role in the selective pressures acting on cognitive evolution of Oldowan hominins. Although stone tool production in the Oldowan seems relatively simple, it involves a complex series of behaviors (e.g., collection and transport of appropriate materials, reduction that is guided by technical rules) that may have placed selective pressures on hominin cognition. Determining exactly which aspects of the archaeological record testify to the advanced cognitive capabilities will be difficult (McPherron 2000). It might be possible to understand these cognitive capabilities by investigating different methods of learning and social transmission in these early technologies (Nonaka et al. 2010; Whiten et al. 2009).

What do we know about Oldowan hominins' behavior?

1. Oldowan hominins selected stones based on their mechanical properties.

There is an increasing body of evidence that hominin selection of stone for artifact manufacture included a detailed understanding of fracture mechanics. The earliest evidence of stone manufacture comes from the Gona sites in Afar region of Ethiopia. The earliest description of these excavated materials remarked at how the specimens appeared to have many of the hallmarks usually associated with later time frames. Flakes and cores showed multiple series of small removals, and platforms were relatively thin (Semaw et al. 1997). Another interesting aspect of these materials was the fact that they were made on very fine-grained rock types, including rhyolites and cherts (Semaw 2000). Recent analysis shows that these materials are not actually very abundant in the landscapes where the Gona hominins lived. Stout and colleagues have shown that the Gona hominins specifically selected these fine-grained materials to make stone artifacts (Stout et al. 2005). In fact, the selection bias of hominins appears to be directly related to the grain size of the materials. Hominins therefore were not selecting particular types of rocks but were actually selecting individual stones based on the grain size of these materials. This means that at the very dawn of artifact manufacture, hominins understood that fracture could be more easily controlled in rocks that were made of smaller crystal sizes (Brantingham et al. 2000). Further evidence from the West Turkana materials suggested that hominins recognized the lithological features of stone that were important for artifact manufacture. Hominins also used this information to guide decisions about artifact manufacture (Harmand 2009). These decisions can be seen in the differences in reduction techniques seen at Lokalalei 1 and 2C. Hominins used simple flaking methods to reduce stones that were made on more intractable materials. This was especially the case on stones that could be found only as round cobbles (Delagnes and Roche 2005). These behaviors appear to vary across sites, however. Hominins at the site of Kanjera South used a selection process that specifically identified certain features of stone (Braun et al. 2009b). Studies of the material properties of these stones suggest that hominins specifically selected stones that had durable edges. Although most of the rock types that were selected for artifact manufacture were also fine grained, some were not. For example, medium-grained granites were difficult to knap, yet hominins from Kanjera selected these materials for manufacture. These stones were not available near the Kanjera site.

2. *Hominins transported stones around the landscape.*

The earliest studies of the Olduvai and Koobi Fora material emphasized description of the variation in Oldowan assemblages (Isaac 1972; Leakey 1971); however, subsequent studies of these collections that included an experimental referential framework determined that transport of stone was a key feature of Oldowan behavior (Potts 1991; Toth 1987). The most detailed description of this was based on relatively short refit sequences from Koobi Fora. These showed that hominins arrived at archaeological sites with cores that had already been flaked, and most likely they left sites with flakes and possibly with cores they had collected from these sites (Schick 1987). These studies allow us to understand an Oldowan technological organization whereby stones are routinely transported around the landscape. Admittedly, many of the earliest Oldowan industries are found within meters of conglomerates (Goldman and Hovers 2009; Harmand 2009; Stout et al. 2005). Previous models had actually suggested that the early phases of the Oldowan represented a period when hominins were tethered to resources of stone (Rogers et al. 1994). This hypothesis has been mostly substantiated by evidence from many of the earliest sites that have shown that relatively early Oldowan sites are always found near conglomerates (Braun et al. 2010; Kimbel et al. 1996). New data from various sites now suggest that hominins even as early as 2 million years ago transported stone over relatively long distances. Data from the Kanjera South site shows that hominins transported 28% of the assemblage over distances that were greater than 10 km. Furthermore, it appears that hominins chose to transport specific materials based on their mechanical properties (see earlier discussion). Later there is extensive evidence that hominins transported stone over greater distances. At Olduvai, raw materials were transported at distances similar to that seen at Kanjera (Hay 1976). Provenience studies in the Koobi Fora Formation suggest that transport of stone over 25 km took place at least periodically (Braun et al. 2008a). Transport patterns of certain aspects of the Oldowan record are well documented later. At Olduvai the transport of materials was guided by the availability of stone raw material as well as other factors, such as carnivore competition (Blumenschine et al. 2008). Rogers' (Rogers 1996; Rogers and Harris 1992) comprehensive landscape study of the Okote member materials described a detailed account of the means by which hominins transported materials from raw material sources to proximal floodplain contexts. This pattern was further supported by a later investigation of large scraper forms (Braun et al. 2008c). This study showed that not only did hominins consistently transport materials into floodplain contexts but also demonstrated that the largest scraper forms were systematically chosen for transport to those contexts where raw material was scarce. This suggests that hominins did not just collect and discard materials at random; instead it appears that hominins had at least a rudimentary understanding of the arrangement of resources around their landscape. This understanding guided decisions about transport and discard.

3. *Hominin artifact manufacture was guided by specific technical rules.*

The classic description of Oldowan behavior is that it is a "least-effort solution to a sharp edge"(Isaac and Harris 1997). This description still stands as a basic tenet of Oldowan tool manufacture. Oldowan hominins were likely trying to create sharp edges in a way that cost them the least amount of energy and time; however, there does appear to be substantial variation within this conceptual framework. Sometimes the costs of transport or replenishment of raw material sources was high enough to modify the way in which hominins made stone artifacts. The maintenance of striking platform surfaces might have prevented hominins from having to procure further resources. These technical rules could have guided Oldowan tool manufacture. Some of these technical rules do not appear to have been constant throughout East Africa. Much variation exists in the types of technical rules that were adopted by Oldowan hominins, but some have been well documented:

A) *Angles less than 90 degrees are exploited when available.* This is a general rule of fracture mechanics and has been documented in replicative flintknapping (Pelegrin and Texier 2004) as well as controlled experimentation (Dibble and Rezek 2009). Although hominins did not always follow this pattern (Delagnes and Roche 2005; Sahnouni et al. 1997), it is a generalized feature of most knapping techniques.

When hominins did not comply with this rule, they usually shattered the striking surface or produced multiple step fractures that prevented further reduction. This technical rule is rarely violated. Some manufacturing techniques apparently require the flaking of cores at very high platform angles (>90 degrees; e.g., polyhedrons; [Roche 2005]). As a corollary to this rule, stones that do not exhibit acute angles are rarely selected for artifact manufacture (Harmand 2009). In fact, rounded cobbles and large blocks are often split into pieces before they are flaked to maximize the number of acute angles (Toth 1997).

B) *Removals are usually produced in groups that usually have parallel or subparallel technological axes*. This pattern is not exclusive to Oldowan flaking, but it does appear to occur in numerous instances of the Oldowan. This technical rule usually results in removals being grouped in multiple *series*. These series all have similar platform attributes and often have similar dorsal scar patterns (Delagnes and Roche 2005). This is documented in the Gona localities, which have relatively low levels of centripetal flaking (Stout et al. 2010). Refit sets from the Lokalalei site document the use of multiple series of removals that allowed for the maintenance of a flat flaking surface throughout the reduction of the cores (Delagnes and Roche 2005). It appears as if hominins in the Oldowan usually exploited a flaking surface that had acute angles until that part of the core was no longer viable. In highly reduced cores this technical rule usually breaks down. Heavily exhausted cores at Gona, Lokalalei, and Kanjera all show the production of multifacial removals with the alternative use of multiple flaking surfaces (Braun et al. 2009a; Delagnes and Roche 2005; Stout et al. 2010).

Other technical rules that appear to have been followed at early Oldowan sites include the use of opposed platforms to maintain flat flaking surfaces at Lokalalei 1 (Braun et al. 2009a; Delagnes and Roche 2005). At Gona there appears to be an emphasis on the removal of very large flakes early in the reduction sequence to maximize the cutting edges produced from relatively small cobbles (Stout et al. 2010). Many of these rules were focused on increasing the productivity of flake removals. Techniques for increasing productivity have been documented in the Koobi Fora sites as well (Braun and Harris 2003). At later sites, systematic technical rules appear to be associated with specific reduction strategies. Examples from Peninj show that hominins followed technical rules that divided cores into production surfaces and preparation surfaces (centripetal hierarchical reduction technique); de la Torre et al. 2003; Texier 1995). In the Developed Oldowan of Koobi Fora Formation, the production of single platform cores followed technical rules that limited the number of striking surfaces (Ludwig and Harris 1998). In the Omo collections, the use of bipolar flaking was common in small cores, although unidirectional unifacial flaking was also prevalent (de la Torre 2004). Later there is also the appearance of specific techniques of shaping that can be seen in the appearance of true polyhedrons (Roche 2000). Although these technical rules did exist, there were also apparently constraints that many early hominins could not overcome. For example, Lokalalei hominins could not produce new platform surfaces after knapping mistakes that rendered flaking surfaces unusable (Delagnes and Roche 2005).

What don't we know about Oldowan artifacts?

The list of things that we do not know about Oldowan artifacts is depressingly large. Currently the use of Oldowan artifacts for specific tasks has not been conclusively documented. Previous microwear studies have suggested that these tools were used for butchery and woodworking activities (Keeley and Toth 1981). New microwear studies have suggested that hominins might have used stone tools to process tubers (Lemorini et al. 2009). Further experimental studies have suggested that the flakes that were broken off cores were actually the most useful tools for a variety of tasks that we assume hominins engaged in (Toth 1987). Later there is some evidence that hominins engaged in particular woodworking activities (Domínguez-Rodrigo et al. 2001). The best evidence of hominin tool use is derived from bone surface modifications. Although these marks on the surfaces of bones that record the evidence of butchery activities have been studied for almost thirty years, there is still much debate over their behavioral meaning (Blumenschine et al. 2007; Domínguez-Rodrigo 1999, 2003; Domínguez-Rodrigo and Barba 2006; Domínguez-Rodrigo et al. 2007; Domínguez-Rodrigo and Pickering 2003; Domínguez-Rodrigo et al. 2005).

A more troubling gap in our knowledge of Oldowan behavior is whether variation in Oldowan tools is in any way related to the biological evolution of the hominin lineage. Although studies have suggested that almost all hominins were capable of using stone tools (and new Pliocene discoveries confirm this [McPherron et al. 2010] although see debate regarding this [Domínguez-Rodrigo et al. 2010]), there is really no way to determine that tools were linked to certain species (Panger et al. 2002; Susman 1994). The best evidence that tool use is associated with the genus *Homo* is that there is no apparent change in Oldowan tool use toward the end of the *Paranthropus* lineage (although the exact termination of this lineage is not well understood, and the archaeological record at around 1 Ma is sparse). New research is providing the basic knowledge of understanding how stone tool form can be modified by different behavior patterns, and this could provide the vital basic information needed for future advances (Williams et al. 2010).

Determining the role of Oldowan artifacts in the biology of our ancestors has another great hurdle, however. We currently do not know the mode and tempo of Oldowan variation through time. One theory suggests that the variation in Oldowan tool use is the result of very long period of stable adaptation that remained unchanged (Semaw 2000; Semaw et al. 1997; Semaw et al. 2003; Stout et al. 2010) Others suggest that the variation within the Oldowan represents multiple lineages of tool use that originate and extinguish in a similar fashion to that seen in the multiple lineages of hominins that existed in the Plio-Pleistocene (Delagnes and Roche 2005; Roche 2000, 2005). This debate is unlikely to be resolved in the near future, mostly because neither perspective has developed explicit expectations that would falsify these hypotheses. Furthermore, each side of the debate uses completely different (yet complementary) techniques to investigate these hypotheses.

A major concern with this debate revolves around the inferential models used to investigate these hypotheses. The study of archaeological remains over the past thirty years has emphasized the importance of directly understanding each inferential step within an archaeological argument (Gifford-Gonzalez 1991). Without this type of focus we are bound to overlook possible equifinalities between higher-level inferences and the archaeological data. Oldowan studies may have come to an impasse because of the lack of integration between higher-order inferences about hominin behavior and lower-level observations. Gifford-Gonzalez provided a framework for understanding the connection between these inferences that she described as a *nested hierarchy* (Gifford-Gonzalez 1991). This nested hierarchy begins with the trace, which is the static archaeological object and in the case of the Oldowan is most likely a stone tool. Then the nested hierarchy builds up to causal agent that concerns actual physical activity that produces the trace. In the case of the Oldowan this is usually the propagation of force through a stone. This depends on the raw material properties of the stone and certain details of fracture mechanics. After the causal agent is the *effector*, which almost always in the case of the Oldowan is hard hammer percussion. Certainly in some cases this can be modified to include bipolar percussion or the use of different types of hammerstones (Texier et al. 2006).

One step up from the causal agent is the actor. In Oldowan studies we are on very shaky ground here. We cannot know the exact species that produced these tools, but we can be reasonably sure that it was a hominin that was bipedal, had a relatively small body size, and a wrist/hand architecture that was archaic (Tocheri 2007; Tocheri et al. 2003; Tocheri et al. 2008). In addition, we also know this hominin was likely omnivorous, and for the latter parts of the Oldowan (after 1.4 Ma) we can be sure that the hominins had a brain size that was much larger than chimpanzees or earlier hominins (Lee and Wolpoff 2003; Wood and Collard 1999). One inferential step up from the actor is the behavioral context, which would involve the production and use of chipped stone tools. This would also include details of the reason for the production of tools (e.g., butchery, woodworking, tuber processing). Again we have limited knowledge about what use hominins had for their tools, but we can be certain that the production of multiple sharp edges played at least some role in this process. This behavioral context would also include the social aspect of tool production (Roux and Bril 2005). There is a possibility that Oldowan hominins taught each other to make stone tools; however, there is currently no good referential model for determining what teaching looks like in the Oldowan archaeological record (Nonaka et al. 2010). Alternatively, a single hominin within a group could have been largely responsible for much of the tool production. Currently these types of questions remain unanswered, except that we can be sure that tools were made within a social context (Lemmonnier 1990). The final inferential step, between the behavioral context and the ecological context, is where the major paleoanthropological questions can be answered. The way in which hominins interact with their environment is almost certainly responsible for evolutionary changes in hominin behavior.

Too often studies of Oldowan research are concerned with only part of this nested hierarchy. I view most Oldowan research as either focused on the nested hierarchy from the perspective of the actor and below (*chaîne opératoire* approach) or from the actor up to the behavioral context (behavioral ecological approach). Bridging the gap that links the full range of the nested hierarchy is rarely accomplished. Approaches that focus on the detailed aspects of stone tool production (e.g., Delagnes and Roche 2005) have the advantage of providing detailed descriptions of the way in which tools are made. They fall short of providing any type of link between these behaviors and the behavioral or ecological context of hominins, however. Conversely, studies that explicitly state the hypothesized details of the behavioral context (Kimura 1999, 2002) of tool production can often apply “decontextualized lithic attributes” in a “rigid and mechanistic way” without attention to the details of the methods and technical strategies of Pleistocene knappers (De la Torre and Mora 2009). Will it ever be possible to bridge the gap between these two methods and their shared inability to link the traces of Oldowan behavior (i.e., artifacts) to the behavioral context of Pleistocene hominins?

One of the ways to begin to bridge these gaps is by first describing the assumptions that are implicit in each of the two approaches, which can provide room for understanding how linking these two methods will afford a more detailed approach to hominin behavior.

The chaîne opératoire approach: The use of detailed description of stone artifact manufacture has a very long history but has really reached its pinnacle in the study of Oldowan research in the past few years. This approach sees the cognitive setting as an essential part of the organization of hominin activities. Pelegrin's (Pelegrin et al. 1988) description of this approach details its three major foci. First, *chaîne opératoire* provides a mechanism for linking aspects of concrete technical activities. Second, moving up an inferential step, the approach then links these technical activities with assemblage level distinctions, which Pelegrin describes as technical systems. Finally, these technical systems are linked to aspects of social organization. This description is very similar to Gifford-Gonzalez's linkage between causal agents (technical activities) and the behavioral context (social organization). The *chaîne opératoire* approach views artifacts as expressions of a shared system of templates and combined knowledge. This knowledge not only testifies to the social make-up of the group but also to the cognitive capabilities of each individual knapper (Hovers 2009). An implicit assumption of this approach, however, is that the toolmakers have a shared ideology that borders on modern ideas of culture. In later periods there is no doubt that this is the case; however, in Oldowan contexts this should be stated as a hypothesis to be tested. Data from nonhuman primate material culture strongly suggests that shared ideologies in the production of material culture are frequently part of ape behavior (Whiten et al. 2003; Whiten et al. 2005; Whiten et al. 2009). A further assumption of the *chaîne opératoire* approach is that technology cannot be separated from its social context. This requires the analyst to understand fully the social context of the early hominins. Since we do not understand the social systems of early hominins, this is a difficult prospect (Foley and Gamble 2009). This is made even more difficult as many researchers doubt whether tool production systems were shared by groups in the Oldowan: "... Inter-group transmission of technical knowledge was [not] yet an established practice" (Delagnes and Roche 2005). Does the identification of technical practices really require a priori assumptions about the infrastructure of culture and society (Hovers 2007, 2009)? It might be possible to use a combination of detailed technological analyses and ecological contexts to understand hominin behavior.

Behavioral ecological approach: This approach to hominin behavior is based largely on the processual models initially devised early in the 1960s that also termed this *behavioral archaeology* (Binford 1977; Schiffer 1976). The approach sees hominins as producing stone tools as a method of solving ecological problems (i.e, procurement of food). Those who conform to this approach assume that ultimate explanations of behavioral variability will be found in adaptive explanations. These adaptive explanations usually incorporate some form of environmental determinism (Stout et al. 2010). The real strength of these models is their direct applicability to models of evolutionary change. In these models, the currency of natural selection is genetic fitness and is directly correlated to adaptive success through improvements in the acquisition of resources aided by stone artifacts. The assumption is that methods of artifact production vary according their benefits, and these methods are heritable (Ferraro 2007). These models often equate to linear models of evolution that assume progressive, directed change through time (Roche 2005). This is not necessarily the case, and environmental explanations for variation in Oldowan behaviors allow for flexible adaptations to varied environments (Braun et al. 2009a). Behavioral ecological models do carry major assumptions about hominin behavior, however. The most basic is that all stone tools in the archaeological record represent adaptive responses to the environment in which the hominins lived. Behavioral ecology models assume that hominins are rational actors and follow patterns that are the "most" adaptive. There is certainly reason to believe that hominins were rational in their decisions to produce one tool over another. However, the likelihood that every single artifact in the Oldowan archaeological record represents an adaptive response seems naive. Some have suggested that the production of tools might have provided the kind of buffer between hominins and environmental pressures that allowed for variation to be more closely aligned with social norms (Hovers 2009). How much of the Oldowan hominin archaeological record reflects social constraints on artifact production? Considering the impact of social mechanisms on the material culture of chimpanzees, it would seem difficult to relegate social variation to the "left over" variation after environmental variables had been fully explored (Boesch and Boesch 1993; Whiten et al. 2009). Recent investigations into the material culture of chimpanzees has shown that social mechanisms are vitally important (Haslam et al. 2009). Furthermore, holistic descriptions of chimpanzees' tool use that include both *chaîne opératoire* approaches as well as behavioral ecological information have been extremely successful at characterizing these behaviors (Biro et al.; Carvalho et al. 2009; Carvalho et al. 2008). By seeing Oldowan assemblages as a palimpsest of multiple knapping events and looking for probabilistic inferences based on modal trends of attribute variation, we might lose sight of individual variation. Natural selection acts on the level of the individual, and so it would likely be important to understand tool use at this level.

Where do we go from here?

Is it possible to integrate these two approaches in a way that (1) allows for detailed understanding of artifact manufacture in its social context, yet still (2) provides answers that can be linked to major evolutionary forces that are no doubt integrated with environmental variation? The way forward will not be easy; the *chaîne opératoire* approach requires the primacy of social interaction in an analysis of artifacts and systematically rejects quantitative approaches. The behavioral ecological approach similarly calls for an emphasis on environmental variables and is very often quantitative.

What would a mixture of these approaches look like? Stout and colleagues (2010) have made the first attempt to bridge this gap by providing a detailed analysis of the Gona material, using concepts that derive from both schools of thought. Their analysis emphasized the importance of certain knapping strategies (unifacial, unidirectional) in the East Gona sites and documented the presence of significant variation in technology between localities. Although their analysis did not include the in-depth description of knapping techniques seen in the analysis of the Lokalalei materials (Delagnes and Roche 2005), this is to be expected given the unique situation of entire refitting sequences that were preserved at Lokalalei. The combination of the detailed technological analysis and the comprehensive study of the raw materials allows detailed inspection of hominin competence in knapping behaviors (Harmand 2009). The analysis at Gona was able to provide some insights into the sources of variation within this area. The archaeological sites within the Gona region span a diversity of habitats allowing for the inspection of the relationship between environmental variation and technological diversity . Stout and colleagues were able to show that the variation seen at Gona could be explained by environmental differences rather than cognitive variation within the hominins that made these tools (Stout et al. 2010). At Kanjera South, a detailed behavioral ecological study has shown that it is possible to explain the selection process for certain raw materials (Braun et al. 2008b). These varied raw materials are also knapped differently (Braun et al. 2009a). A detailed technological study shows that different raw materials appear to reflect different technical competences, when in fact they can be explained by different raw material frameworks (high availability of low-quality materials and low availability of high-quality materials).

Although Oldowan hominins appear to follow certain technical rules, these rules are not universal. One possible step toward integrating these approaches is to identify the presence of certain technical rules and abilities at several sites. If it is possible to isolate the variation in technical actions that cannot be explained by environmental diversity, we may be documenting real differences in traditions (Whiten et al. 2009). Diachronic patterns could be tested against evolutionary models to see if they conform to the models of natural selection acting on behavioral attributes (Boyd and Richerson 1985, 1993, 2005; Lyman and O'Brien 1998; O'Brien et al. 2008). There is certainly the possibility that much of the variation that characterizes the Oldowan could be the result of random drift of behaviors within somewhat isolated groups of hominins (Bentley et al. 2004; Cavalli-Sforza and Feldman 1981; Henrich 2004).

Conclusion

The study of Oldowan behavior is derived from rather meager evidence. Bits and pieces of shattered stone are all we have to gain insight into behaviors that really have no modern analog. The closest representations we have for analogical reasoning are chimpanzees and modern hunter-gatherers, and both of these models pose significant analogical difficulties. In the past twenty years, however, archaeologists have deduced a series of important aspects of the Oldowan behavioral repertoire. We now know that from the very beginning of Oldowan tool use, hominins understood detailed aspects of rock fracture patterns and selected these materials specifically for certain attributes. Furthermore, hominins appear to have transported stone systematically over relatively large distances (especially given the diminutive stature of these hominins). Finally, hominins seem to have followed certain technical rules in the production of artifacts. These rules guided the manufacture of Oldowan cores and appear to be focused on increasing the productivity of cores. Experimental studies have also given us insights into those features of the hominin brain that are associated with Oldowan tool manufacture (Stout et al. 2008; Stout et al. 2000) and provided baseline functional explanations for different tool forms (Tactikos 2005; Toth 1985, 1991). All of these insights together serve to support the idea that hominins likely invested a significant amount of time and effort to learn aspects of tool manufacture and subsequently invested time into maintaining their own personal toolkits.

Further insights into the behavioral variation of the earliest tool users probably will require an integration of the two major approaches to Oldowan studies. There is much that we still must learn about Oldowan behavior. Right now we cannot know whether the strategies we see in the archaeological record represent the same type of structured behavioral patterns that characterize modern human culture (Hill et al. 2009). Semaw and colleagues see the Oldowan as a period of stasis (Semaw 2000; Semaw et al. 2003), but what does this stasis really reflect? Does this stasis represent cognitive capacities of learning in Oldowan hominins (Bettinger et al. 2006; McElreath and Henrich 2007). How can we know more about the evolution of behavior in these earliest of cultural systems? These are the types of questions that archaeologists interested in the Oldowan will pursue in the future. The answers will require more fieldwork, more experimentation, and a greater inclusion of a variety of inputs from fields as diverse as engineering through ethology.

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CHAPTER 9

Testing cognitive skills in Early Pleistocene hominins: An analysis of the concepts of hierarchization and predetermination in the lithic assemblages of Type Section (Peninj, Tanzania)

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Inferences about human cognitive evolution have been gathered by several research avenues, among which stone tools have played a major role. This source includes the study of the technical capabilities exhibited during lithic reduction or the neuropsychological, motor, linguistic, and transmission-learning implications of knapping activities (e.g. Belfer-Cohen and Goren-Inbar 1994, Byrne 2004, Delagnes and Roche 2005, Dibble 1989, Gibson 1991, Gibson and Ingold, 1993, Gowlett 1984, Johnson-Frey 2004, Karlin and Julien 1994, Toth 1985, Mithen 2000, Noble and Davidson 1996, Osvath and Gärdenfors 2005, Stout et al. 2008, Wynn 2002). The traditional evolutionary paradigm, which has also shaped the study of lithics, implies an epistemic construction of concepts and processes from simple to complex within the evolutionary scale (Maschner and Mithen 1986). Researchers therefore are inclined to look for signs of complexity in the technological continuum and to interpret them as markers of the various steps in the process of hominin cognitive development (Foley and Lahr 2003, Karlin et al. 1993).

One of these mental thresholds could be represented by the concept of predetermination in lithic knapping. *Predetermination* has been defined as a process of core exploitation that implies several technical actions aimed at predetermining the shape of flakes (Boëda 1994, Chazan 1997). This process has been perceived as proof of conceptual and operational complexity (Roche and Texier 1991, Delagnes 1995), in which great manual dexterity, long-term planning, decision making, control, and precise intentionality are needed. The accomplishment of these technological requirements is thought to involve a clear sign of cognitive sophistication as well. Although predetermination has been observed in other knapping methods related to the production of large flakes (Bourguignon 1997, Isaac 1969, 1977, Mourre 2006, Sharon 2009, 2010, Sharon and Beaumont 2006, Texier and Roche 1995), the quintessential example of this form of technological achievement is traditionally represented by Levallois core technique (Ambrose 2001, Schlanger 1996). Although the technical definition of what can be considered Levallois is a matter of debate (see various contributions in Dibble and Bar-Yosef 1995, and Peresani 2003), one of the most influential descriptions of this reduction method has been given by Boëda (1991, 1993, 1994, 1995, Chazan 1997).

In recent years, some authors (de la Torre et al. 2003) have argued that the Lower Pleistocene hominins that inhabited the western margin of Lake Natron (Tanzania) between 1.6 Ma and 1.2 Ma exhibited quite complex technological behaviors, which included recurring display of the concept of core hierarchization and blank predetermination in lithic reduction strategies. The interpretation of the lithic assemblages recovered from the Type Section area, in a penecontemporaneous depositional context of deltaic channels located in the proximal area of a lake margin (Domínguez-Rodrigo et al. 2002), led these authors to conclude that hominins were commonly using a *bifacial hierarchical centripetal* exploitation strategy, among other relevant reduction strategies. According to their counts (de la Torre et al. 2003: 214), the bifacial hierarchical centripetal method accounted for 30% of cores, in which most of the technological criteria used by Boëda to define Levallois technique were present.

Accordingly, many cores consisted of the following traits (de la Torre et al. 2003:218). (1) Two surfaces were separated by a plane of intersection. (2) There was a hierarchical relation between these surfaces, with the main surface acting as the exploitation area (aimed at obtaining flakes) and the subordinate surface serving as a preparation surface (aimed at preparing striking platforms). The hierarchical relationship between surfaces (preparation/exploitation) was not interchangeable, because their roles remained stable throughout the flaking process. (3) The maintenance of this volumetric structure was aimed at obtaining predetermined flakes. (4) Flakes detached from the main flaking surface were parallel or subparallel to the intersection plane. (5) The preparation or subordinate surface produced secant flakes with respect to the intersection plane between both surfaces.

These authors (de la Torre 2002, de la Torre et al. 2004, de la Torre and Mora 2009: 182) argued that many of the cores retrieved from Type Section represented the continuity of a single technological sequence and that this sequence could be reconstructed. Based on the fact that the dimensional relation among unifacial centripetal, hierarchical centripetal, and multifacial irregular cores (the three most abundant core types identified at the Type Section area) seemed to be constant, in a decreasing trend from the former to the latter, these authors (de la Torre 2002: 150–151, de la Torre and Mora 2009: 180–183) created a theoretical model that explained in detail the way in which the hierarchical centripetal sequence was performed by hominins. This model encompassed six consecutive phases (de la Torre and Mora 2009: fig. 7.52), in which several archaeological specimens could be actually inserted (Ibid.: fig. 7.53): (1) The core would initially be exploited centripetally on one surface. (2) As the unifacial centripetal exploitation continued, the core would lose its peripheral convexities. (3) The loss of the required convexities would make exploitation difficult, and to reactivate the convex volume of this striking surface, it would be necessary to start preparation in the sagittal plane. (4) This reactivation would produce a hierarchical core, as explained previously. (5) Once the hierarchical pattern was established, the model would continue in successive series. (6) Core exhaustion would imply a final irregular multifacial form.

As a result of their diagnosis, briefly summarized here, these authors (de la Torre and Mora 2009: 185) argue that the technology of the Type Section complex constitutes a basic reference for understanding the technical/operational complex skills of early hominins by proving that they were able to conceive the whole process of core exploitation before the reduction sequence was actually carried out, a clear sign of in-depth planning and foresight in the Early Pleistocene that was previously undocumented to this degree of complexity for this period.

Despite the implications of this study for the technical capabilities of Lower Pleistocene hominins, and the impact that such conclusions have had in the scientific community (Braun et al. 2008, Davidson and McGrew 2005, Delagnes and Roche 2005; Harmand 2007, Semaw et al. 2009), the proposed reduction model has never been experimentally tested and remains highly conjectural. To date it is supported by only two arguments: the decreasing dimensional relationship between various types of cores, and the classification of different cores within the stages created ad hoc to interpret the assemblages (de la Torre and Mora 2009: 182).

Several questions must be addressed prior to the acceptance of this model, however: first, can the model be experimentally replicated following the theoretical reduction sequence proposed and using the same types of raw materials? Furthermore, can the hypothetical constraints related to the loss of convexities in unifacial centripetal exploitation be an obstacle to the progress of reduction, and if so, is starting core hierarchization the proper way to fix that problem? To what extent is this model compatible with the natural block formats present in the area and with the various textures and qualities of the local volcanic lithologies? Do the archaeological cores identified as part of the various exploitation stages come from specific sites, and do they represent the remains of a coherent operational sequence, or conversely, have they been selected from diverse locations, representing incoherent fragments of unknown or heterogeneous knapping events? Were examples of the different reduction stages produced in the same type of raw material, or conversely, were different stages produced in different textures, formats, and qualities? Answering these questions is crucial to understanding whether early Pleistocene hominins carried out core exploitation as described in the centripetal hierarchical method across the entire reduction sequence.

The present study addresses these questions and tests the model and its implications. This will be the first attempt to reproduce the bifacial centripetal hierarchical model experimentally. If the theoretical construction proposed by de la Torre and colleagues is correct and the main goal of the complex knapping strategies exhibited by hominins at Peninj was the production of predetermined flakes (de la Torre et al. 2003: 218), then it should be possible to recognize this pattern in the archaeological and experimental flake collections. For this purpose, we have undertaken a revision of the archaeological collections so far retrieved from the Type Section, accompanied by a program of experimental work reproducing alternative models (predetermined and nonpredetermined). The goal was to compare the flakes experimentally obtained through different knapping methods with the archaeological collection and understand which exploitation strategies are represented in it.

Method

The null hypothesis to be tested in the present study, based on de la Torre et al. (2003: 204), suggests that the lithic assemblages retrieved from Type Section at Peninj show clear signs of technological complexity, implying the preconception of the complete reduction sequence. This hypothesis is based on two premises (de la Torre and Mora 2009: 184–185): (1) in many instances core volume was conceived hierarchically, and (2) this conceptual and operational template was maintained throughout the whole reduction sequence. The alternative hypothesis states that, conversely, the lithic assemblages recovered at the Type Section do not show substantial and recurrent signs of predetermination and that the criteria used to make such inferences can be attributed to alternative core reduction strategies. To test the validity of the null hypothesis, we must first test the premises on which it is founded, reviewing the previous diagnoses of blank predetermination, core hierarchization, and continuous planning of the reduction sequence. For this purpose, we have used the following analytical methods:

Archaeological sample analysis

Core and flake categories are interrelated and constitute the most efficient source of technological information in lithic assemblages. The former is a reliable source for the reconstruction of technological schemes and the latter represent the by-products of those processes. Thus, we have studied a collection of 46 cores plus 274 detached pieces (including flakes, retouched flakes, and flake fragments in which diagnostic traits relevant to this study could be read) retrieved from sites in the Type Section site complex (Domínguez-Rodrigo et al. 2002, 2009a): ST2, 3, 4, 6, 15, 30, 31, 32, 35, 36, 37, 38. Most of these objects correspond to the collection previously analyzed (de la Torre 2002, et al. 2003, 2004, and Mora 2009).

Cores are key to the dynamic reconstruction (e.g., operational sequences) of technological processes (Boëda 2001, Geneste 1989, 1991, Karlin et al. 1991, Turq 1996, 2003). General attributes studied in the core sample include (Table 9.1): (1) Raw material type (basalt, nephelinite, quartz); (2) Raw material quality (assigned values ranging from 1 = very poor quality to 5 = very good/optimal [Baena et al. 2010]); (3) Support type (spheric and subspheric, angular fragment/block, hemispherical or flake); (4) Dimensions (length, width, thickness) and weight; (5) Exploitation system (Díez-Martín et al. 2010); (6) Number of negative scars per striking surface.

The study of the complete core set included the application of selected variables to all specimens (recorded in a data set created in Excel), and the drawing of each specimen. This drawing included sketchy diacritical schemes of all specimens (i.e., the reconstruction of scar trajectory and scar sequential order). About 31% of these sketches and drawings were selected to undertake elaborate diacritical graphic layouts (using Adobe Photoshop, Paint Shop Pro, and Freehand software). This selection was qualitative and tried to include the widest possible range of variability previously seen in the core sample (raw material type and quality, degree of exploitation, size range, and support type). For comparative purposes, we also included, in

TABLE 9.1. *Description of several variables used for the analysis of the lithic material (both archaeological and experimental)*

Variable	Rank	Description	References
General			
Raw material quality	1. Very poor; 2. Poor; 3. Medium/average; 4. Good; 5. Very good/excellent	Combination of two criteria: a) Tenacity (friable, brittle, sectile, malleable, flexible, elastic, and tough); and b) Homogeneity (smoothness indicators: inclusions, porosity, grain size, fissures, and natural cleavages).	Baena et al., 2010.
Cores			
Support type	1. Spheric and subspheric; 2. Angular fragment/block; 3. Hemispherical or flake	This variable is based on the natural morphology of the support.	
Exploitation system	1. Unifacial unipolar; 2. Unifacial multipolar; 3. Bifacial unipolar; 4. Bifacial bipolar; 5. Bifacial orthogonal; 6. Bifacial centripetal; 7. Multifacial multipolar	Combination of these three attributes: a) Facial exploitation (unifacial, bifacial, trifacial, multifacial); b) Polarity (unipolar, bipolar, multipolar); c) Relationship between different knapping series (linear, opposed, circular, orthogonal, centripetal)	Díez-Martín et al. 2010.
Flakes			
Technical axis location	1. Both axes are coincident; 2. ≤30° deviation; 3. ≤60°; 4. the morphological axis is transversal to the technical axis.	This field expresses the degree of deviation between the technical axis and the morphological axis in a flake.	

TABLE 9.1 *(continued)*

Variable	Rank	Description	References
Total edge length		Total cutting edge length per flake, measured using a plastic meter and summing the flake segments in which active cutting edge can be identified.	
Exterior platform angle		Measured with a goniometer, as explained in the references	Dibble and Whittaker, 1981; Gallet 1998
Interior platform angle		Measured with a goniometer, as explained in the references	Dibble and Whittaker, 1981; Gallet 1998

our complete graphic treatment of archaeological cores, most of the pieces put forth as representative examples of the different stages considered in the hypothetical predetermination model (de la Torre 2002, et al. 2003, 2004, and Mora 2009: fig. 7.53). The *diacritical analysis of cores* is a procedure that allows a detailed reconstruction of the reduction strategies carried out by the knappers and is a crucial tool to understand and reconstruct operational sequences (Boëda 2001, Dauvois 1976, Forestier 1999, Inizan et al. 1995, Li et al. 2009, Slimak 2008, Slimak and Lucas 2005, Tiffagon 2006). The diacritical analysis is a macroscopic interpretation of the last technical gestures prior to core abandonment, reproducing core contour and negative scars in the last reduction phases. Scar directionality has been carefully decoded through the topographic analysis of negative scars and the study of other technical traits representative of scar trajectory (Callahan 1988, Baena and Cuartero 2006; Figure 9.1). The analysis of longitudinal and transversal core topography can be used to discriminate between maximal depressed/concave areas (those identified as proximal or initiation faces, or the surface of the core where the crack originates) versus less depressed areas (side and distal). This analysis was complemented with the observation of other technical traits that identify the process of fracture propagation in flakes from the striking point outward: waves and grooves or striae (Cotterell and Kamminga 1987). As important as scar directionality in the reconstruction of diacritical schemes is *scar chronology*, or the sequential process of reduction (Dauvois 1976: 194, Inizan et al. 1995). In other diacritical schemes (de la Torre 2002, et al. 2003, 2004, and Mora 2009), however, the chronological parameter is not included, limiting the study to scar directionality. Negative scar chronology is essential to this analysis and can be reconstructed by observing the superposition or sequential order of negative scars. To this purpose, several attributes (e.g., breakages of morphology and side or distal grooves) have been considered (Baena and Cuartero 2006: 150): breakages of negative scar morphology (forms other than the original oval silhouette of a negative scar tend to indicate an older step in the reduction sequence), distal margin morphology (the presence of stria in the distal margin of a negative scar indicates a recent step in the reduction sequence), intersection morphology (new negative scars tend to eliminate previous ridges, or in other cases to spread following the direction of the ridges created by previous negative scars). The combination of all these morphological criteria constitutes an effective tool to reconstruct the sequence of detachments shown by the core in its final phase of exploitation.

Flakes have been studied using the following criteria (Table 9.1): (1) Raw material type (basalt, nephelinite, quartz); (2) Raw material quality (we have assigned values ranging from 1 = very poor quality to 5 = very good/optimal [Baena et al. 2010]); (3) Technical axis location; (4) Dimensions (maximum length, width, thickness) and weight; (5) Villa-Toth types (Toth 1982, Villa 1978); (6) Termination (feather, step, hinge, overshot [Cotterell and Kamminga 1987]); (7) Snapping (Longitudinal axial or Siret, side longitudinal, transversal, proximal [Diez-Martín et al., 2011]); (8) Total edge length; (9) Exterior platform angle (Dibble and Whittaker, 1981; Gallet 1998); (10) Interior platform angle (Dibble and Whittaker, 1981; Gallet 1998); (11) Striking platform (cortical, unifaceted, bifaceted, multifaceted, line, point, absent, fragmented); (12) Striking platform size (width, thickness); (13) Number of dorsal scars; (14) Dorsal pattern (unipolar proximal, unipolar distal, unipolar orthogonal, bipolar opposed, bipolar transverse, bipolar orthogonal, multipolar orthogonal, multipolar radial); (15) Edge location, when present (right sided, left sided, both right and left sided, proximal); (16) Bulb of percussion (present, diffuse, absent); (17) Initiation (hertzian, bending, wedging, wedging/bending, wedging/hertzian, bending/hertzian [Cotterell and Kamminga 1987]).

Experimental analyses

We have carried out the experimental reduction of thirty lava cores retrieved from the Natron area. Good quality, fine-grained basalt supports were

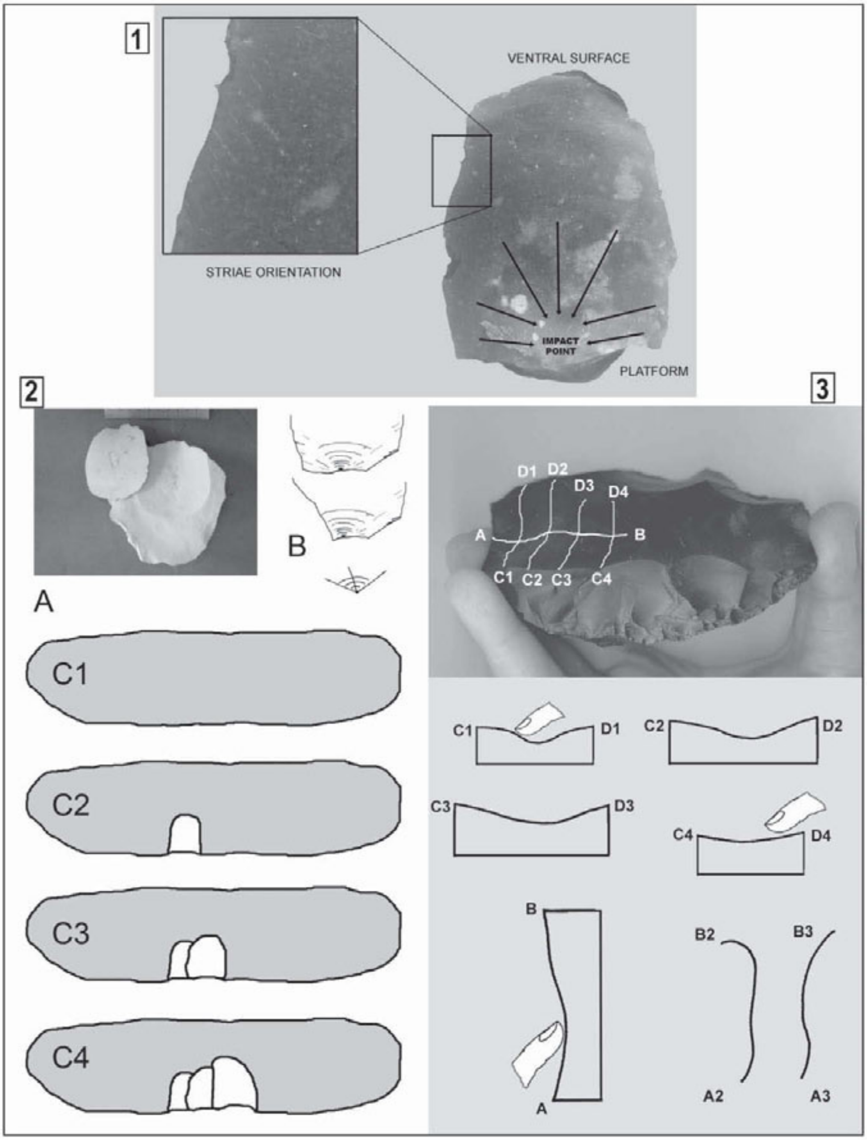


FIGURE 9.1. Graphic representation of the criteria followed for the diacritical analysis of the Type Section cores: Criterion 1. Stigma on the negative scar edge (striae and grooves). Although this parameter is more difficult to assess in coarse-grained rocks, conchoidal fracture produces a set of striae and fissures on the distal edge of the detachment. Criterion 2. Theoretical flake morphology (symmetry and convex edges). In a negative scar, the negative bulb and negative waves show a general tendency to expand in a convex and symmetric manner. The most complete negative scar (complete negative bulb, complete negative waves, theoretical symmetry in relation to the longitudinal axis) is the most recent. Criterion 3. Relative topography between negative scars (macro- and micro-levels on the negative side edge). A new detachment shows a relatively lower topographic position in relation to a previous detachment. This topographic trait is observed both in relative morphology and in intersection ridges between detachments. A new negative presents a slightly concave edge.

TABLE 9.2. *List and brief description of the replicated experiments*

Exp.	Raw material					Hstn.	Reduction
No.	Support	Source/Type	Quality	Format	Module	No.	model
1	1	B2	3	1	3	1	2
2	1	B2	3	1	4	1	1
4	1	B2	4	1	2	2	1
5	3	B1	5	3	2	3	1
7	3	B1	5	2	3	4	1
8	2	A1	5	1	2	5	1
9	2	A3	4	1	2	6	2
13	2	A3	4	1	2	6	2
14	2	A1	5	2	2	4	1
15	1	B2	3	1	2	6	2
18	3	B1	5	1	1	6	1
19	3	B1	5	1	2	4	1
20	3	B1	5	1	3	6	2
21	3	B1	5	1	3	6	1
22	3	B1	5	1	3	7	1
23	2	A1	4	2	2	7	1
24	2	A2	3	1	2	6	1
26	2	D4	3	2	2	3	1
27	2	D4	3	3	2	3	1
29	2	C4	3	1	2	8	1
30	2	D5	5	1	2	8	2

Experiment number (only those used for this analysis are cited here); Raw material (see code in Table 3); Hammerstone number (see reference in Table 2); Reduction model (1 = Search of maximal concavexities; 2 = Bifacial hierarchical centripetal).

selected on the banks of the Peninj river, not far away from its delta. Coarse-grained, porous basalts were quarried closer to the lake floodplain. Different varieties of nephelinites were quarried in the vicinity of its primary source (the Sirere Hills, about 12 km from Type Section), at the midcourse of the Peninj river, and at various secondary streams located on the escarpments (Luque et al. 2009a). Basic descriptive traits of each experiment are summarized in Tables 9.2 and 9.3. Core reduction was undertaken by two of us with experience in knapping (FC and PS) on a large plastic sheet to ensure that all the by-products were preserved. Knapping was carried out following the different reduction models considered in this study (see later discussion) until cores reached the point of exhaustion. Finally, flakes were

TABLE 9.3. *List of hammerstones used for the experimental replication*

No.	Rock type	Origin	Format	Module	Weight	Cohesion
1	2	B2	2	2	450	2
2	3	B1	1	2	561	1
3	2	B1	3	3	186	1
4	5	B2	2	1	245	3
5	2	B1	1	2	603	1
6	1	B6	1	1–2	756	3
7	3	A1	2	2	236	1
8	2	B1	1	1	563	1

Rock type, origin, format and module (see legend in Table 9.3); Cohesion (1 = High, 2 = Medium, 3 = Low).

sequentially labeled, each reduction sequence/core was refitted with glue and kept in individual plastic bags for further laboratory analysis.

A field sheet was used to register variables relevant to the knapping process, which have been put into three groups (Table 9.4). Two reduction methods were reproduced in the course of the experiment (Table 9.5):

Model 1: *Search for maximal convexities* (CC; short series of discontinuous alternation in two, nonhierarchical, surfaces). We identify three different phases in this reduction model, as observed through experimentation:

1. Initiation: Reduction is carried out from as many natural striking platforms as possible, with the main goal of maximal exploitation of each striking surface. In some cases knapping series were relatively long, although without exploiting the whole perimeter of the core; however, spherical or hard supports prevented long series from the same striking surface. This stage barely transforms the natural morphology of cores. Thus, when the core is a massive pebble or block, morphology tends to be polyhedral or orthogonal; when the support is a flat, hemispherical block or a thick flake, the morphology tends to be discoid.
2. Second series: Negative scars, obtained in the course of the initiation phase, are used as striking platforms for subsequent series. Flakes detached at this point preferentially show plain platforms, although some cortical and faceted striking platforms have also

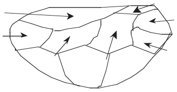
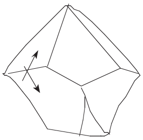
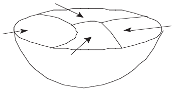
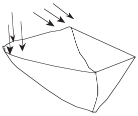
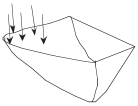
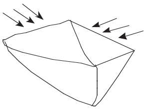
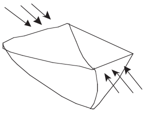
TABLE 9.4. *Variables used for the experimental program*

Raw material	
Quality	1. Very poor; 2. Poor; 3. Medium/average; 4. Good; 5. Very good/excellent
Format	1. Large (>10 cm and >500 g), 2. Medium (5–10 cm and 100–500 g), 3. Small (<5 cm and <100 g).
Support	1. Pebble, 2. Angular block, 3. Flake or angular fragment
Module	1. Uniaxial (Length = Width = Thickness); 2. Biaxial thick (L > W = T); 3. Biaxial thin (L = W > T); 4. Triaxial (L > W > T).
Source	1. Lake flood plain; 2. Peninj river proximal area; 3. Peninj river midsection; 4. Tributary channel in the Peninj mid-section; 5. Primary outcrop.
Petrography	1. Fine-grained, black-bluish basalt; 2. Coarse-grained, porous brown basalt; 3. Coarse-grained, porous reddish basalt; 4. Nephelinite with abundant pyroxenes; 5. Nephelinite with scarce pyroxenes; 6. Irregular nephelinite, with natural cleavages.
Technique (type of hammerstone and hammering action)	
Raw material	1. Quartz (coarse-grained, heavy, scarce toughness); s2. Basalt (fine-grained, heavy, and high toughness); 3. Basalt (fine-grained, heavy, scarce toughness); 4. Porous basalt (light, scarce toughness); 5. Fine-grained basalt (light, scarce toughness).
Format	1. Large (>10 cm, >500 g); 2. Medium (5–10 cm, 100–500 g); 3. Small (<5 cm, <100 g).
Module	1. Uniaxial (L = W = T); 2. Biaxial thick (L > W = E); 3. Biaxial thin (L = W > T).
Grain cohesion	1. High; 2. Medium; 3. Low
Hammering striking direction	1. Cushioned frontal; 2. Tangential.

been documented. At this stage, knapping series tend to follow a bifacial discoid pattern.

3. Discard: Core rejection results when suitable striking angles are lost, and particularly when the last negative scars prevent the obtainment of new flakes on the same surface (step or wedging fractures would be common).

TABLE 9.5. *Schematic reconstruction and general characteristics of the various reduction models replicated in the experiment and/or cited in the text*

Reduction model	**Name**	**Strategy**	**Hierarchical?**	**Volume (Surfaces)**	**References**	**Sites and Contexts**
	Levallois centripetal recurrent	Succession in surface exploitation	Yes	2 asymmetric surfaces	1, 2, 3, 4	MSA/Middle Palaeolithic (MP)
	Alternate discoid	Continuous alternation	No	2 asymmetric or symmetric surfaces	1, 2, 5, 6, 7, 8	MP
	Bifacial hierarchical centripetal	Succession in surface exploitation	Yes	2 asymmetric surfaces	6, 9, 10, 11	MP Lower Pleist. East Africa ? Olduvai ? Peninj ?
	Search of maximal convexities	Discontinuous alternation	No	2 or more surfaces, symmetric or asymmetric	12, 13	Dmanisi II, East Africa Olduvai?
	Unifacial unipolar	Without alternation	No	2 or more surfaces, symmetric or asymmetrical	12, 13	Gona, Dmanisi IV, Fejej
	Unifacial orthogonal	Without alternation	No	2 or more asymmetric surfaces	13	Dmanisi IV, East Africa (Lower Pleist.)
	Bifacial Bipolar Opposed	Without alternation	No	2 or more surfaces, symmetric or asymmetrical	14	East Africa (Lower Pleist.)

References: 1 = Boëda 1993, 2 = Boëda 1994, 3 = Van Peer 1992, 4 = Bourguignon 1997, 5 = Slimak 2003, 6 = Jaubert 1993, 7 = Bourguignon and Turq 2003, 8 = Roche and Texier 1991, 9 = Vaquero 1999, 10 = de la Torre et al. 2003, 11 = de la Torre and Mora 2005, 12 = Lumley et al. 2995, 13 = Baena et al. 2010, 14 = Delagnes and Roche 2005.

> Model 2: *Bifacial Hierarchical Centripetal* (HD; long series, successive and hierarchical exploitation of both surfaces, *sensu* de la Torre, 2002, et al. 2003, 2004, and Mora 2009).

Both methods were experimentally reproduced to document which criteria can be used to differentiate a method that hypothetically predetermines the reduction sequence (bifacial hierarchical centripetal) from an alternative method that does not (search of maximal convexities).

Statistical analyses of data

Archaeological flakes and a sample of experimental flakes produced in the framework of the two different reduction methods (see later) were statistically compared. In each sample, the described sets of flake attributes were used to establish comparisons. Data were statistically analyzed with R, a software program that allows a high degree of plasticity in the design of the statistical tools and parameters applied to the analysis (www.r-project.org). All graphs were also programmed in R.

Initially, it could be argued that using an intuitive approach by selecting specific variables that from the technologist's point of view may more aptly reflect variability among data sets would be more adequate than letting statistical tests determine which variables are discriminating. Any intuitive analytical approach can be epistemically questioned, if not properly justified, owing to the intrusion of subjectivity and its potential biasing, however (Bunge, 1998). A potential biasing factor in the application of an intuitive approach to the study of lithics is that relationships of variables detected in some types of raw materials (with defined dimensional properties or technical gestures) do not need to be universally reflected on other raw material types. Even if some variables are related to dimensional properties of artifacts, an intuitive approach neglects the real discriminatory power of these variables when trying to differentiate data sets, because it does not consider the possibility that other variables may have a better discriminatory power. It has been stressed that when one is handling a large set of variables, multivariate analyses should determine which variables are mathematically discriminant and which are not (Hair et al. 1998). This can be successfully done when applying a two-stage process. In the first stage, exploratory methods should detect which variables account for most of the sample variance and which ones are potentially good classifiers of data sets (Hair et al. 1998). A second stage targets those selected variables and performs confirmatory tests aiming at determining the degree of discrimination of those variables. This double approach is the one we have used for the present study.

To discriminate the weight of each variable in determining sample variance and its effects on the differences between the experimental and archaeological assemblages, a principal component analysis (PCA) was carried out. PCA synthesizes the sample variance contained in a set of variables into factors. These factors explain most of the variance and classify each variable according to its contribution (communality) to the factorial solution (Hair et al. 1998). The final scoring was obtained through a rotated matrix. For this purpose the "mva" library was used and the function selected was "prcomp," which was preferred over the "princomp" function, because calculations are done by a decomposition of a centered and scaled data matrix not using eigenvalues on the covariance matrix. This usually is the preferred method for numerical accuracy (Crawley 2007).

Usually, when combining categorical and noncategorical variables, the most adequate PCA approach is the use of a categorical principal component analysis (CATPCA), as we have done when analyzing data without a prior exploratory analysis (Diez-Martin et al., 2009c). When an exploratory analysis is performed, however, standard PCA can be used as well if results are confirmed later with alternative tests. Categorical variables in this case must be subsequently adapted to a technique that was created for the analysis of numeric variables. Although a widely used method consists of turning categorical variables into dummy variables (Filmer and Pritchett, 2001), it has been shown that originally categorical variables produce better results in PCA analyses when used as ordinal and treated as the rest of numeric variables (Kolenikov and Angeles, 2004, 2009). Categorical variables in the present study were turned into ordinal, and the PCA was carried out lumping them with the rest of variables (Hair et al. 1998). To prevent the effects of variables that are at different scales, all continuous numeric variables were standardized (by using the function "scale") to minimize the wide range metric data among them and to approach them to the numeric values of the discrete variables. Cases with missing data were omitted instead of replacing them with imputed values. Multicollinearity was screened and documented to be low, except for strict dimensional variables (namely, flake length, width, and thickness), where it was higher.

On top of using the "prcomp" function, a parallel principal component analysis was carried out using the "dudi.pca" function of the "ade4" R library. The reasons for selecting this library were the framework provided for Euclidean exploratory methods, which for exploratory PCA was very useful, and the implementation of graphical functions, which were much more visually informative for classification purposes than the "biplot" function associated to "prcomp." The function "dudi.pca" also treats variables and/or cases jointly, whereas "princomp" and "prcomp" deal with cases only. The method used allows introducing new information (through a matrix of weights on the observations or the variables), such as distances between neighboring cases. Furthermore, "dudi.pca" also enables a PCA on numeric as well as ordinal categorical variables. For this reason, variables in this test were not standardized or scaled prior to the analysis. The use of this test was meant as a control for the standard PCA. Because PCA with "dudi.pca" showed similar results (with higher variance explained) than the PCA with "prcomp," it validates its results.

Once the overall variance of the samples was understood through the PCA, it became necessary to target the impact of discriminatory variables in the comparative experimental and archaeological samples. Given the degree of overlap detected among the comparative samples, sample variance became of limited value, and between-group variance seemed a more effective way of addressing which variables could be potentially useful to discriminate among the three assemblages analyzed. For this purpose, a classification tree (CT) approach was used. Regression and classification trees represent a balanced approach between linear models and nonparametric models, because they allow the simultaneous comparison of continuous numeric and discrete categorical variables and their systematic and random components. The predictive value of regression trees has been argued to be comparable to linear regressions, and in mixed-effect models even superior to them (Faraway 2006). Regression and classification trees proceed by recursive partitioning, creating regions. In regression trees, the algorithm used performs the partition by minimizing the residual sum of squares and computing the mean within each partition, which conditions the distance of the splits in the resulting tree. In classification trees, the algorithm used is based on the deviance, the entropy, or the Gini index. The model is fitted using binary repeated partitioning, whereby the data successively split in two branches so that at any node, the variable that maximally differentiates the response variable is selected. R includes some libraries that can be used for regression trees; RPART is one of the most popular. The "rpart" routines implement some algorithms, which enable splitting with higher homogenity and reducing impurity, as measured by the Gini index, with the resulting models being represented in binary trees.

Samples were analyzed through a function that bears the same name as the "rpart" library. By default, "rpart" runs a tenfold cross-validation. This validation is carried out by the "rpart" function, providing a "cp" parameter defined as the ratio of the number of terminal nodes to the residual sum of squares (regression) or the cross-validated error (classification) of the root tree. The initial exploratory tree had to be pruned into a smaller number of splits. Predicted values and the deviance within each node were obtained. The pruning criterion is the predicted loss, which usually is the error rate. The criteria used for tree pruning was based on the lowest value of the random error from the "xerror" return, assisted by a scree plot of the relative error obtained with the function "plotcp." Trees were graphically represented with branch length proportional to the improvement in the fit, which is proportional to the decrease in the residual sum of squares (regression trees) or the random error (classification trees). Contrary to PCA, CTs handle missing values very easily. Their structure is easier to understand for nontechnical people. In the present study, cross-validation and pruning did not affect significantly the resulting trees and only modified the lowermost branches, where discriminatory variables accounted for a very small fraction of variance. During cross-validation, the main variables selected were the same as in the definitive tree. The CT was applied selecting model type (the Peninj assemblage and both experimental assemblages) as the dependent variable.

In the present study, two types of CT were obtained. The first one was based on the analysis of raw data. This provides differences in the absolute values of the variables, which is convenient to detect differences in size-related models. Raw data, however, do not produce useful information when considering relative dimensions caused by shape of stone tools. For this reason, principal component scores were also used in the elaboration of a second tree. Faraway (2006) suggested this approach when variables related to size possessed high variance and might bias the classification. If selection is influenced by size, relative dimensions are more informative. To use relative dimensions, Faraway (2006) suggests using principal component (PC) scores rather than the raw data. The PC scores "seek out the main directions of variation in the data and might generate more effective predictors for classification" (Faraway 2006, p. 265) and select variables that would describe shape or other categorical values better (for examples of better classifications when absolute size is biasing the CT, see Faraway 2006). This approach allowed detecting the direction of variation in the data and served to confirm the inferences drawn from the previous standard PCA.

Our statistical approach targeted to use exploratory techniques, such as PCA and CT to select variables with discriminatory value. Then, we used the most adequate confirmatory analysis for studying the differences of the three data sets. With a clear picture of which variables determined the sample variance and which were more crucial (regardless of their weight in the sample variance) in between-group variance, to differentiate the three analytical sets, a multiple discriminatory analysis (MDA) was implemented. Whereas PCA maximizes sample variance, MDA maximizes intergroup variance, which allows factor discrimination. There is some debate regarding the effects of using samples with normal or nonnormal distributions and heteroscedastic versus homoscedastic variance in PCA (Hair et al. 1998). For linear discriminant analysis this is mandatory, however. For this reason, all of the numeric variables were inspected for skewness and normality. Shapiro-Wilk and Anderson-Darling normality tests were used both prior to and after transformation of data. The kurtosis and skewness of data were measured using the "fBasics" library of R. The "nortest" library of R was used to perform normality tests. Those variables with a nonnormal distribution were transformed using a Box-Cox method. For this purpose, the "geoR" and "car" libraries of R were used. Box-Cox transformations require the use of a function that determines the transformation according to the heteroscedasticity of data. Both R libraries contain functions that determine which type of heterocedasticity the data set exhibits and the subsequent most adequate transformation procedure. The determination of the type of heteroscedasticity is provided by the "boxcox.fit" function of the "geoR" library, and the transformation is then performed by the "box.cox" function of the "car" library.

Two analytical approaches were used for MDA. The first one involved the use of the "lda" function of the MASS library. This is the typical linear discriminant analysis (LDA), resulting from a linear combination of variables with respect to the discriminant coefficients. The other approach involved using the "discrimin" function of the "ade4" library. This MDA consisted of linearly combining the variables after normalization with the coefficients derived by the factor variable. Both maximize the intergroup variance to detect differences for discriminating factors. Wilk's lambda was estimated using the "klaR" library and the "greedy.wilks" functionin R.

Some of the downsides of linear discriminant analysis are the inadequate definition of group division boundaries and its lack of flexibility. Furthermore, when too many correlated predictors exist, LDA has to manipulate simultaneously many parameters with high variance. A mixture discriminant analysis (MXDA) allows mixing heterogeneous distributions in the same model. It is argued to outperform linear discriminant analyses (LDA) and quadratic discriminant analysis (QDA; Hastie and Tibshirani 1996; Ma and He 2008). For these reasons, MXDA introduces the flexibility of nonlinear regression models by allowing the use of nonnormal distributions and heteroscedastic samples. When they are compared simultaneously, it has been shown that LDA was suboptimal in situations of skewed predictors and that MXDA performed significantly better (Rausch and Kelley 2009). Given that in our analytical samples, the discrete variables did not adapt to a linear normal distribution, this may have introduced some noise in the results obtained by MDA. For this reason, and as a confirmatory test, we also used a MXDA in the classification of the three data sets and the selection of discriminant variables. This was done by using the "mda" library of R.

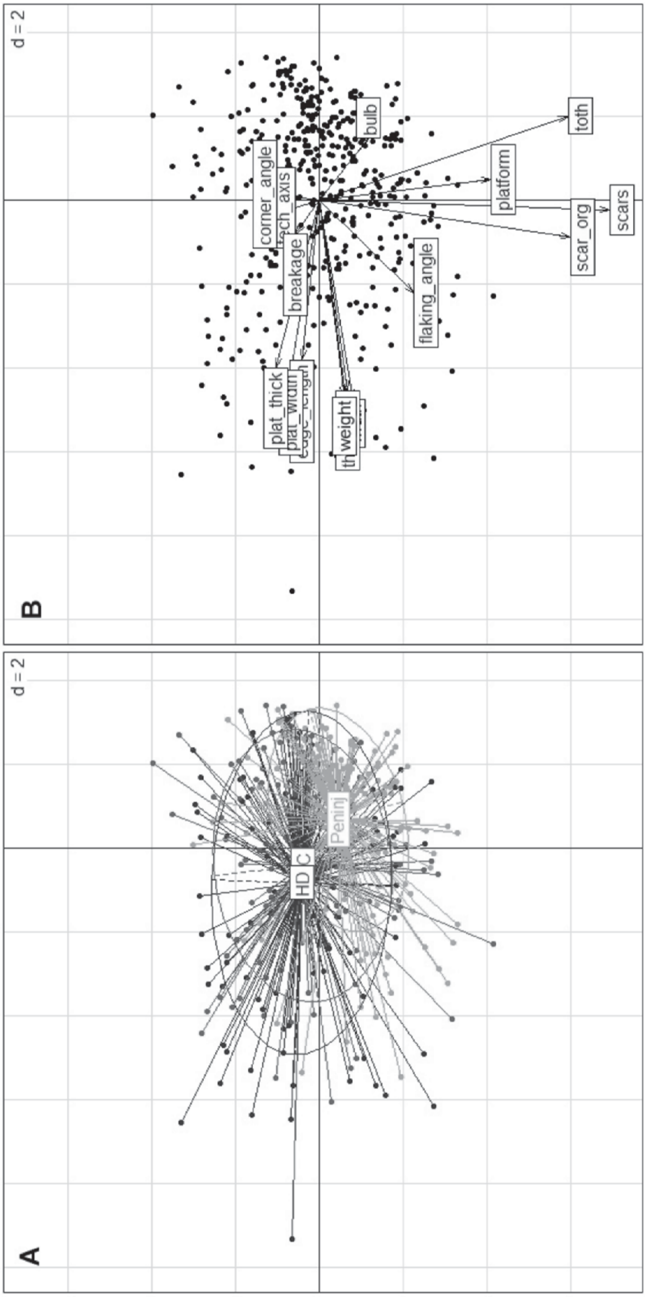
Finally, a comparative analysis of the properties of the archaeological cores was carried out to document how core size and exploitation intensity were related to raw material quality, number of negative scars, type of support (cobble, angular fragment, flake), and exploitation system (unifacial, bifacial, multifacial and centripetal). For this purpose a multiple correspondence analysis (MCA) was carried out using the MASS library of R and the "mca" function.

Results

Statistical comparison of the archaeological and experimental flakes

As remarked earlier, the concept of core hierarchization must be understood, taking into account that it implies several technical actions (driven by the specific roles played by the two different surfaces involved in the process) aimed at obtaining predetermined flakes (Boëda 1994, Chazan 1997, Lenoir and Turq 1995). As predetermined flakes should be the result of core hierarchization, we decided to study to what extent the criteria linked to hierarchization (as experimentally tested) are statistically detectable in the archaeological samples. For this purpose, we compared the technical traits identified in a sample of archaeological flakes ($n = 274$) with those observed in an assemblage of flakes resulting from four experiments reproducing a centripetal hierarchical model ($n = 147$), and another assemblage comprising four experiments replicating a search of maximum convexities model ($n = 142$).

An exploratory PCA using the "prcomp" function yielded a several-component solution, with a KMO value of 0.68, which indicated the moderate adequacy of the sample for a factorial analysis. With a value of >1 for the eigenvalues in the screen plot being used, three components were selected, which explained 50.7% of the sample variance. The loadings on the variables reflected a moderate contribution of most of them (<0.5) to the final three-component solution. An exploratory PCA using the "dudi.pca" function yielded a several-component solution. With a value of >1 for the eigenvalues in the screen plot being used, three components were selected, which explained 54.4% of the sample variance. The loadings on the variables reflected a moderate contribution of several of them (<0.5) to the final three-component solution. The low proportion of variance explained and the low values of the individual scores of each variable could be accounted for by the large number of variables used and the variably small contribution that most of them to the final solution (Figure 9.2A,B).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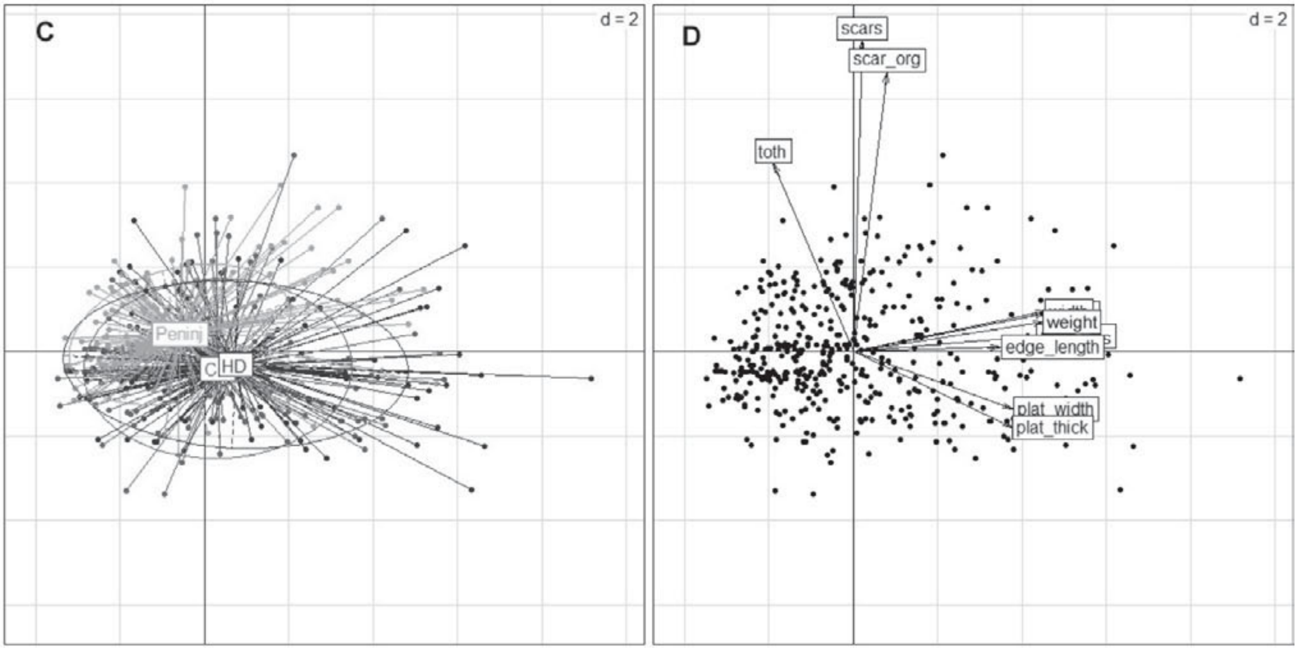

FIGURE 9.2. **A**, Plot of loadings of data after an exploratory principal component analysis (PCA) including all the variables and producing a two-dimension solution, showing the clustering of the search for maximal convexities (CC) and the hierarchical discoid (HD) experimental methods, as well as the Type Section archaeological data (Peninj). **B**, Canonical weights on the two-dimension solution of each of the variables used for the PCA and their spatial relation to the plotted individual data. **C**, Plot of loadings of data after a principal component analysis (PCA), including only those variables with high loading scores on a two-dimension solution, showing the clustering of the search for maximal convexities (CC) and the hierarchical discoid (HD) experimental methods as well as the Type Section archaeological data (Peninj). **B**, Canonical weights on the two-dimension solution for the selected variables used for the modified PCA as shown in Figure **C** and their spatial relation to the plotted individual data. Ellipses for A and C show 95% confidence intervals for each sample. Key to the labels for Figures **B** and **D**: Platform width (plat_width), platform thickness (plat_thick), length of the edge (edge_length), technical axis (tech_axis), external platform angle (corner_angle), initiation (breakage), Toth's flake types (toth), number of previous detachments (scars), dorsal pattern (scar_org).

A second PCA exploratory approach, discarding those variables with a loading score <0.3 in the coefficients of any of the three components, yielded a KMO value of 0.73, which showed that the sample with the discarded variables is more useful to explain the model. The variance explained by a two-component solution was 62.3% for the "prcomp" function and 66.2% for the "dudi.pca" function. For the "prcomp" function," the highest loading scores (>0.88–0.90) for the first factor were documented in the scaled dimensional variables (length, width, thickness, and weight) and for the second factor in dorsal side pattern variables: dorsal scar organization (0.78) and number of scars (0.86). If the variables with loading scores >0.75 are also selected for the "dudi.pca" function, then, those related to flake dimensions (first component; loading score range: 0.85–0.90) and dorsal side pattern (second component; loading score range: 0.77–0.86) were also selected and are more influential than those related to the dimensional properties of platform and edge length, which score slightly lower (Figure 9.2C,D).

The PCA tests show a strong overlap among the two experimental models and the archaeological sample when complete sample variances are contemplated, with a more reliable difference based on a model that has discarded the variables with low variance (Figure 9.2). The resulting model relies on a first component (which explains 49% of the variance) based on the dimensional variables (length, width, thickness, weight) and a second component composed of the variables related to the analysis of dorsal patterns (17.2% of the variance). See Table 9.6 for a comparison of the mean values of the three samples. This shows that slightly less than one half of the sample cannot be differentiated using these variables alone. In both PCAs, variables related to striking platform morphology and type also rank high, although lower than the selected variables for the main component solution (Figure 9.1B,D). It is actually surprising that the experiments cannot be separated when using the set of analytical variables selected for the present study (Figure 9.2). The difference between the experimental set and the archaeological sample is more pronounced, but one could argue to what extent this difference reflects differences that are technological in nature or taphonomic: the archaeological sample can be biased toward certain sizes owing to preservation processes affecting the fluvial contexts where these sites were formed. This has been documented to be a minor factor (Domínguez-Rodrigo et al. 2009a). Alternatively, it could be argued that the archaeological sample represents a portion of (or a fragmented stage of) a reduction sequence initiated and continued somewhere else in contrast to the complete reduction sequence represented in the experimental

TABLE 9.6. *Mean values of the three lithic samples compared in the statistical analysis*

	Length	Width	Thickness	Weight	Toth's types*	Edge-length	Platform*	Plat-width	Plat-thick	Scars	Scar_org*	Bulb type*	Termination*
CC	48.53782	34.17647	13.01681	26.82353	5–6	63.82353	1–2	26.04202	10.739496	2.1	1, 6, 3	1–2	3–5
HD	50.34513	36.46903	13.52212	32.54867	5–6	72.63717	1–2	27.43363	11.920354	2.3	1, 6, 3	1–2	3–5
Peninj	44.55882	32.06373	11.99510	27.79902	6	43.00980	2–3	18.59804	7.789216	2.7	1, 6, 7	2–3	1–2

* Most abundant.

CC (Search of Maximal Convexities), HD (Bifacial Hierarchical Centripetal), Peninj (archaeological collection retrieved from the Type Section site complex).

* Categorical variables showing the most frequent factors documented in each of them.

assemblages, and that this could be reflected in differential preservation of determined sizes (as indicated by the first component).

The issue at stake here is to what extent this archaeological collection can be used to scientifically infer that hominins were predetermining their products across the reduction sequence. This would first imply extraordinary cognitive skills on behalf of the Early Pleistocene stone knappers, and second extraordinary contextual behavioral characteristics for such a strategy to have been adopted. Hence the importance of providing scientific support for these types of interpretations.

Important information can be gained if, rather than stressing the overall similarity of the experimental and archaeological samples by considering global sample variance, emphasis is instead put on finding meaningful differences that can be used to split the sample into groups. For this purpose, a CT is a useful statistical method because it uses algorithms that produce partitioning by the overall probability of misclassification. Optimization is selected for each split instead of aiming at optimizing the whole tree. The application of a CT to the samples in hand produced an exploratory tree in which as many as twenty splits were produced, involving a very small fraction of variance. When the tree was pruned, using "xerror" values (see "Methods"), a final tree with seven splits was produced (Figure 9.3A). The CT selected two variables as the most influential when determining the differences between the archaeological and the experimental samples: bulb and initiation. These variables also were detected as important by the PCA (see Figure 9.2B,D). They were disregarded in the PCA, however, because they became significant only in a multicomponent solution and only in a small fraction of the total variance. When the bulb is type 3 (absent), however, it seems it is more common in the archaeological sample than in the experimental samples. Likewise, when termination is 2 or lower (i.e., wedging or hertzian), it is more frequently documented in the archaeological sample. The remaining variables selected (width, weight, exterior platform angle, and edge length) are useful to differentiate between both experimental samples, according to specific value ranges (see Figure 9.3A).

CT uses splits on raw data in a nonparametric way. If the goal is to seek the main sources and directions of variation in the data, using principal component scores instead of raw data would be a better predictor for classification. If a CT based on PCA is performed, the resulting tree shows some interesting results (Figure 9.3B). The first splits occur in component 15 and component 11, which in the PCA shows the highest loadings on the variable's flake width and weight for component 15 and number of dorsal scars, bulb, and initiation, as well as weight for component 11. The following split on component 2 (dorsal pattern and Toth's flake type variables)

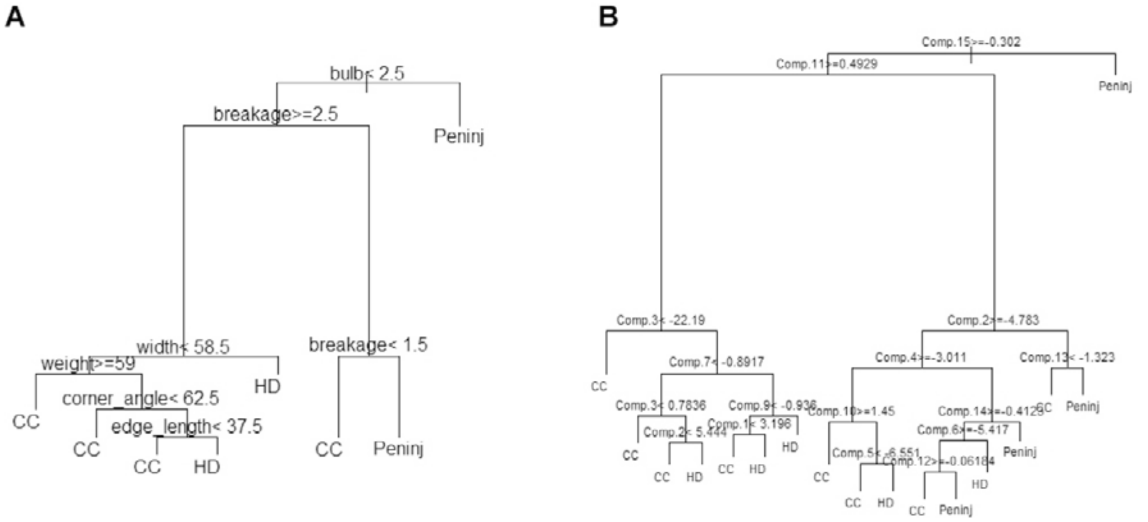

FIGURE 9.3. **A**, Classification tree of the three comparative groups: the search for maximal convexities (CC) and the hierarchical discoid (HD) experimental methods, as well as the Type Section archaeological data (Peninj). The depth of the branches is proportional to the improvement in the fit. The first split on the bulb type produces the largest reduction in the RSS (residual sum of squares). The initiation type is the second split and the most discriminatory variable. Positive answer to the proportion shown in each variable determining a split is indicated in the left branch. Key: initiation (breakage), flake width (width), flake weight (weight), external platform angle (corner_angle), length of the edge (edge_length). **B**, Classification tree based on the commonalities for each factor in a principal component solution. Each component (comp.) is shown with their respective discriminatory threshold values.

and component 3 (technical axis- and striking platform-related variables) determines differences between experimental and archaeological samples only for the former component (as also displayed in Figures 9.2B,D). There, subsequent splits on components 12 (platform and Toth's flake type variables), 13 (flake thickness and platform width), and 14 (flake length and width) can be used for differentiating the archaeological sample from the experimental ones. In sum, the components that can be used to differentiate the Peninj materials from the experimental models account for a total of 20.4% of the total sample variance (component 15 = 0.9%; component 11 = 2.8%; component 2 = 11.6%; component 12 = 2.3%; component 13 = 1.8%; component 14 = 1%). This is an extremely small amount of variance that can be reliably used to differentiate the Peninj materials from both experimental samples. This analysis, however, supports the use of dimensional variables (flake length, width and thickness) as well as dorsal configuration and bulb, initiation, and Toth's flake types. This CT analysis has shown that only about 20% out of the total variance that can be potentially used to classify the three assemblages (that determined by specific values of the variables selected) is useful to do so, because of the intense overlap of the samples.

The selection of the most relevant discriminant variables by the CT analysis is similar to the variables produced by a MDA (Figure 9.4). When variables alone (regardless of specific value ranges) are considered in an analysis aiming at maximizing intergroup differences, an exploratory MDA produced a two-discriminant factor solution in which Factor 1 accounted for 96% of the between-group variance and Factor 2 explained the remaining 4%. The variables determining between-group differences for the "lda" function" are: bulb, Toth's flake types, and flake initiation (breakage; Table 9.7). The number of scars on the dorsal side and platform thickness are a distant second set. For the "discrimin" function, the selected variables are bulb, weight, platform thickness, flake initiation (breakage), and Toth's flake types. Other dimensional variables are predominant for the second discriminant factor (Table 9.7). Both experimental sets have a higher proportion of present and diffuse bulb types, whereas the Type Section archaeological collection presents a higher abundance of diffuse and absent bulb types (Table 9.6). Likewise bending initiation, followed by wedging/bending, wedging/hertzian, and bending/hertzian are more abundant in the experimental models, whereas wedging and hertzian are more common in the archaeological sample (Table 9.6). Platforms are thinner in the archaeological collection than in the experimental samples. Cortical flake types are also substantially more abundant in the experimental

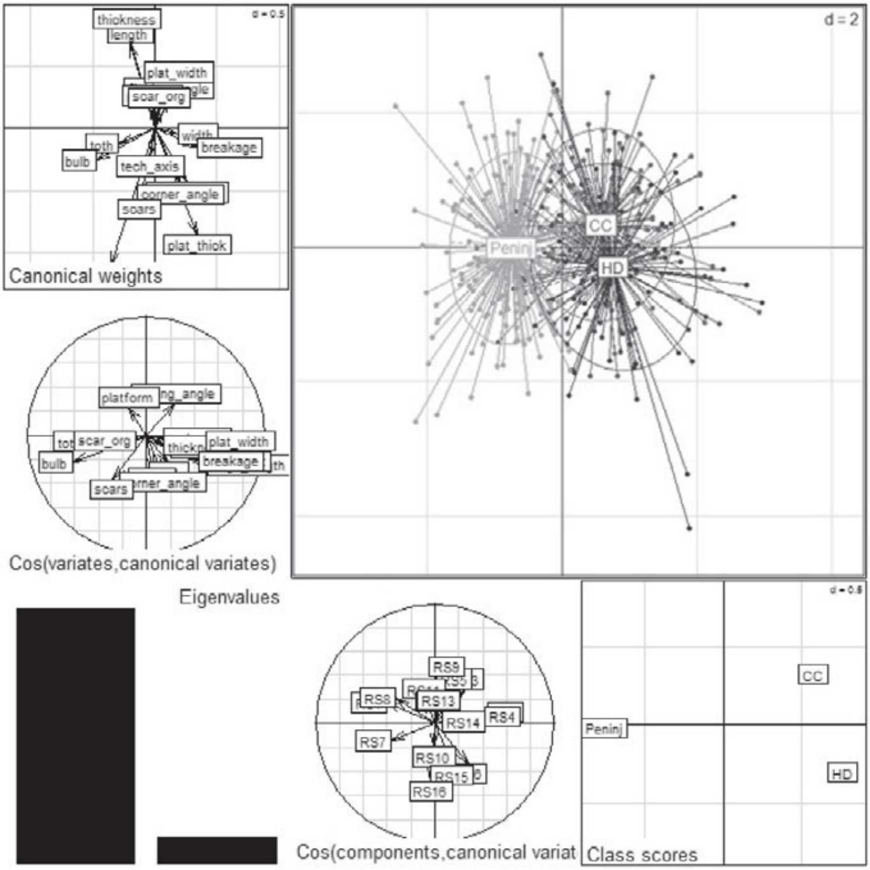

FIGURE 9.4. Multiple discriminant analysis (MDA) showing the canonical weights of each variable (*upper left*), the loading scores for the data of each comparative sample (search for maximum convexities [CC], hierarchical discoid [HD], and the Type Section archaeological data [Peninj]), including their 95 confidence interval ellipse (*upper-middle right*), the correlations between normal and canonical variables (*middle left*), the eigenvalues of the two-dimension solution (*lower left*), the correlation between the canonical variables and their PCA values (lower middle), and the centroids of the canonical variables divided per class according to each of the comparative samples (*lower right*). The discordance between the canonical weight and the correlations between normal and canonical variables graphs suggest limited discriminatory value of the solution. Label keys: flake thickness (thickness), flake length (length), platform width (plat_width), platform thickness (plat_thick), length of the edge (edge_length), technical axis (tech_axis), external platform angle (corner_angle), initiation (breakage), Toth's flake types (toth), number of scars on the dorsal side (scars), dorsal pattern (scar_org).

TABLE 9.7. *Coefficients of discriminants based on a standard linear discriminant analysis**

	lda		discrimin		lda		discrimin		mda		Wilk's lambda	F test	p
	LD1	**LD2**	**LD1**	**LD2**	**LD1**	**LD2**	**LD1**	**LD2**	**NLD1**	**NLD2**			
tech_axis	0,013	0,048	–0,029	–0,224	0,512	–0,656	0,188	–0,277	–0,002	–0,001	0,912	62,79	0.000e
length	0,015	0,057	–0,198	0,687	–0,179	0,553	–0,554	1,964	0,011	0,007	0,801	53,534	0.000e
width	–0,016	–0,005	0,192	–0,041	–0,206	–0,225	–0,461	–0,578	–0,001	–0,008	0,675	46,859	0.000e
thickness	0,016	0,117	–0,217	0,808	–0,041	2,579	–0,029	2,141	0,226	0,007	0,604	41,107	0.000e
weight	0,016	–0,039	–0,356	–1,099	–0,102	0,121	–0,356	–1,099	–0,006	0,008	0,583	33,278	0.000e
toth	0,338	–0,028	–0,289	–0,119	1,968	0,131	1,501	0,115	0,303	0,169	0,477	17,202	0.000e
edge_length	–0,011	–0,013	0,255	–0,433	–0,101	–0,072	–0,478	–0,394	–0,002	–0,005	0,891	58,324	0.000e
corner_angle	0,002	0,021	0,212	–0,451	–0,118	–0,221	–1,435	–3,081	0,001	0,021	0,911	62,788	0.000e
flaking_angle	0,011	0,031	0,117	0,246	–0,011	0,223	–0,155	3,554	–0,002	–0,003	0,831	56,877	0.000e
platform	–0,019	0,308	–0,013	0,184	1,719	–0,977	0,511	–0,335	0,057	–0,012	0,547	30,155	0.000e
plat_width	–0,025	0,025	0,195	0,373	0,342	0,235	0,331	0,219	–0,013	0,003	0,509	24,436	0.000e
plat_thick	–0,056	–0,139	0,339	–0,857	–1,012	–1,761	–0,746	–1,493	0,278	0,266	0,499	22,098	0.000e
scars	0,134	–0,552	–0,119	–0,572	–0,746	–4,106	–0,201	–1,275	0,569	0,073	0,489	20,247	0.000e
scar_org	–0,021	0,091	0,041	0,171	0,137	–0,358	0,032	–0,097	0,016	0,011	0,524	27,169	0.000e
bulb	0,989	–0,354	–0,47	–0,252	8,318	–1,997	2,135	–0,591	0,499	0,038	0,472	15,987	0.000e
breakage	–0,337	–0,132	0,337	–0,152	–1,541	0,256	–0,838	0,161	0,344	0,167	0,482	18,653	0.000e

* Using the discriminant function "lda" in MASS and "discrimin" in ADE4, and nonlinear mixture discriminanat analysis, using the "mda" function in MDA. Wilk's lamba for linear discriminants, including the F-test and p values are also shown. For p, "e" indicates the presence of variable number of zeros to the right. LD, Linear Discriminant. NLD, Non-Linear Discriminant.

collections than in the archaeological sample, which is dominated by Toth's flake type 6. As explained above for the PCA, the differences detected in Factor 2 regarding flake dimensions show that the experimental flakes have larger mean and median values than the archaeological flakes. The latter show slightly higher mean values for the number of previous flakes on their dorsal surfaces (Table 9.6). These differences can be easily explained by the fact that one deals with fragmented reduction sequences in the archaeological assemblage. Furthermore, the lower proportion of flakes in the Type Section assemblage retaining cortical areas can also be explained by the same reason and/or different original mass of the flaked cores.

For this reason, it could be argued that the differences detected by the MDA could be more artificial than real, because they could be related to the dimensions of the flakes preserved in the archaeological collection, depending on the stages that they represent of the (incomplete) reduction sequence. To test this hypothesis and overcome in part the bias introduced by differential reduction sequences and their effect on flake sizes in the archaeological versus experimental samples, if dimensional variables are tallied according to mass (weight) or are log-transformed (also making sure that they are homoscedastic and normally distributed), the resulting MDA shows that differences are substantially smaller than previously inferred (Figure 9.5). A two-component solution shows that 85% of the between-group variance is attributed to the first component and the remaining is accounted for by the second component. In this case the five most discriminatory variables are: bulb type, Toth's flake types, striking platform types, flake initiation and platform thickness (according to the "lda" function), and the same variables but with exterior platform angle instead of platform type if using the "discrimin" function. These two procedures show a higher divergence regarding the variables selected for the second factor, which given that it only explains about 15% of the between-group variance does not seem to be very relevant. The "lda" function emphasized the number of previous dorsal flaking, flake thickness, bulb, platform thickness, and the platform type as most discriminant, whereas the "discrimin" function used most of the remaining variables, emphasizing exterior platform angle, interior platform angle, the dimensional values (length, thickness and weight), as well as platform thickness and numbers of dorsal flaking (see Table 9.7). Therefore, the log-transformed sample shows that the selection of bulb and initiation types, as well as Toth's flake types, number of scars on the dorsal surface and platform thickness are not related to their differential presence in a fragmented reduction sequence (as reflected in dimensional values) in the archaeological sample.

As a further support to this interpretation, in the confirmatory MXDA, a two-component solution produced a first factor that explained 89% of the sample variance. This shows a better solution and more variance explained than the previous MDA test. The variables that explained most of the differences within the sample are, in order, number of scars on dorsal surface, followed by bulb type, flake initiation (breakage), Toth's flake types, and platform thickness. This virtually reproduces the results obtained by the MDA, showing that the use of nonnormal discrete variables in the latter did not bias the final solution (Table 9.7).

These discriminant models are only moderately valid, however, because they show an error of classification of 38% in the MDA and 34% in the MXDA; that is, almost four out of every ten flakes are misclassified. This is especially relevant when observing the percentage of misclassifications of both experimental sets within the Peninj cluster: 48% for the search for maximum convexity (SMC) model and 46% for hierarchical discoid (HD) model. Only 36% of flakes belonging to the SMC model and 30% of those from the HD model were correctly classified. This shows a high degree of misclassification owing to the overall overlap of the three analytical samples.

Technical analysis of the archaeological sample

Cores. We have observed important differences between previous diacritical diagrams (de la Torre 2002, and Mora 2009) and our own. Diagnosis of scar directionality in some of the archaeological materials retrieved from the Type Section of Peninj might be subject to discrepancy, as in many technological or typological approaches (Hermon and Niccolucci 2002). Thus, we are aware that different lithic analysts might reach different conclusions regarding the organization of negative scars in cores. A paradigmatic example of the differences between de la Torre's diacritical analyses and our own can be found in a specimen retrieved from ST4 site, however (Figure 9.6). In various works, these authors (de la Torre 2002: 201, 2009: fig. 6.3, et al. 2003: fig. 11.3, and Mora 2009: fig. 7.27) have interpreted this object as an example of a hierarchical core in an advanced phase of reduction. Their diacritical reconstruction shows a hierarchical core, in which a plane of intersection bisects different and asymmetrical surfaces: an exploitation area where negative scars follow a centripetal pattern and a subordinated surface, showing a set of small extractions on the whole perimeter of the piece (a common trait seen on preparation surfaces of complex cores in an advanced stage of reduction). After the object was cleaned with a hydrochloric acid

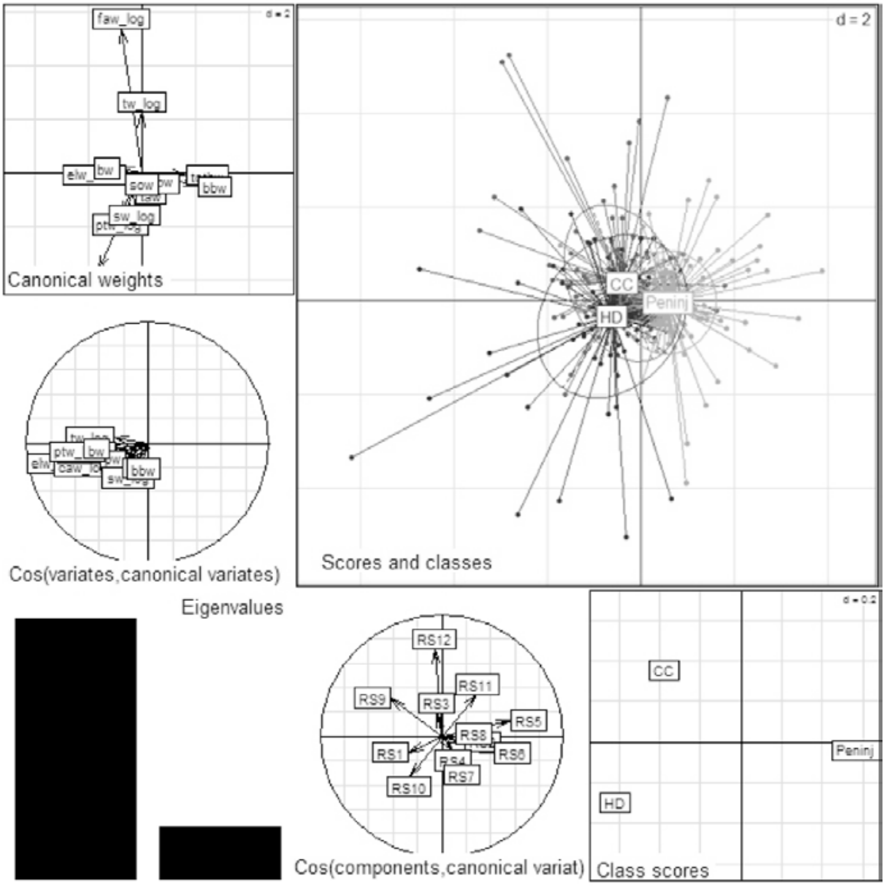

FIGURE 9.5. MDA on log-tranformed variables showing the canonical weights of each variable (*upper left*), the loading scores for the data of each comparative sample (search for maximal convexities [CC], hierarchical discoid [HD] and the Type Section archaeological data [Peninj]), including their 95 confidence interval ellipse (*upper-middle right*), the correlations between normal and canonical variables (*middle left*), the eigenvalues of the two-dimension solution (*lower left*), the correlation between the canonical variables and their PCA values (*lower middle*), and the centroids of the canonical variables divided per class according to each of the comparative samples (). The discordance between the canonical weight and the correlations between normal and canonical variables graphs suggest limited discriminatory value of the solution. Label keys: flake thickness (tw_log), flake length (lw_log), flake weight (ww_log), platform width (pww_log), platform thickness (ptw_log), length of the edge (elw_log), technical axis (tww), external platform angle (cw_log), flaking angle (faw_log), initiation (bw), Toth's flake types (tothw), number of scars on the dorsal side (sw_log), dorsal pattern (sow), bulb types (bbw).

solution to enable analysis (concretions, still adhering to the artifact prior to our analysis, prevented reliable interpretation), however, a diacritical study and a high-resolution 3D scan confirm that even signs of anthropogenic manipulation of this item are highly questionable.

The set of diacritical schemes reconstructed for the present study showed that at the Type Section, different reduction sequences were carried out, and that this operational variety correlates with the different support formats available in the area (spherical and subspherical, hemispherical and angular). According to these core formats, the collection can be divided into two different exploitation groups (Figures 9.7, 9.8, 9.9, and 9.10):

1. Massive supports (spherical, subspherical and polyhedral morphologies). Reduction was based on different unipolar series, intersecting orthogonally (Figures 9.7.1, 9.7.2, 9.8.1). A few cases show an incipient centripetal organization of negative scars. Other examples show pairs of negative scars that intersect perpendicularly and tend to overlap as a result of core rotation. Occasionally, relatively large negative scars can be observed in advanced phases of the exploitation sequence, although we have documented examples in which the latest series are related to small negative scars that could be the result of use or other causes (hammering activities, knapping failures, artifact recycling).
2. Medium-sized hemispherical and flake supports. In these cases, exploitation starts with parallel short series (two or three negative scars) that converge in a bipolar manner. The exploitation continues first with alternate isolated detachments and then with new unipolar series that, in some cases, converge again in a bipolar manner (Figure 9.8.2, 9.9.1). The latest phases of the reduction sequence show centripetal and orthogonal schemes (Figures 9.9.2, 9.10.1).

Raw material quality seems to have played an important role in the operational schemes carried out by hominins at the Type Section area. We have identified the following groups based on associations between raw material and exploitation patterns: (1) Group 1: Poor quality basalts (mostly related to massive supports) show a partial alternation of negative scars and a remarkably low degree of exploitation (Figures 9.7.1, 9.7.2, and 9.8.1). (2) Group 2: Other basalts and nephelinites of medium to good quality (hemispheric and flake supports) show a medium degree of exploitation through unipolar and orthogonal series (Figure 9.9.1). (3) Group 3: Very good and optimal basalts were intensively exploited in centripetal, orthogonal, and orthogonal-polyhedral sequences (Figures 9.8.2, 9.9.2, 9.10.1) Group 1 seems

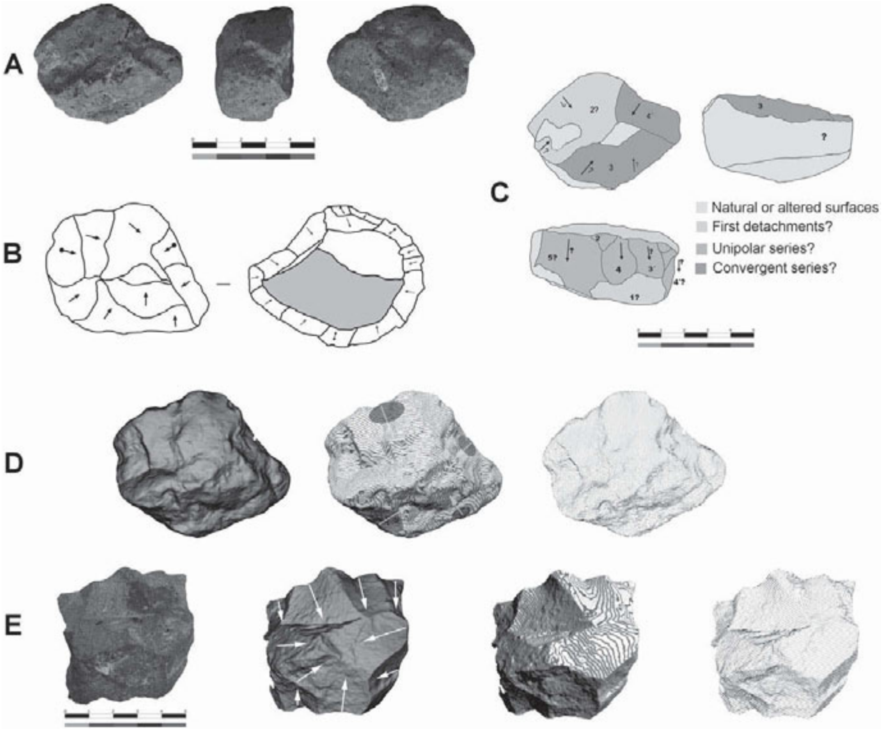

FIGURE 9.6. Divergent interpretations of a medium quality (type 3) nephelinite specimen retrieved from ST4 site (ST4S-16). **A**, Two-view photograph of the object. **B**, De la Torre's diacritical reconstruction (redrawn from de la Torre 2002: 201, scale not provided) presents a hierarchical bifacial centripetal core in an advanced/exhausted stage of exploitation (see also de la Torre et al. 2003: figure 11.3, and Mora 2009: figure 7.27): the main exploitation surface (aimed at producing flakes) shows a centripetal pattern, whereas the subordinate surface shows a very intense preparation (affecting the whole perimeter of the piece) of the striking surface. Both surfaces are shown to be hierarchical as the roles assigned to them in the diacritical scheme seem not to be interchangeable. **C**, Our diacritical reconstruction of this specimen shows, however, quite a different diagnosis. Most of the purported preparation surface is natural or heavily altered, showing no signs of preparation negative scars. The purported exploitation surface suggests a conflictive interpretation. It shows two different series of possible detachments (one probably convergent), although highly problematic. Some of these negative scars are dubious, and we do not rule out the possibility that they do not bear signs of anthropogenic manipulation. On the orthogonal plane we have identified a possible unipolar series, although some of the negative scars are equally dubious. Owing to the disparate diacritical interpretations, we have decided to use a Konica Minolta Vi 910 laser scanner to overcome interpretative bias related to *de visu* diagnosis. **D**, 3D scanning of the specimen (precision 22μ) with a postprocessing analysis of the purported exploitation surface based on topographic attributes. The final outcome is a digital topographic model (DTM) that supports our diacritical scheme: the topographic reconstruction would favor the existence of four short and very problematic negative scars, arranged in a convergent manner although not centripetal *sensu stricto*. **E**, For comparison purposes, a 3D scanning (precision 27μ) of an experimental core replicated using the same raw material (experiment 8, aimed at a hierarchical centripetal strategy) has been undertaken. The DTM shows a rather different topographic outcome in which clear radial negative scars can be observed.

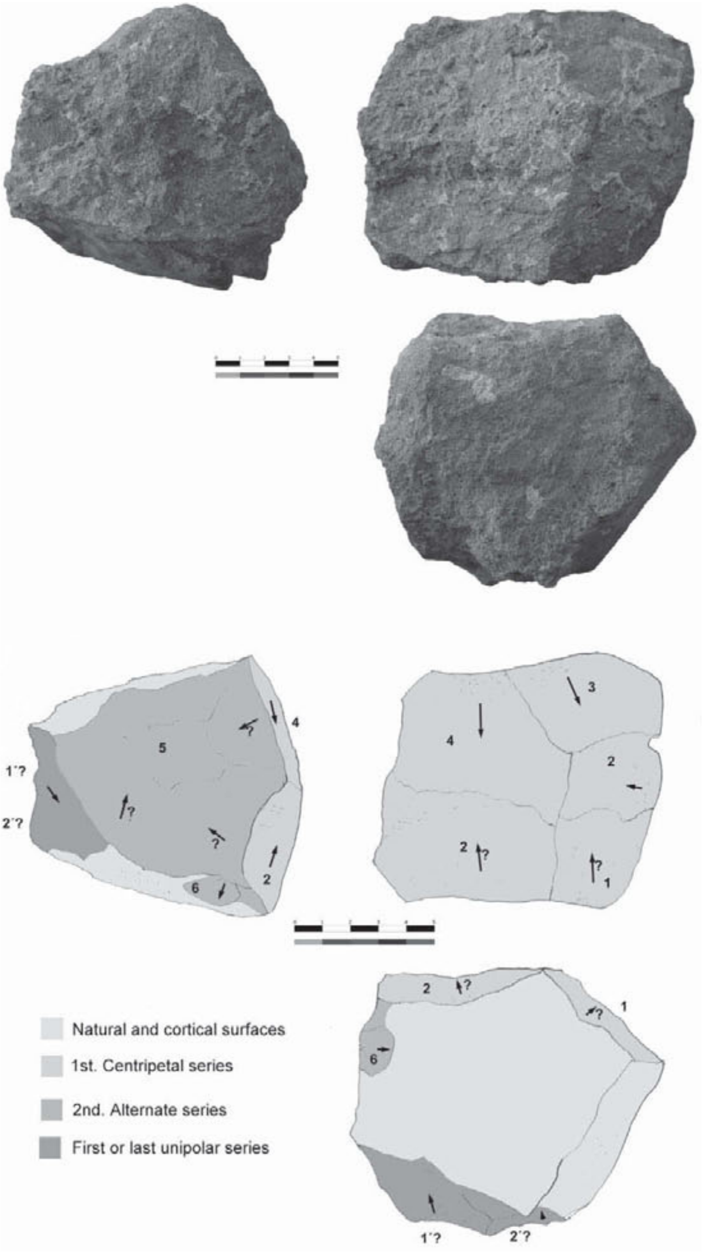

FIGURE 9.7. Photographs and diacritical schemes of cores selected by de la Torre (2002) and Mora (2009) as representative examples of the various phases hypothesized for the bifacial hierarchical centripetal reduction method. 1. ST31 A-28 (Example of Stage 1). Large basalt cobble (97 × 93 × 85 mm, 1,164 gr) of very low quality (type 1) showing orthogonal series of detachments on two different surfaces. Note the polyhedral-like morphology and the absence of the unifacial centripetal strategy previously proposed. 2. ST4 U0–33 (Example of Stage 3). Very low quality (type 1) and irregular basalt

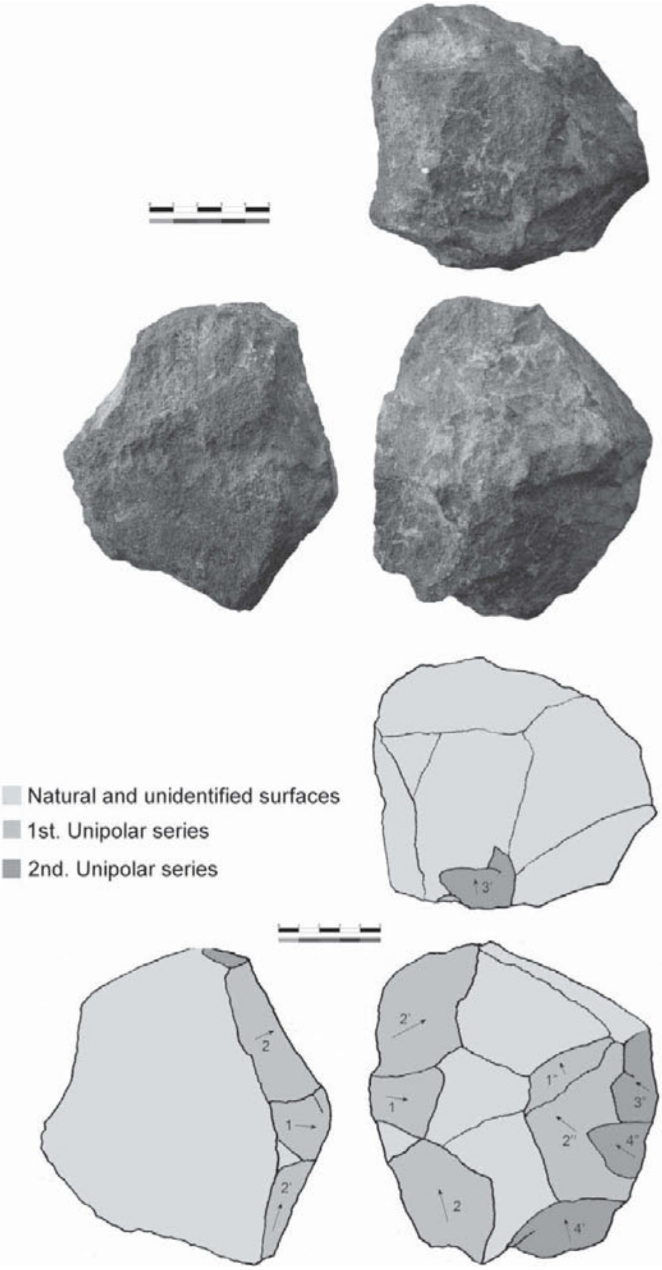

FIGURE 9.7 (*continued*) support (114 × 96 × 95 mm, 1,073 g) showing unifacial orthogonal series on one surface. Note the polyhedral shape and the lack of preparation on transverse and sagittal planes as previously suggested. The diacritical schemes show different colours (representing discrete knapping series) organized in a chronological sequence starting from time 1 (natural and unmodified areas) to time X (the final knapping series recognized in the core). Arrows indicate blow directions, and numbers indicate the proposed chronological sequence of negative scars within each knapping series.

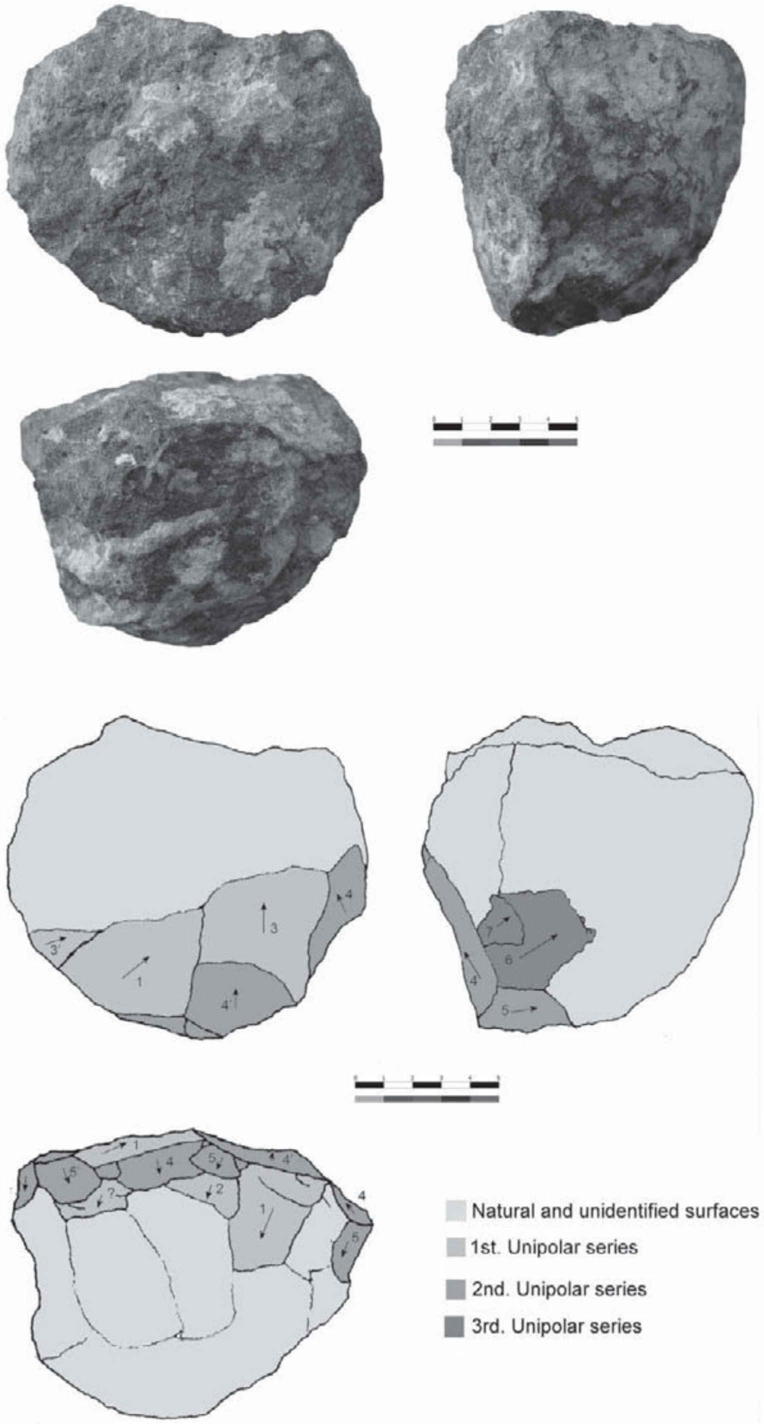

FIGURE 9.8. Photographs and diacritical schemes of cores selected by de la Torre (2002) and Mora (2009) as representative examples of the various phases hypothesized for the

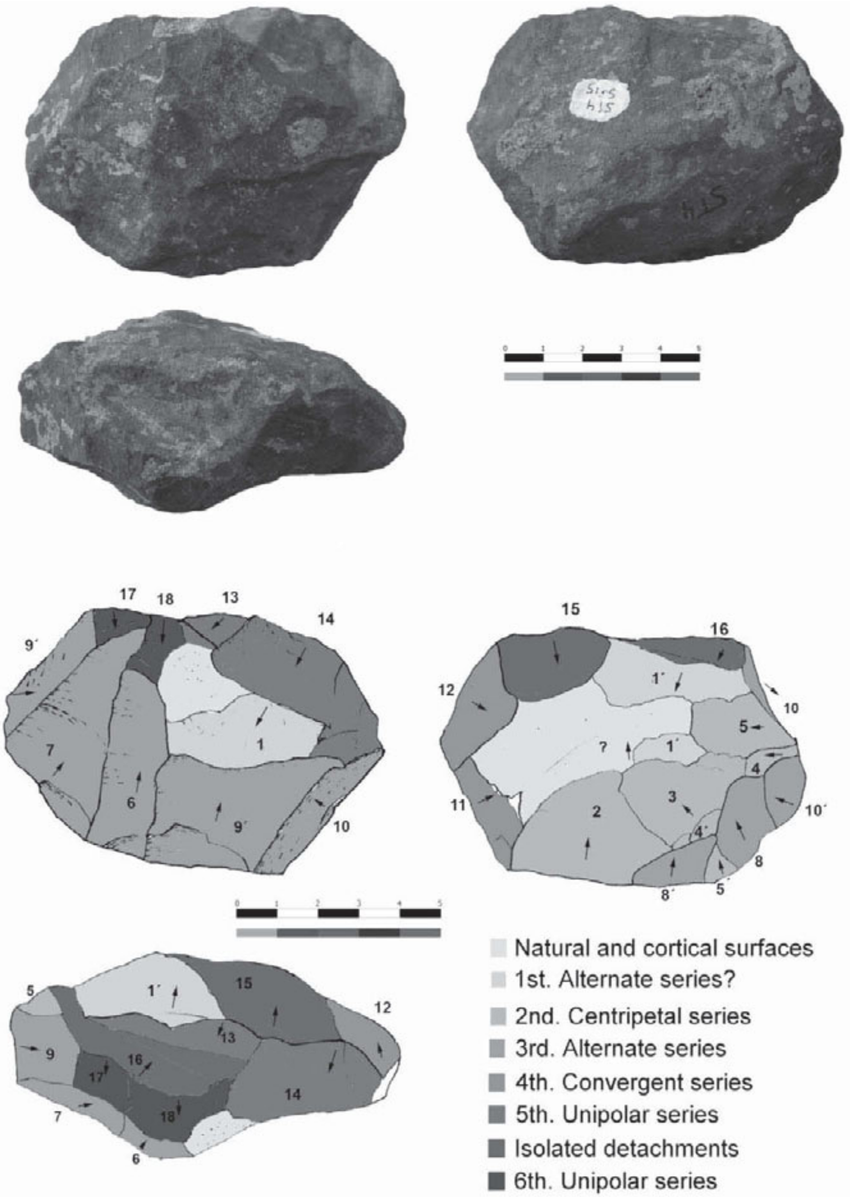

FIGURE 9.8 (*continued*) bifacial hierarchical centripetal reduction method. 1. ST2–12 (Example of Stage 4). Very low quality (type 1) basalt core on a block support showing bifacial unipolar alternate series produced from an edge (99 × 84 × 82 mm, 780 gr). The specimen shows a final polyhedral shape and not a fully hierarchical core as previously proposed; 2. ST4 S-15 (Example of Stage 5). Medium quality (type 3) nephelinite core (88 × 60 × 40 mm, 303 g), probably on a hemispherical flake support, showing alternate bifacial series (discoid-like) from two opposed edges. No signs of core hierarchization (de la Torre and Mora 2009: fig. 7.55) are deduced from our diacritical analysis.

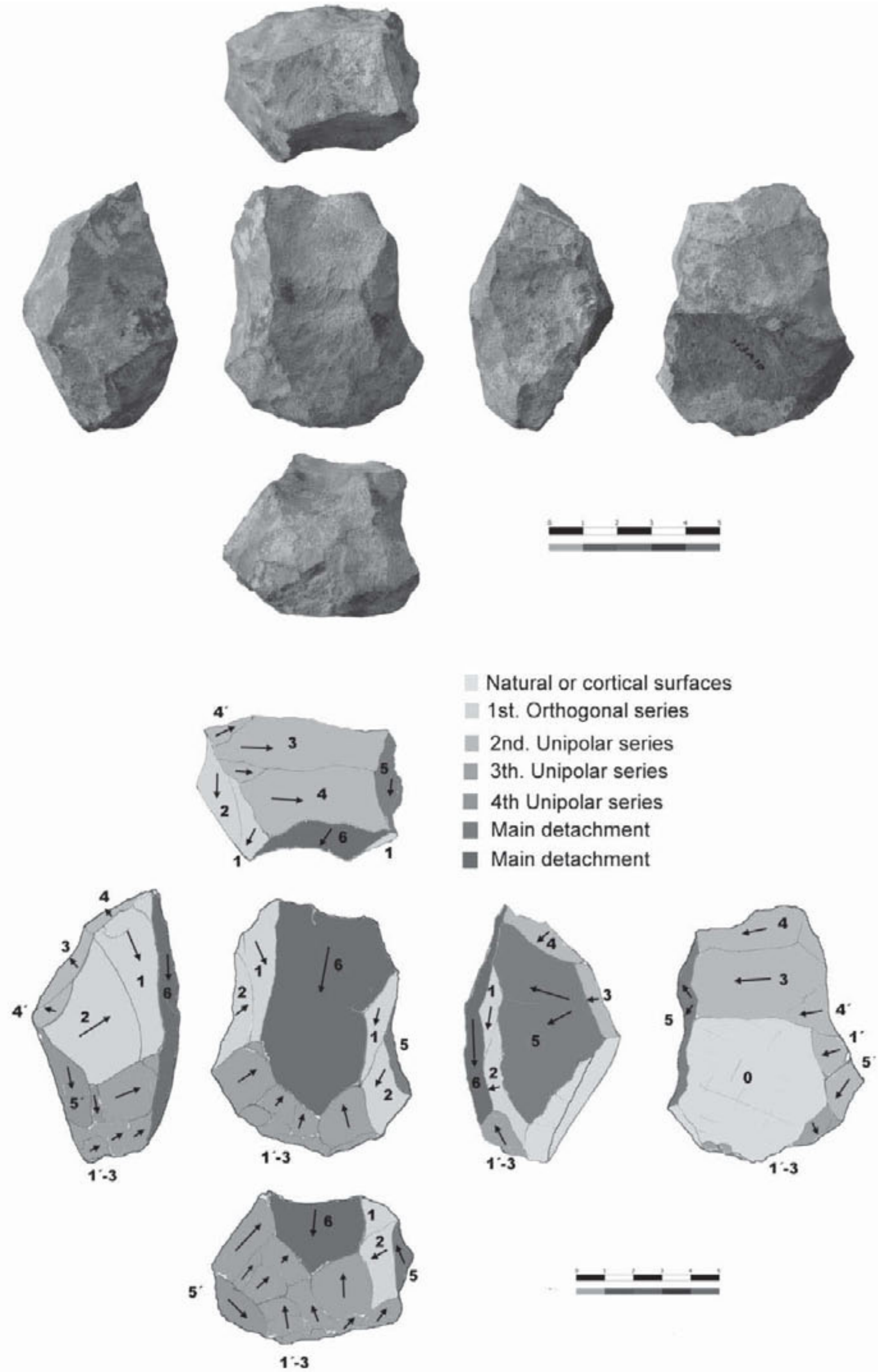

FIGURE 9.9. Photographs and diacritical schemes of cores. 1. Good quality (type 4) basalt core on an angular fragment support (ST3 A10). It shows an orthogonal exploitation

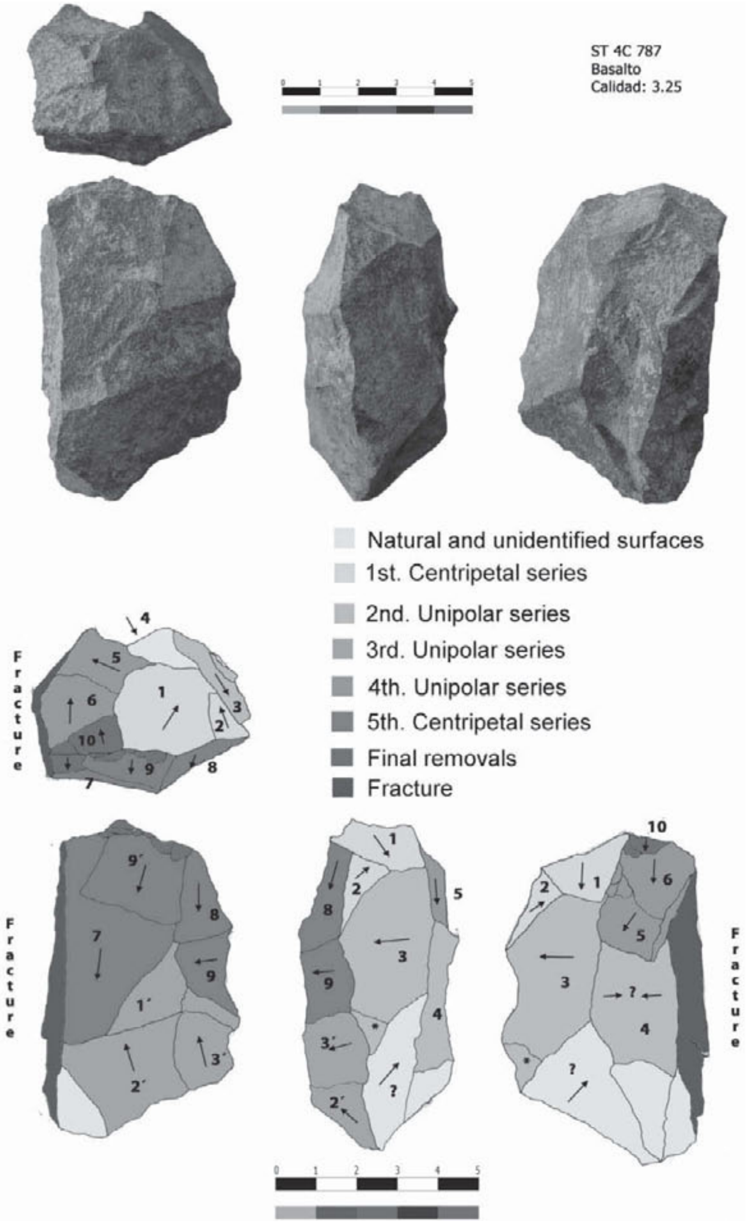

FIGURE 9.9 (*continued*) through a combination of unipolar series and isolated detachments. 2. Good quality (type 4–5) basalt core (ST4C 787). Is shows an orthogonal/discoid exploitation, by combining centripetal and unipolar series.

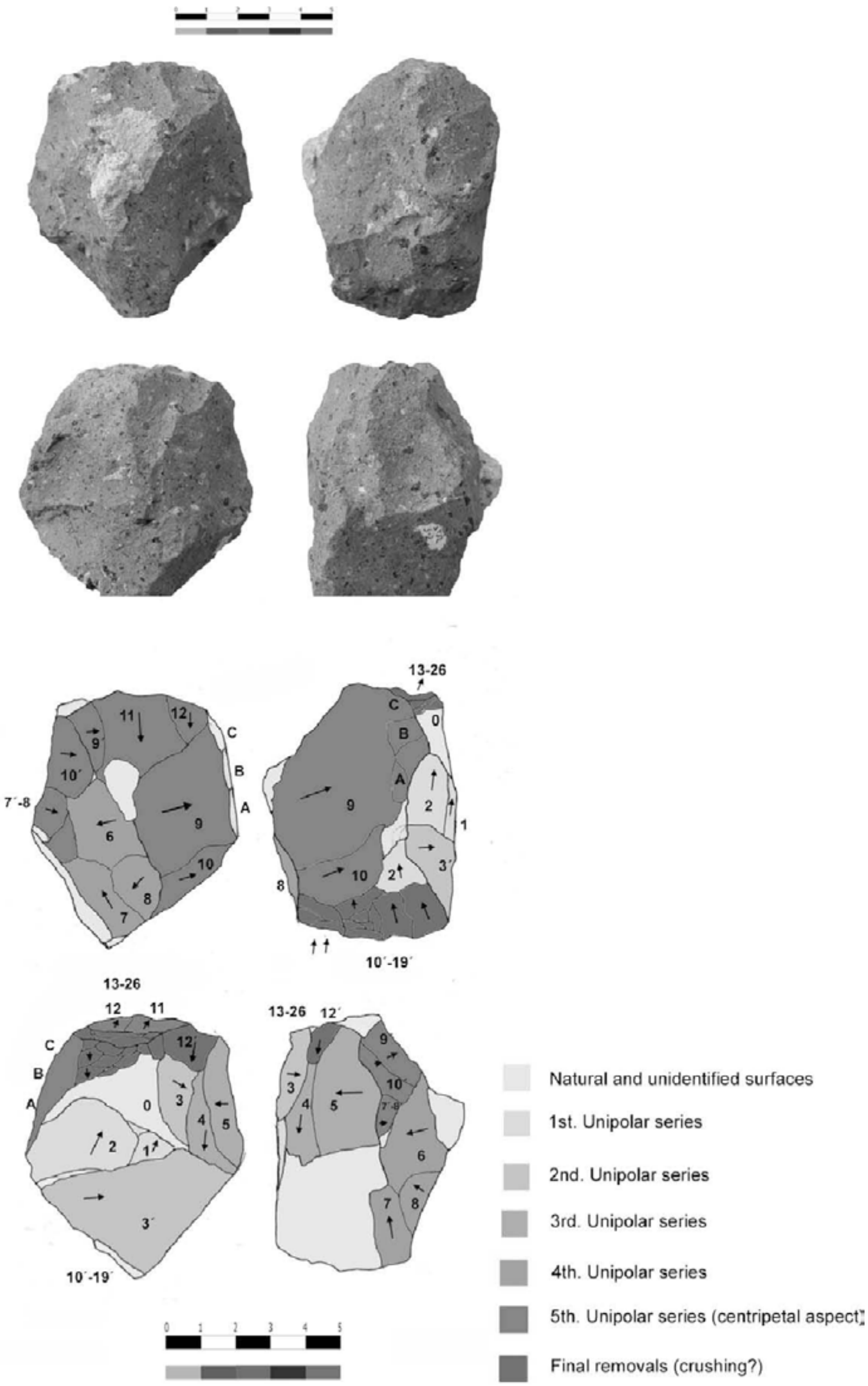

FIGURE 9.10. Photographs and diacritical schemes of cores. 1. Core on good quality (type 4) nephelinite (ST32 S2), showing orthogonal/discoid-like exploitation by the combination of unipolar series. 2. Very good quality nephelinite (type 4–5) large flake or angular

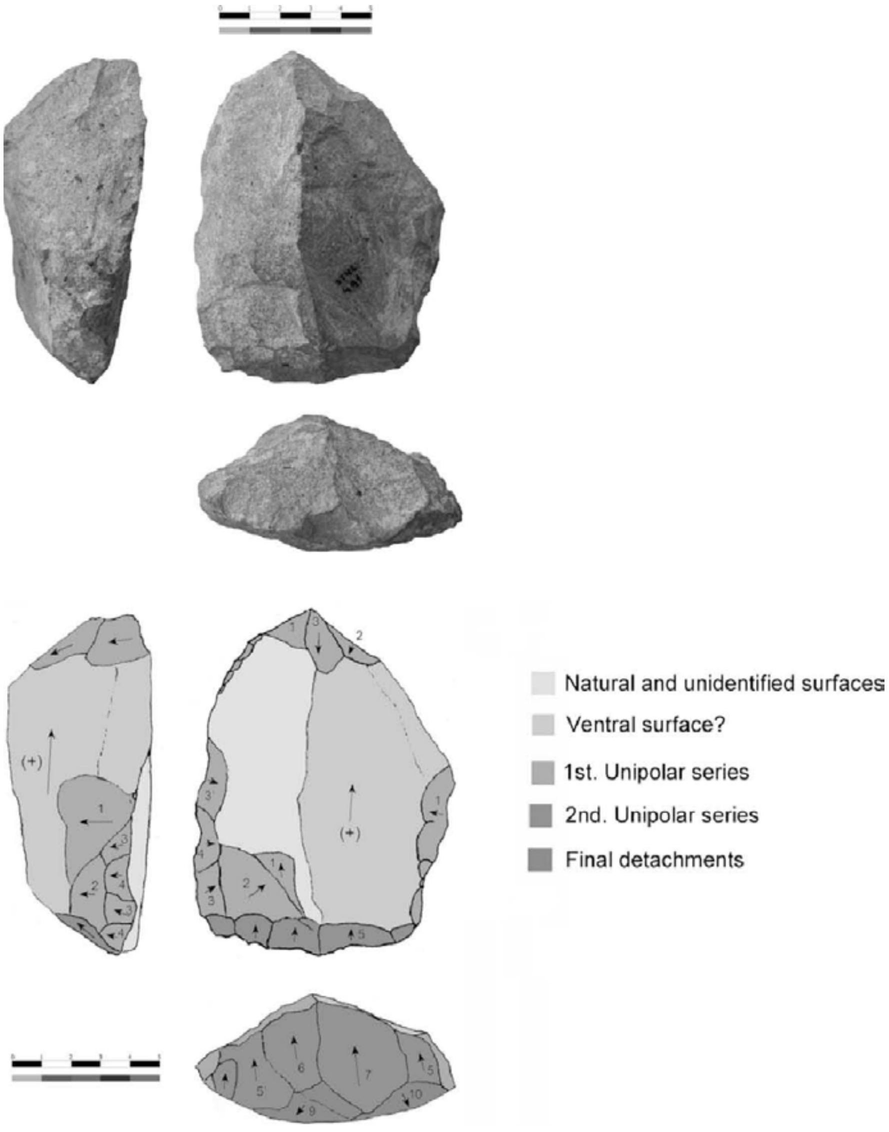

FIGURE 9.10 (*continued*) hemispherical block support (140 × 78 × 45 mm). The core is in an initial phase of exploitation, showing unipolar series from a flat (ventral?) surface (ST46 491).

not to have any link with the others: spheric and subspheric relatively heavy volumes show limited exploitation (less than 10% of core mass) and could have been related, among other tasks, to percussion behaviors. Several medium-sized nephelinite cobbles have been used in percussion activities after being successfully exploited as cores. It is worth bearing in mind that some of the cores included in the earliest stages of the model belong to Group 1 (Figures 9.7.1, 9.7.2, and 9.8.1). Groups 2 and 3 might be interconnected and might represent different parts of the same reduction scheme (initial reduction phase and a full exploitation phase; Figure 9.9).

In sum, taking into account the constraints imposed by support morphology and raw material quality, the diacritical schemes show that exploitation at the Type Section was driven by the technical principle of discontinuous alternation of different surfaces (preferentially two) in the knapping process, *sensu* the "base algorithm" proposed for the Clactonian reduction method by Forestier (1993). Knappers were systematically undertaking the unipolar exploitation of appropriate striking surfaces as intensively as possible. In the course of this reduction scheme, the generation of new appropriate striking platforms favored the exploitation of new, adjacent surfaces. This technical principle differs substantially from the discontinuous discoid technique (in which each new blow strikes on the edge of previous negative scars) or the Levallois technique (in which striking platform preparation shows a complex set of technical gestures; Table 9.5). In the Type Section area, recurrence of this discontinuous alternation resulted in orthogonal or bipolar schemes, although centripetal discoid in appearance. Although some cores show a clear centripetal organization of negative scars, the only reduction pattern that seems to show consistency is constituted by long knapping series of discontinuous alternation. We see neither recurrent successive exploitation/preparation of surfaces with asymmetrical and noninterchangeable surfaces (representative of more sophisticated hierarchical discoid or recurrent Levallois methods), nor continuous alternation of knapped surfaces (representative of a nonhierarchical discoid method). Furthermore, when the discontinuous alternation seen in the Type Section is taken to a point of intense exploitation, the intersection plane that bisects both surfaces shows a discontinuous rectilinear outline and not a regular zigzag (as would be expected for discoid exploitation).

Importantly, combining the diacritical analysis with a closer look at raw material quality and format shows why the link between specific archaeological cores and the purported reduction model is compromised (Figures 9.7 and 9.8). The examples selected to illustrate the three first stages of the hypothetical model (ST31A-28, ST4U0–33, and ST2–12) are

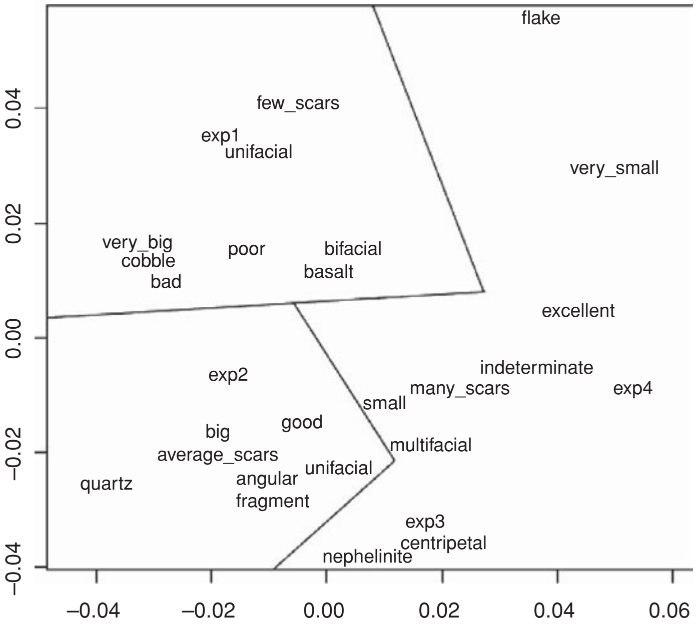

FIGURE 9.11. Multiple correspondence analysis of the factor variables determining the analysis of cores. The variables analyses are type of materials (basalt, quartz, nephelinite), raw material quality (bad, poor, good, excellent), type of support (cobble, angular fragment, flake, and indeterminate), intensity of the exploitation (exp1 [low], exp2 [medium], exp3 [exploited], exp4 [exhausted]), proportion of negative scars (few [<5], average [<10], and many [>10]), exploitation system (unifacial, bifacial, multifacial, centripetal), and core size (very small [<100 g], small [100–300 g], big [300–600 g] and very big [>600 g]). The raw material quality types are categorized relatively.

representative of the poor quality basalts found in the area. Among them, the first specimen (Stage 1 in the model, Figure 9.7.1) has been exploited on a cobble/rounded support, whereas the other two (Stages 3 and 4, Figures 9.7.2, 9.8.1) are related to analogous massive supports. All of them show final polyhedral morphologies. The full hierarchical exploitation phase (Stage 5) is represented by a better quality item exploited on a flake support. These observations show that the purported homogeneous model is in fact disrupted and that the different objects belong to fragments of different operation sequences and are not interrelated.

An MCA yielded a two-dimension solution (Figure 9.11), which incorporates 30.9% of the inertia. In it, cores can be clearly differentiated according to their raw material properties and size. The first dimension is determined by small and very small cores of very good material quality (nephelinite) made from either indeterminate or flake supports, which are intensively exploited and show abundant negative scars, as well as centripetal and multifacial negative scar patterns. The second dimension is determined by the other two types of raw materials. Very large cores are of poor or bad raw material quality, usually of basalt, which are exploited unifacially and bifacially and show few or an average number of negative scars (less than ten). They are only minimally to moderately exploited. The exploitation of large cores is slightly more intense than that of very large cores, and their support type is more frequently good-quality angular fragments of quartz, which are usually unifacially exploited. This comparison of the properties of the different types of raw material and cores shows that there is not a continuous, predetermined, hierarchical centripetal exploitation reduction sequence represented in the Type Section collection, simply because each exploitation phase and type is carried out using different types of raw materials (Figures 9.7 and 9.8).

Detached products. Our technical analysis of the detached objects category includes a sample of 274 specimens. We have not included debris (<20 mm) or flake fragments. Whole flakes constitute the most abundant group (93% of the sample, including a few retouched pieces), although we have also included fragmented flakes in which technical traits were recognizable. Edge and naturally backed flakes are quite abundant in the collection, since 20% of them show an abrupt edge on one side (natural or not). Among them, rejuvenation or edge core flakes represent 4% (n = 11) of the sample studied here. Interestingly, we have identified eight flakes (3%) that could be the result of hand ax or LCT resharpening, showing edge remnants on dorsal surfaces and other traits experimentally interpreted by other authors as the result of handaxe knapping (Goren-Inbar and Sharon 2006). Most of the knapping accidents recorded are related to step fractures (57%), although flake snapping is also well represented by longitudinal fractures, mostly axial or Siret type (21%).

The bulk of flakes (81%) have been produced from a variety of basalt textures and qualities: 16% very poor to poor basalt, 52% medium/average, and 32% good to excellent. Contrary to what has been previously stated (de la Torre et al. 2003: 207), our study does not support a particularly good quality or homogeneity (in terms of porosity, grain size, presence of natural cleavages) of the nephelinites retrieved from the Type Section. Very good, fine-grained specimens represent only 27% of the sample, whereas most of the flakes produced from this raw material (60%) show an average/acceptable quality (pieces that include relatively abundant and coarse pyroxenes in their fabric). Quartz flakes are scarce (3%) and coarse-grained; however, quartz detached pieces show clear signs of conchoidal fracture, and no evidence of bipolar technique has been found in this sample (Díez-Martín et al. 2011). Regarding breakage patterns, hertzian initiation predominates (42%), followed by wedging (21%) and bending (14%). A combination of initiation types is relatively common, however, particularly the association of bending and wedging (21%).

Flakes show a mean maximum length of 44.5 mm and width of 32 mm. Flake modules indicate some sort of size recurrence in the Type Section, as already pointed out by other authors (de la Torre and Mora 2009). Hominins here aimed at producing relatively small flakes, although of note, they were able to produce long and thick flakes (at ST2, for instance, several pieces have a maximum length >90 mm). Mean cutting edge total length is 43 mm (the bulk of the collection falls within the 21 mm–40 mm interval), which is in strong agreement with mean axial dimensions.

The difficulty of clearly distinguishing cortical surfaces in many flakes is a characteristic problem of the Type Section assemblages, as already noted (de la Torre and Mora, 2009: 174). Thus, diagnosis of this trait should be considered somewhat biased toward noncortical surfaces. According to Toth's flake classification (1982), type VI is by far the most abundant type (82% show no signs of cortical areas), followed at a distance by type V (12%). Recognized cortical flakes (types I and IV) represent barely 2% of the collection. Striking platforms show a mean area of 19 mm in width and 8 mm in thickness. Most striking platforms retain no cortex (6% are cortical, 83% noncortical, 3% linear-points, and 8% are broken/absent butts). This sample does not show complex signs of platform preparation, however: 73% of butts are plain, whereas 9% are dihedral and only 1% faceted. This pattern is consistent with a scenario in which hominins would have been taking advantage of previous exploitation surfaces to use them as new striking platforms, in a process much closer to platform alternation rather than to platform preparation. Regarding dorsal surface, most blanks have three previous detachments (41%), although pieces in which only two previous detachments have been identified are abundant as well (31%). Specimens displaying a larger number of previous flaking scars are few: four have been counted in 15%, five in 3%, and six or more in 3% of the sample. Regarding dorsal pattern reconstruction, our analysis does not support de la Torre's counts (et al. 2003: 210, and Mora 2009: 174). Although these authors referred to a considerable number of flakes showing a dorsal radial pattern (as a result of centripetal core rotation), we conclude that dorsal schemes are predominantly related to unidirectional exploitation. Figure 9.12 shows a schematic graphic reproduction of the different patterns observed in this study (see "Methods"). Our study supports a consistent predominance of unipolar directionalities (67%, including unipolar proximal, distal, and orthogonal). When core rotation is recognized, it shows a repeated orthogonal pattern (28%), demonstrating that convincing centripetal core rotation is virtually absent from the sample and once again supporting the important role played by discontinuous platform alternation in hominin knapping strategies.

Discussion

The experimental replication of the hierarchical centripetal model

One of the main goals of the experimental program was to replicate the hierarchical centripetal model, (as described by de la Torre 2002, de la Torre et al. 2003, and de la Torre and Mora 2009). For that purpose, several cores were knapped trying to replicate the same reduction strategy (Table 9.5). The knapping experience that was gathered through our experimental study – in which, apart from those presented in this study, other strategies were replicated as well (Levallois, alternate centripetal discoid, unipolar, and bipolar opposed) – has led to the following main observations.

First, the reduction sequence hypothesized for the bifacial hierarchical centripetal model overlooks the dramatic influence of the natural volumes of the blocks selected in the course of the reduction strategy. Rather, an inflexible and simplistic scheme has been presented that would in theory be mechanically repeated by hominins (as an example of technical anticipation), regardless of other factors such as core morphology and raw material quality. Our experiment shows that reduction strategies that can or cannot be undertaken by the knapper depend highly on these factors.

Second, the theoretical sequence proposed in the bifacial hierarchical centripetal model is neither universal nor univocal, and it can be replicated partially only if very precise conditions are met: namely, good-quality hemispherical supports (a large or medium-sized flake or a hemispherical angular fragment or cobble). Another option to ensure success would be to "create" this morphology through a previous *mise en forme* of alternative, less suitable morphologies. The hemispherical shape would favor the organization of the core volume in two dissymmetric surfaces, a pattern that is not possible for the knapper when the support has a spherical shape and volume. Although technical operations are patterned and constantly

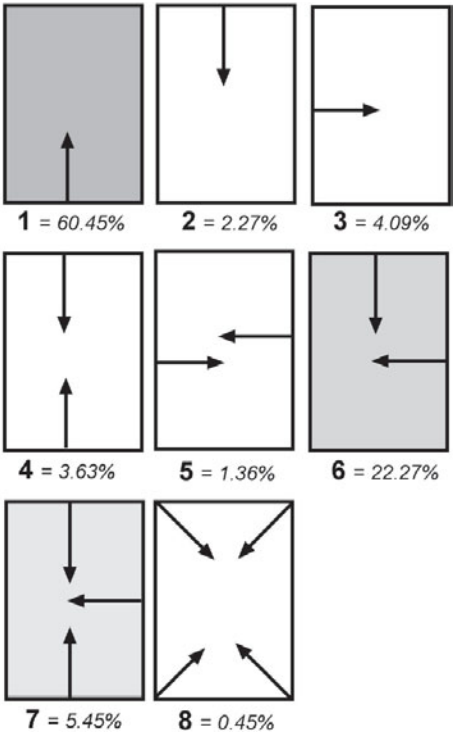

FIGURE 9.12. Percentage contribution of the dorsal scar patterns observed in the archaeological flake sample: 1. Unipolar proximal; 2. Unipolar distal; 3. Unipolar orthogonal; 4. Bipolar opposed; 5. Bipolar transverse; 6. Bipolar orthogonal; 7. Multipolar orthogonal; 8. Multipolar radial.

repeated, if the morphology of the supports is varied, then the results should show a great morphological variety as well. These empirical observations are at odds with the purported model inferred for the Peninj materials, as the Type Section complex lithic collection is basically characterized by a remarkable variety of support morphologies. Furthermore, the archaeological specimens identified by de la Torre and colleagues as representative of the earliest stages of their model are always made from massive, poor-quality spherical supports, whereas the latest stages are represented by fine-grained rocks knapped on flake or hemispherical supports. These differences in raw material quality and support type show that we are concerned with fragments of unrelated processes (as already pointed out) and that this evident heterogeneity among the archaeological materials makes it impossible to fit them into a continuous and coherent sequence.

Third and finally, if the specified conditions are met (use of a hemispherical support with a high degree of textural homogeneity), volume control can be produced, and an asymmetrical organization of the volume can be accomplished. The replication of the theoretical model and its different stages had the following problems, however:

Stage 1: *Unifacial centripetal exploitation*. In our experiment, in which angular fragments or thick flakes were used as supports, prior to unifacial centripetal reduction it was necessary to search for favorable striking platforms and surfaces (related to optimal ridges and convexities). Once this knapping is done, eventually it is possible to reach a full centripetal production phase (hierarchical or not). In our experiment, however, the knapping prior to centripetal exploitation of these volcanic supports is not economical, because it implies a remarkable loss of core mass and produces abundant irregular and snapped flakes (particularly longitudinal axial or Siret fractured blanks).

Stage 2: *Loss of convexities*. This phase is crucial to the bifacial hierarchical centripetal model, because it explains the future actions theoretically taken by the knapper towards the onset of the hierarchical strategy. Recurrent unifacial centripetal exploitation does not imply loss of convexities on the exploitation surface. On the contrary, our experimental replication suggests that at this point, instead of a horizontal exploitation surface (expected if the theoretical model were correct), the exploitation surface tends to maintain and enhance convexity (Eren and Bradley 2009). Thus, it is incongruent to propose that a hypothetical loss of convexity would lead to core hierarchization. Furthermore, if radial exploitation were to lead to the loss of convexities, it would imply that hominins were not aware of simple knapping principles such as the control of striking angles. This fact would be incompatible with the control of other, far more complex technical concepts, such as the precise control of angles and volumes required for core hierarchization.

Stages 3: *Core reactivation*. In our experiment, this phase does not serve the purpose of reactivating the volume of the knapping surface and recovering lateral convexities but produces new striking platforms allowing eventual limitations related to excessive central convexity on the striking platform to be precisely overcome. In several cases (e.g., experiment 1), the first centripetal series generated an extremely convex exploitation surface that encouraged the knapper to use it as striking platform to detach new series from the opposite surface.

Stage 4: *Core hierarchization*. Good-quality basalts from the Natron area are suitable to undertake exploitation models in which the principles of hierarchization and predetermination are present (e.g., we have successfully reproduced typical MSA/Middle Palaeolithic Levallois preferential and recurrent cores). In a final phase of core exploitation, however, we also achieved morphologies similar to hierarchical cores from alternate, nonhierarchical, bifacial centripetal cores. Although in those cores both faces were indistinctly used to produce flakes (not supporting the principle of core hierarchization), the fact that one face is slightly more productive than the other (among other uncontrolled factors) can produce morphologies that are only apparently hierarchical. This is extremely relevant to the interpretation of the archaeological record. At this point, a lithic analyst must take into account a determining factor: core hierarchization is not only related to a given volumetric organization (e.g., surface asymmetry) but also to the role played by the different surfaces in the process of predetermining flakes. Predetermined flake production is thus closely linked to core hierarchization. Hypothetically speaking, the lithic analyst could get insight into the proper definition of predetermined flakes through two main methodological approaches (Dibble and McPherron 2006: 777): (1) technological traits allowing the flakes produced from the exploitation surface to be distinguished from the flakes detached from the preparation surface; (2) functional traits (e.g., use-wear analyses) enabling inferences about which types of flakes are most likely the result of complex predetermination process (following the logic that expensive products would be the most valuable and would be desired for several tasks).

Stage 5: *Hierarchical exploitation*. At this point the centripetal reduction of the core can continue and we can obtain more standardized flakes, at this stage not much core mass is left, and knapping accidents are common. Our observations imply that if we accept the model, then the full production phase in such a hierarchical core would be very limited, because a high amount of raw material would be lost in previous phases of the sequence. This observation is at odds with the economic advantage of this exploitation strategy proposed elsewhere (de la Torre 2009: 110). Raw material efficiency is an important issue in regional settings where, as at Peninj, an intense raw material flow, tool operational transfer, and technological interconnectivity can be detected through the paleolandscape.

In sum, our experimental replication does not support the hypothesis that the Peninj hominins were engaged in knapping through the bifacial hierarchical centripetal theoretical model for the reduction of the cores retrieved at the Type Section complex. The problems related to this proposal can be summarized, within the different stages of the reduction sequence, as follows: (1) Initial stage: although support morphology is crucial to its feasibility, the model overlooks the role played by morphological constraints and the variable supports observed in the archaeological sample; (2) Middle stage: core hierarchization is not the consequence of any loss of convexities on the exploitation surface; (3) Final stage: we have reproduced cores that morphologically can be mistaken for hierarchical but that are not technically hierarchical, because both surfaces have played identical roles in core exploitation (Figure 9.13).

The statistical analysis

A multivariate statistical analysis provided valuable information about the characteristics of the experimental and archaeological samples studied in the present work. On the one hand, most of the variance is explained by different dimensional properties of experimental versus archaeological assemblages (PCA), which show that they represent different reduction sequences (complete sequence in the experiments, incomplete sequence in the archaeological collection). Some structural differences were detected in the second PCA component, showing different dorsal scar patterns.

When one focuses on the discriminant variables in the sample by combining variable type and within-variable values (CT), about 20% of the overall sample variance could successfully be used to discriminate the three factor groups (two experimental sets and the archaeological set) if using bulb and initiation, followed by Toth's flake type and the dimensional variables.

A reflection of this discriminant result was supported by the MDA, which yielded a two-component solution based on bulb and initiation types as well as Toth's flake types as the main discriminatory variables. Platform thickness and number of previous flaking ("lda" function) or exterior platform angle ("discrimin" function) were also relevant. It could be argued that the main differences among the three sets that seemed unrelated to reduction sequence pertained to bulb and initiation, followed by platform thickness and numbers of previous flaking scars. Type Section hominins were producing relatively thinner striking platforms, most frequently with bulb types 2 and 3 (diffuse and absent) and initiation types 1 and 2 (wedging and hertzian).

Notably, these differences do not clearly separate the experimental samples from one another, nor from the archaeological sample. Figure 4 shows that a large portion of the 95% confidence interval of the Peninj archaeological sample overlaps with both experimental samples. This indicates that none of the arguments statistically analyzed here can be taken as indicative

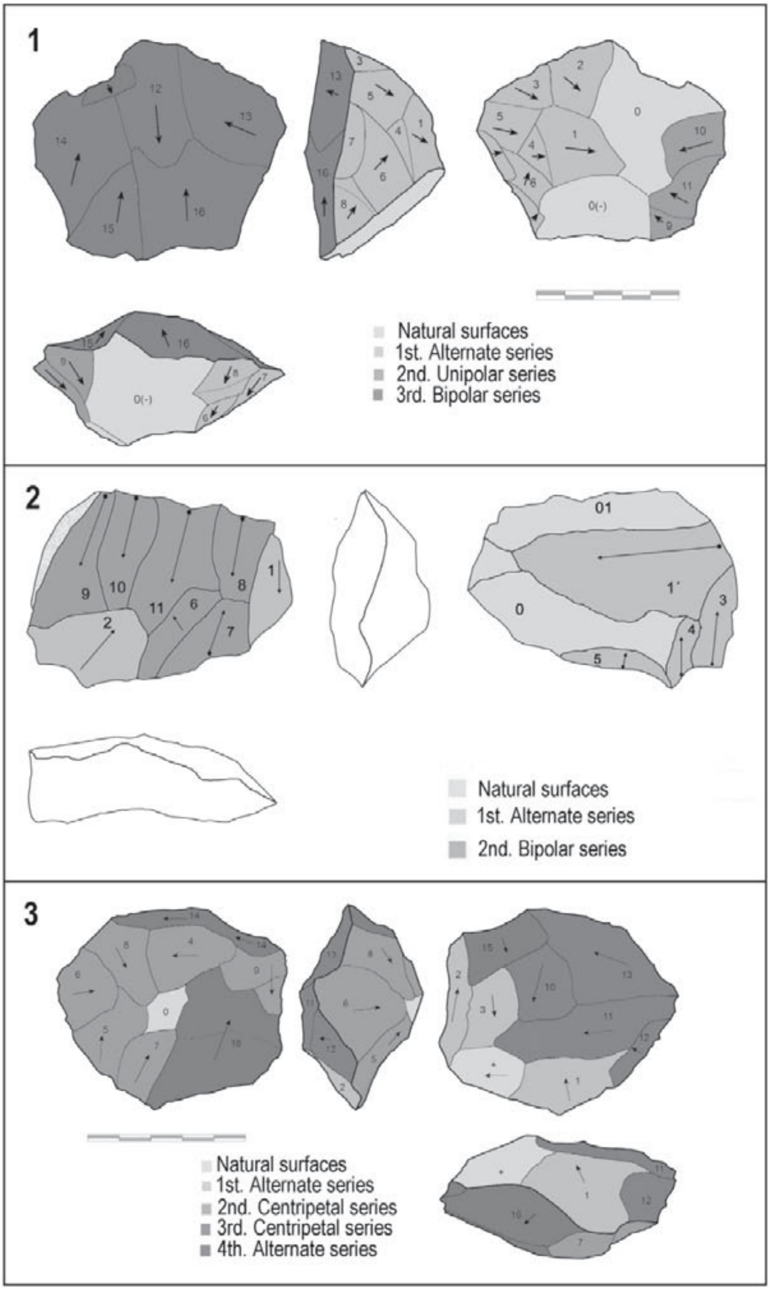

FIGURE 9.13. Diacritical schemes of experimental cores. 1. Final form of a core exploited following a search of maximal convexity model. Although never intended by the knapper, the final core shows a hierarchical centripetal-like morphology. 2. Final form of a core exploited following a search of maximal convexity model. The support is a flake and the reduction has followed a bipolar sequence of knapping series. 3. Final form of a core exploited following a bifacial hierarchical centripetal model, produced on a flake support.

of predetermination, because they can be reproduced with flaking methods (e.g., CC) that are not aimed at producing predetermined flakes. In this regard, it should be emphasized that the Peninj sample clusters more closely with the CC experimental model than with the HD model and shows a more similar distribution to that of the former (Figure 9.4).

De la Torre et al. (2003: 210) emphasized that the centripetal flaking on the dorsal surfaces of flakes could be taken as indicative of predetermination, based on the idea that radial flaking in a representative percentage of the flake collection was conceptually similar to Middle Paleolithic exploitation strategies. The present experimental study has shown that this is debatable. The variable showing the scar organization patterns has not been contemplated in a meaningful way by any of the CT or MDA results. As a matter of fact, similar centripetal flaking is widely documented in the CC model. The proportion of centripetally flaked pieces in the latter is similar to that obtained in the HD experimental model. Furthermore, our analysis of the archaeological sample does not support a relevant presence of radial patterns on dorsal flaking. On the contrary, it shows unidirectional and orthogonal organizations of the previous flaking, in agreement with the exploitation model described in our diacritical schemes.

The present study shows that there currently is no experimental model reproducing flake predetermination with the raw materials used by early Pleistocene hominins (volcanic, very hard rocks), clearly showing discriminatory differences with other experimental models not intending such predetermination. Without this experimental support, there is no epistemic basis to interpret determination of detached products from Early Pleistocene archaeological assemblages. This study has also shown that several of the analytical criteria applied to the study of flakes cannot be used to differentiate efficiently between predetermination and nonpredetermination, given the intense overlap between both alternative experimental scenarios.

Recognizing predetermination in blanks remains a complex and difficult issue in lithic studies (Brantingham and Khun, 2000, Dibble and McPherron 2006). At present, most of the criteria used to identify predetermined blanks (i.e., shape, dorsal pattern, striking platform, striking angles) seem to be vague tools (Boëda 1991: 42–50, 1994: 6, Hovers 1998: 63–64, Peresani 2001, Perpère 1989, Van Peer 1991, 1992: 1–8). This is the case because it has been demonstrated that different reduction models can produce apparently diagnostic predetermined blanks (Bar-Yosef and Van Peer 2009, Dibble 1989, Marks and Volkman 1983) and, as pointed out by Bar-Yosef and Van Peer (2009: 107), a significant amount of morphological equifinality is expected in the range of morphologies produced by different reduction strategies. Dibble (1989: 424–425), for instance, concluded that there was no statistical support for the validity of diagnostic traits purportedly related to predetermined flakes detached from complex reduction systems, when compared with regular flakes and bifacially retouched flakes. Although this is a much older archaeological context and a different problem, this study clearly supports Dibble's statement, in that we have not found consistent traits identifying the products of our centripetal hierarchical experimental cores and the results of complex reduction methods fall within the same variability range as that of other, nonhierarchized, less complex reduction strategies. The outcome of this study makes the relevant point that measuring predetermination is by far much more than measuring morphological or technological traits, because predetermined blanks are the result of a chain of processes driven by consciousness and structured mental templates. Those complex "conceptual operative" processes (Karlin and Julien 1994) have not been confirmed by the core sample studied in our diacritical analyses.

The status of the Type Section industry in its regional framework

Most of the lithic assemblages studied here share a quite homogeneous stratigraphic position, on a paleosol directly located on the surface of Tuff 1, in the Upper Sand with Clay member (USC) of the Humbu Formation (Luque et al. 2009a, b), and were deposited in a relatively short period and within the same environmental context: an alluvial area in a deltaic environment at the intersection of river channels (Domínguez-Rodrigo et al. 2009a: 105). This environment was repeatedly visited by hominins to process carcasses obtained in the vicinity of the alluvial area. The absence of high-density lithic patches, the predominance of scatters over the landscape, and the composition of the lithic aggregates suggest sporadic hominin incursion in this area (Domínguez-Rodrigo et al. 2005). The lithic collections retrieved from this paleosurface, in agreement with a scenario of low anthropogenic impact and high raw material flow, is characterized by the production of small to medium-sized flakes retaining no cortex on their dorsal surfaces, with a very low percentage of retouched tools and few cores, hammerstones, and unmodified cobbles. Thus, it is understandable that owing to their composition, the Type Section assemblages were first defined as belonging to the Oldowan technocomplex (de la Torre et al. 2003), in a moment in which it overlapped chronologically with the first Acheulean in other parts of East Africa (Semaw et al. 2009). Also understandable is the impact that the identification of traits of operational and technical complexity in this industry (de la Torre et al. 2003) has made in the academic community (Braun et al. 2008, Davidson and McGrew 2005, Delagnes and Roche 2005; Harmand 2007, Semaw et al. 2009, Stout et al. 2010). Certainly it was the first time that the concepts of core hierarchization and flake predetermination (thought to be expressions of much younger technological achievements) were related to the Oldowan technocomplex.

A recent regional reinterpretation of the Lake Natron archaeological evidence, however, has claimed that this industry fits much better within the Acheulean technocomplex and not with the Oldowan, as previously stated (de la Torre 2009). From this new perspective, the core and flake component of the Type Section industry would represent a functional-economic-technical adaptation to an alluvial environment of the same humans that produced the Acheulean sites located in the Escarpments, placed in a more distal position of the lake floodplain (Domínguez-Rodrigo et al. 2009b, 2009c). There are certainly reasons to support this new cultural attribution: (1) Some of the flakes recovered in the Type Section complex assemblages have been identified as handaxe or LCT resharpening/configuration flakes. Handaxe flow (input and output) in the Type Section complex industry would be implicit through the presence of these objects in some assemblages. (2) Large flakes (about 10 cm long) have been retrieved in sites such as ST2. Furthermore, our analysis suggests that the knappers of the Type Section were aware of the advantages of knapping large hemispherical flakes as core supports, because they provided a good natural interaction between two surfaces. Although large flakes found in the Acheulean sites of the Escarpment are larger (Domínguez-Rodrigo et al. 2009c), the production of this type of blank, either for large tool configuration or (as we suggest in the Type Section case) for exploitation, has been considered to be a representative technological trait of the Early Acheulean (Isaac 1984, 1986). (3) The Acheulean of Peninj, profusely documented in the North and South Escarpments, is stratigraphically related to a slightly younger depositional event in the USC member (post Tuff 4) of the Humbu formation (Domínguez-Rodrigo et al. 2009b,c). Clearly defined Acheulean sites (with large retouched flakes, handaxes, picks, and cleavers) located in the same stratigraphic position have also been found in archaeological aggregates of the Type Section (e.g., at ST 23, 28, 46, 48, 75, 76), as well as in other slightly younger positions, as at ST 69 (Díez-Martín et al. 2009a). Furthermore, the oldest Acheulean site documented in the Peninj region (PEES1) has been found in the south escarpment right on the Main Tuff, and thus older than the sites studied here (Domínguez-Rodrigo et al. 2009b). It seems apparent then that the ST site complex is bracketed between clearly defined Acheulean sites. This evidence speaks to the fact that, beyond typological variability, the different archaeological areas of the Lake Natron region should be considered as subsystems of a regional Acheulean system interconnected with and driven by different environmental, locational, economic, and functional interests and/or constraints. Our interpretation of the core assemblage retrieved in the Type Section favors a scenario in which hominins were maximizing raw material and intensively exploiting some specimens (specifically those showing a final morphology similar to the discoid method *sensu lato*). This fact and the high percentage of type VI flakes support the idea that some good quality basalts and nephelinites were quarried at a certain distance (e.g., the midsection of the Peninj River area) and discarded in the delta of the Peninj river (the Type Section) in an advanced stage of reduction. This reinforces the idea that rock supplies were intensively flowing along an interdependent and interconnected landscape. The Type Section site complex would then represent full production stages of different operational sequences.

Thus, formal variability observed in the Natron area (e.g., core and flake assemblages versus assemblages in which large flake configuration is observed) would be related to synchronic (functional, locational, environmental) and not diachronic (Oldowan/Acheulean) variability (Isaac 1977: 98).

De la Torre (2009) has suggested that the key trait that supports a link between the various industries retrieved from the Lake Natron area is precisely represented by the centripetal hierarchical reduction method, found both in the Type Section site complex (where handaxes are formally lacking) and the Acheulean sites in the escarpments (where abundant handaxes have been found). Following this perspective, de la Torre (2009: 103) suggests that "the ability to exploit the entire volume of a piece through a structured bifacial method . . . which is what defines the ST Site Complex cores – shares the same technical scheme usually attributed to the Acheulean." A number of authors have already remarked on the technological and conceptual affinities between Acheulean handaxe production and complex hierarchical reduction strategies (Rolland 1995, Schick 1998; DeBono and Goren-Inbar 2001, Lycett et al. 2010). Thus, the bifacial centripetal hierarchical model would be the expression of the same technical model, and then the humans that inhabited the region and produced assemblages both with and without handaxes shared the same technological concept and knapping structure (i.e., the technical skills and methods shared by a human community, as defined by Boëda [1991] and Pelegrin [1985]).

The present study casts doubt on the role played by the reduction methods for the definition of such a shared knapping structure among the human populations that inhabited the Natron basin during the Lower Pleistocene. Our study does not support the existence of the so-called bifacial centripetal hierarchical reduction method among the Type Section knappers; thus, it cannot be the link between this industry and the escarpment industries. If the same species were responsible for the production of the varied technical behaviors displayed in the Natron area, the links connecting such a variety of technical solutions must be found elsewhere. At present, a new round of investigation in the region is being conducted. Fieldwork is in progress both in the north and south escarpments and in the Type Section area, and it is aimed precisely at better understanding the regional integration of the archaeological evidence found in Lake Natron (Díez-Martín 2008, et al. 2009b). This study should be understood in the framework of the crucial behavioral transformations that emerged in East Africa with the origin of *Homo ergaster* and the emergence of the Acheulean technology.

Conclusion

Our revision of the lithic assemblages retrieved from the Type Section site complex of Peninj, bracketed between 1.6 Ma and 1.2 Ma, has been supported by experimental replication and a robust statistical comparison between the archaeological and experimental flakes. This analysis has led to the following conclusions.

Our study of the core collection does not support the existence of complex core hierarchization in the Type Section assemblages. The core collection shows a remarkable morphological diversity related to a high variability of support types. Hominins were systematically flaking cores in series of two to four flakes detached from suitable striking platforms (cortical or created by a previous series of detachments). This pattern seems to be a standardized behavior in the Type Section area, although it is not applied to the whole perimeter of the cores, it is not performed throughout long series, and it produces highly diversified final morphologies (discoid in hemispherical or flake supports, and more polyhedral in massive supports). Cores showing a discoid final morphology can be considered discoid in only a broad sense, since an alternative exploitation of both surfaces has been ruled out.

The archaeological flake sample is in agreement with our observations regarding the reduction strategies exhibited in the Type Section. Although humans were systematically producing small and medium-sized flakes, they show little evidence of radial core rotation in their dorsal patterns.

On the contrary, dorsal surfaces preferentially show unidirectional and orthogonal patterns, in agreement with the reduction models reconstructed through our diacritical schemes. Our statistical comparison between the archaeological flake collection and the flakes experimentally obtained through two alternative knapping methods (in which core hierarchization was and was not implicit) has failed to provide significant signs of flake predetermination.

Neither our diacritical analysis nor our experimental replication support the validity or feasibility of the hypothetical model created to explain the reduction sequence of centripetal hierarchical cores (de la Torre and Mora 2009). This theoretical model appears to be too simplistic, because it does not take into account aspects relevant to the knapping process, such as the morphology of the supports and their influence on the final core forms.

Finally, it is worth noting the most remarkable technological advances implicit in the lithic collections knapped by the groups that inhabited the Type Section area of Peninj in Lower Pleistocene times:

1. Humans were systematically taking advantage of the hemispheric supports in their knapping strategies, maximizing exploitation of two alternate surfaces.
2. They were efficiently controlling their knapping actions and producing large flake supports for exploitation.
3. This pattern produced a relatively high productivity, as recurrent series allowed a considerable detachment of blanks.
4. Humans were successfully and recurrently creating suitable striking platforms in their subsequent knapping series.
5. Humans were clearly carrying out different knapping strategies depending on the type of raw material (support quality and format). This behavior is close to the concept of raw material economy (Perlès 1991).

In sum, humans were here managing to exploit a very hard volcanic raw material successfully. They were applying adequate knapping control and creating efficient striking surfaces to obtain subsequent series. This evidence implies a successful human adaptation and flexibility to the local conditions and constraints imposed by raw material in the Lake Natron area. The particular volume construction produced by the Lower Pleistocene knappers of the Type Section area in Lake Natron (discoid-like) is related to two remarkable aspects (Slimak 2008): technical knowledge (allowing an intense exploitation) and core rotation or systematic exploitation (allowing the exploitation of the whole perimeter of the piece). At the

Type Section area, however, this technical knowledge (or savoir-faire), that must be acknowledged here as a valuable proof of technical adaptation and complexity is not due to the use of a specific reduction method that was inflexibly and mechanically repeated.

Acknowledgments

We wish to thank COSTECH and the Antiquities Unit (Tanzania) for permits to conduct research at Peninj. Funding for this research has been provided by the Spanish Ministry of Culture. We are extremely thankful to D. Chessel, A. B Dufour, and S. Dray for their design of the "ade4" package and to W. N. Venables and B. D. Ripley for creating the MASS package in R. We are also thankful to the members of the R Project Foundation for Statistical Computing for keeping this software available (http://www.r-project.org).

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CHAPTER 10

The Early Acheulean in Africa: Past paradigms, current ideas, and future directions

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How many bifaces are needed to characterize a site as Acheulean?

The study of the Palaeolithic has been conceptually shaped by a lineal evolutionary paradigm, framed by ideas of cultural progression (Adams 1998: 9–73) and "behavioral modernity" (Shea 2011). The analysis and interpretation of stone artifacts reflects this paradigm, which has been influenced at least in part by the dichotomy of "simple" versus "complex" (Torrence 1989). Because these concepts continue to shape the conceptual tools used by contemporary archaeologists, it is unfortunate that critical reviews that examine the way in which old paradigms impact contemporary research are uncommon in the Old World (but see Bisson 2000; Strauss, 2009: 5).

Inspired by the methodological procedures of nascent nineteenth century geological sciences, French Prehistorian Gabriel de Mortillet (1869) introduced an archaeological classificatory procedure that still impacts lithic studies today (O'Connor 2007: 115–125; Sackett 1981; Vega 2001). Aware of the descriptive and synthetic power of artifacts (Mortillet 1883: 18–19), Mortillet proposed the use of specific lithic objects found within the assemblages recovered from successive periods as *fossiles directeurs*: type-fossils for unilinear cultural evolutionary interpretations of past societies (Mortillet 1869). It is within Mortillet's classificatory scheme (1872), where the term *Acheulean* was coined (based on the lithic materials recovered from the site of Saint Acheul, in the Somme valley of northern France). The hand ax (or more generically the biface,[1] if we include type variations

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[1] In this work, when following other authors' nomenclature, we will respect the term *hand ax*. When in our own discourse, we refer to the generic concept of biface (hand ax, cleaver,

such as cleavers, picks, and knives) was recurrently identified as the requisite emblem of the Acheulean technocomplex (Clark 1994; Lycett and Gowlett 2008; Noll and Petraglia 2003; Schick and Toth 1993, 2001; Sharon 2007, 2010; Wynn 1995). As practiced in European archaeology until World War II, most researchers used the *fossil directeur* concept in their analytical approach to the study of Palaeolithic collections (Vega 2001). That paradigm also influenced the tripartite division of the Stone Age in Africa (Goodwin and Van Riet Lowe 1929). French archaeologist Henri Breuil (1936), for instance, extensively used type-fossil markers within the cultural historical perspective, aiming to interpret empirical data (artifacts in this case) as if they were discrete and historical human-made entities (see also Childe 1929: v–vi; Collins 1969: 277; Shanks and Tilley 1987: 81). The culture history approach envisioned archaeological sequences as complex webs of cultural units, traditions (*tradition-cultures*, as defined by Collins 1969), groups, facies, and subfacies that were related to each other as both synchronic and diachronic phyla (Diez-Martín 2003: 36). As a result of this, examination of interacting "genetic" schemes between regional archaeological phases and cultures was a consistent practice in the European Palaeolithic (Otte 1996: fig. 116, Tieu 1991: 93–102). For the African Stone Age, immersed in the same conceptual *koine*, an example exemplifying Breuil's culture history perspective can be seen in the discourse shown by Kleindienst's (1967) work, which is devoted to the discussion and definition of appropriate terminological concepts to be used in the East African ESA record. Kleindienst commonly applies terms such as cultural units to the archaeological record (namely to lithic implements).

After World War II, François Bordes' influential work on flaked stone assemblages overcame, at least partially (Sackett 1991: 128), the *fossil directeur* approach and the cultural history paradigm. This was done by including more empirical and diversified observations of both technological and typological aspects of lithic assemblages (Bordes 1947, 1950, 1961). The simple identification of specific types for defining sequences was replaced by a variety of typological indexes designed to identify and organize assemblages within "cultural" sequences. Although he used "technological" observations in his assessment of lithic assemblages, Bordes was more interested in relying on parameters now generally considered to be typological (morphology and metrics) in nature (Vega 2001: 204). Of note, in his sequential classification of the Acheulean, Bordes relied on different

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and trihedral pick), we will adopt the term LCT or large cutting tools, of common use in current days (i.e., Sharon 2007).

morphologies and types of hand axes, based on metric measurements. This fact demonstrates that hand axes themselves still played a central role in Bordes' studies of the Acheulean, suggesting that these objects still retained much of their status as type-fossils.

The influence of Bordes' theories and methods have been pivotal in the formation of lithic analysts' perspectives in general and have played an active role in shaping the methodological approaches to the African Early Stone Age (ESA) in particular. Many works, some of them fundamental to our present views of ESA, have used analytical procedures that reproduce the Bordesian paradigm in their studies of African lithic collections, showing preference for typological rather than technological assessment (Archer and Braun 2010; Chavaillon et al 1979; Clark and Kleindienst 1974, 2001; Crompton and Gowlett 1993; Gowlett and Crompton 1994; Isaac 1977; Leakey 1971, 1994; Machin et al. 2007; Roe 1994, 2001; Tixier 1956).

Both the cultural history paradigm and the subsequent typological tradition of lithic analysis, as updated versions of the type-fossil paradigm, exemplify the latter as one of the most persistent methodological tools in the history of archaeology. It has influence well beyond a particular school of thought or linguistic background. The African Acheulean, where a convergence of researchers from different backgrounds can be observed, demonstrates this very point, as the same form once identified as the type-fossil marker of the Acheulean by Mortillet still occupies the predominant role in research programs devoted to technotypological descriptions of specific assemblages as well as to cognitive, symbolic, social, or functional adaptations of the Acheulean (Clark 1994; Davidson and Noble 1993; Hodgson 2009; Edwards 2001; Gowlett and Crompton 1994; Kohn and Mithen 1999; Lycett 2008; Lycett and von Cramon-Taubadel 2008; Lycett and Gowlett 2008; McPherron 2000; McNabb 2004; Pelegrin 1993; Stout et al. 2008; White 1998; Wynn, 1979, 1995, 2000; Wynn and McGrew 1989).

Some of the most controversial debates related to the origin of the Acheulean technocomplex are rooted in the prominence given to the hand ax in the scientific discourse. A quintessential example is constituted by the Developed Oldowan debate. The technological sequence established in Olduvai Gorge by Mary Leakey (1971: 4–8) was based on both typology and frequency of particular types of artifacts (e.g., the hand ax). The Developed Oldowan B was defined on the basis of artifact form and dimension (the typometric procedure intimately tied to straight typology) and, more specifically, the low hand ax frequencies. Conversely, following Leakey's procedures, normative Acheulean sites were *arbitrarily* defined as those sites in which the *fossil directeur* of the Acheulean represented ≥50% of the specimens included in the lithic collections (Leakey 1976a: 447). It is widely known that Leakey's hand ax frequency threshold followed from Kleindienst's (1961: 40) assertion that only assemblages containing ≥40% of hand axes could be classified as formal Acheulean. Although there was no logical rationale for either of these percentages, it of course also created the awkward situation of a site exhibiting a type-fossil but not subsequently categorized as the brand that that type-fossil epitomizes. This is similar to a person's not being considered a football fan because they are wearing only their team's jersey and not the entire uniform.

Later, Leakey (1976b: 31) argued that other features were more relevant than counts or percentages for categorizing assemblages, finally considering shaping technique to be a relevant trait for examination. She pointed out that in those samples classified as normative Acheulean, hand axes tended to be larger and more symmetric, relying on a typometric and stylistic approach to data interpretation (1976a: 447–448).

The Developed Oldowan concept, a purported transitional phenomenon between the Oldowan/Acheulean interface at Olduvai Gorge, can be seen as an extension of the intermediate concept created to link periods in the African Stone Age (Clark 1957). The cultural sequence created by Leakey at Olduvai on the basis of typological grounds has had a tremendous impact in both archaeological field research and theoretical discussions. Regarding the former, many researchers have applied Leakey's nomenclature to assemblages from African sites other than Olduvai (Chavaillon et al. 1979; Chavaillon and Piperno 2004; Clark and Kurasina 1979; D'Andrea et al. 2002). Regarding the latter, subsequent interpretations on the meaning of the Developed Oldowan and the convenience of its use have been the focus of an intense debate that continues to the present day.

Taking into consideration that the particularities of the Developed Oldowan A, described by Leakey, might be explained through the constraints imposed by raw material type and use (Diez-Martín 2005; Kimura 1999; Semaw et al. 2009); our discussion here applies mostly to the *Developed Oldowan B&C – Acheulean* dichotomy. The formal difference between these two concepts did not consider technological continuity or change but entirely focused on on percentages and morphology of bifacial implements. In the course of revisions and reinterpretations of the Developed Oldowan, some authors have devoted substantial effort trying to add fresh interpretations to Leakey's original framework. These reinterpretations have tried to explain the differences between the Developed Oldowan and Acheulean in terms of functionality (Gowlett 1986), differential use of raw materials (Stiles 1979, 1981, 1991), different reduction stages (Jones 1994), or different mobility patterns (Ibid.). Most of these views favor a framework in which the concepts of Developed Oldowan and Acheulean can be conceived as complementary and interrelated parts of the same technological processes that characterized the dawn of the Acheulean at about 1.7 Ma in East Africa. Mary Leakey also entertained the idea, however, that both complexes could be the same entity and that morphological or type-frequency differences could be explained on the basis of synchronic variability driven by functional and/or ecological factors (Leakey 1976a: 450; for a similar discussion centered in the North American Late Pleistocene, see Eren et al. 2011).

More recently, other authors have explicitly pointed out internal contradictions produced by the Developed Oldowan concept (de la Torre and Mora, 2005). From a critical technological reappraisal of the classic collections retrieved from Olduvai Gorge, de la Torre and Mora (Ibid.) suggest that at EF-HR (the only site that was formally classified by Leakey as Acheulean in Bed II on the basis of the artifact frequency), most of the artifacts initially classified as a hand ax actually correspond to large flakes with unifacial and marginal secondary retouch. These items might in fact be similar to the massive scrapers recently defined by Goren-Inbar et al. (2008), rather than to true bifacially flaked hand axes (de la Torre and Mora 2005: 109). Conversely, several hand axes from BK (formally ascribed to the Developed Oldowan B) are some of the best volumetric and technological examples of Acheulean hand axes (Ibid.: 181), in regards to plan-form and cross-section symmetry (Hodgson 2009; Toth and Schick 1993; Wynn 1989, 2002). De la Torre and Mora therefore stress technological continuity and similarity between the Developed Oldowan and Acheulean during the upper part of Bed II. Their observations concurrently question the validity of the Developed Oldowan concept (Ibid.: 228). Other authors have made analogous inferences (Semaw et al., 2009).

New interpretations of the Developed Oldowan were formulated as alternative responses to the influential scheme proposed by Mary Leakey. All of them perceive lithic implements not only as static objects but also as once-dynamic components within a larger eco-functional and possibly social context. As such, flaked stone implements can potentially reveal different sorts of information about hominin behaviors (Gowlett and Crompton 1994), such as site function, tool production techniques, artifact function and use, raw material quality and availability, and landscape use and regional interconnectivity. To various degrees, all of these aspects are necessary for the holistic evaluation of behavioral variability within a technocomplex. From this perspective, percentages of specific tool classes in any given site lose their categorical meaning and can be understood as by-products of behavioral and/or taphonomic processes.

Although Glynn Isaac retained aspects of the culture-history paradigm in his intellectual rationale (Isaac 1989: 245), his contextual perspective was radically influential in our present understanding of technological processes within their ecological and other (e.g., social) frameworks (Isaac 1984, 1986; Toth 1985). Technological systematics show a similar importance for the present understanding of lithic studies in human evolution. This perspective includes the variety of research avenues related to the concept of reduction sequences or *chaînes opératoires* (for more extensive discussions on these various conceptual frameworks, see for instance Andrefsky 2008; Eren et al. 2005; Eren and Prendergast 2008; Nelson 1991; Shott 2003, de la Torre and Mora 2009 and references therein). Influenced by the plethora of new approaches that have enlarged our understanding of lithic artifacts and their use-lives (Goren-Inbar and Sharon 2006; Soressi and Dibble 2003) it can be expected that, as predicted by Gowlett (1996: 135), "static, classificatory, typology is out. A dynamic approach to technology as social practice is in contrast much favoured" (but see Bar-Yosef and Van Peer 2009). The contemporary understanding of the Early Acheulean as intellectual concept has certainly profited from the addition of typologies of process to typologies of product (Eren 2006).

The Mode 2 and the Acheulean

In accordance with recent methodological contributions, we need a conceptual and terminological overhaul capable of embracing all the facets now included in the domain of Acheulean research: typological, technological, cognitive, behavioral, environmental, spatial, and functional. It seems that there has been a revival in recent years of the terminological framework proposed by Grahame Clark (1977), and owing to its specific commitment to a more global scale of research (Gamble 2001; Villa 2001), it may be perceived as a candidate for fulfilling this role (Diez-Martín 2003). Unfortunately, Clark's technological modes model (TMM), because of its vague and generic nature, has proved to be in fact a close corollary of the type-fossil approach.

Grahame Clark presented his ideas on the evolution of lithic industries in his *Word Prehistory*, first published in 1961. The TMM model was first included in the 1969 and enlarged in further editions (we refer here to the 1977 edition: 23–38). Clark acknowledged the importance of lithic knapping for the study of human prehistory, as he recognized that the variety and diversity of techniques accomplished by hominins could potentially be a robust informative tool for researchers. Clark acknowledged as well that human technological behavior is driven by evolutionary premises: because lithic artifacts facilitate adaptation to different contexts, the technological process is pushed to the production of more complex and efficient objects, whereas the less effective objects/techniques would be dropped out for the particular context in question (Clark 1977: 23). The idea of adaptation as the motor of technology was used within the context of the cultural-ecological approach of the mid-twentieth century, in which Clark's ideas were rooted (Adams 1998: 63–64). Certainly, Clark's perspectives on the development of technology are framed within the ecological-functionalist approach and the British paleoeconomy approach (Maschner and Mithen 1996: 5). In sum, the successive stages of development from Mode 1 to Mode 5 were argued to evidence a diachronic and apparently unilinear sequence. There are several other aspects in Clark's proposal that are relevant to an accurate understanding of his ideas, however: (1) the process is a continuum and does not show sharp boundaries between different Modes; (2) the technological sequence does not necessarily imply a chronological sequence: two human groups placed on the same temporal plane might be pushed to keep or reject any technical element (seen as elementary or progressive within their schemes) as a response of environmental pressure and constraints (Clark 1977: 24); (3) although homotaxial, the model is not universal as it could be influenced by regional (i.e., environmental) constraints (e.g., technological stasis would be favored in regions where ecological and demographic conditions require no new adaptive challenges).

Without neglecting the influence of the culture history paradigm in Clark's intellectual background (Gamble 2001), one of the most interesting and useful aspects of the TMM is related to the relevant role played by the ecological framework throughout it. In some aspects, this idea is a precursor not only of processualism but to positions currently held by most archaeologists interested in lithic studies: (1) the rejection of an aprioristic connection between any one mode and a specific chronological frame can find a correlation in later debates on the role played by stylistic aspects (Gamble 1995) and the oppositions simple/complex or efficient/inefficient (Torrence 1989) as variables with chronological meaning; (2) the link between environmental/ecological conditions and the mechanism of lithic variability (Gowlett and Crompton 1994; Isaac 1984, 1986); and (3) the role played by technology as a tool of ecological adaptation, seen in the commonsensical assertion that lithic objects are simply resources used to facilitate human interaction with the environment (Carbonell et al. 1983).

Clark's TMM has been revitalized in recent years by several authors in various regional contexts, including the African ESA. Among its proponents are Foley and Lahr (1997, 2001, 2003) who, departing from previous ideas (Foley 1987), argue that Stone Age/Paleolithic technology can be approached from a phylogenetic perspective. To a certain extent, this conception favors the separation of technological analyses from the contradictions of the culture history perspective (as pointed out by Carbonell et al. 1996: 89) and lines them up with the domain of evolutionary biology, in line with Clark's interpretation of technological traits as subject to the same selective process as biological organisms (Clark 1977: 23). Foley and Lahr (2001, 2003) think that there is a direct association between specific hominin taxa and certain technological behaviors. From this point of view, the TMM could constitute a useful tool for addressing various research topics. Foley and Lahr (1997: fig. 3), for example, used the model in their analysis of early hominin dispersals by interpreting technological improvements as mere biological apomorphies. Leaving aside the critiques shown to this phyletic (and rigid) conception of technological behaviors (Clark 1989) and the numerous archaeological and ethnographic examples contradicting the *one species, one technology* axiom (Bar-Yosef and Belfer-Cohen 2001; Bar-Yosef and Kuhn 1999; Cosgrove 1999; d'Errico et al 1998; Gamble 2001; Villa 2001), the cladistic discourse in technology has nonetheless influenced research devoted to both the African and European records. It should be briefly noted that Foley and Lahr's (1997) informal *cladistic* approach differs from other recent cladistic approaches, which trace the evolution of technologies and artifact forms through formal phylogenetic analyses without linking them to particular species (e.g., Lycett 2009).

The revival of Clark's TMM has been possible because it possesses several research advantages. For example, (1) the conceptual framework is more objective and scientific, because the TMM defines archaeological entities empirically observed, leaving aside classic nomenclature and the contradictions seen within the culture history approach (Schick and Toth 2001). This quality has been already exemplified by the cladistic approach, in which basic technical traits are understood and processed in the same way as biological traits. (2) The model constitutes a useful tool for interregional and global analyses (Lycett 2009: 82, Schick 1994: 575) and can minimize constraints imposed by the comparison of type-sites used to define classic technocomplexes. The model allows synthetic grouping of sites located in different geographic areas but sharing common technological traits. (3) The abandonment of classic nomenclature makes it possible to overcome the contradictions produced by a Eurocentric research framework with current globalization of the scientific knowledge (McBrearty and Brooks 2000). Present data suggest that multiple hominin radiations follow the initial "Out of Africa I" and that the hand ax innovation was first produced in sub-Saharan Africa and then spread from there to other areas of the Old World (Lycett and von Cramon-Taubadel 2008; Moncel 2010). Using the Mode 2 label makes it possible to avoid typifying a technocomplex originating in African with data retrieved in France.

Despite advantages, however, no procedure is perfect, and many authors have pondered the convenience of using the TMM and have highlighted its limitations (Bar-Yosef 2006; Bar-Yosef and Goren-Inbar 2001; Gamble 2001; Gowlett 1998, 2009; Villa 2001). Certainly, the model shows several restrictions and contradictions that need to be addressed. According to Clark, the representative traits of technological innovation are (Clark 1977: Table 5): cores and flakes (Mode 1), the hand ax (Mode 2), prepared-core techniques (Mode 3), prismatic blade techniques (Mode 4) and microliths (Mode 5). This scheme hardly differs from the fossil *directeur* approach of the culture history paradigm. Clark's modes also possess other characteristics as diagnostic traits, some of them related to operational or knapping principles (Boëda et al. 1990) of *débitage* (a variety of exploitation processes and techniques related to the production of simple flakes, predetermined flakes, and blades) and *façonnage* (the standardized configuration of final forms, such as the hand ax). Although the TMM has been primarily perceived as "technological," it is apparent that the technological definition of Mode 2 is vague. Other than hand ax manufacture and the production of large flake-blanks (≥10 cm, *sensu* Kleindienst 1962: 84; Isaac 1969: 16, 1977, 1984, 1986) little else is technologically discussed for identifying Mode 2. Although the TMM identifies large flake-blank production as integral to the concept of Mode 2, it lacks any explicit documentation of actual archaeologically identified knapping techniques aimed at the production of large flakes, even though the process has been experimentally modeled by many researchers (Jones 1994; Madsen and Goren-Inbar 2004; Toth 2001) and has been extensively addressed in recent years (Sharon 2007, 2009, 2010). Additionally, in both the oldest formal Acheulean sites reported to date (Asfaw et al. 1992, Roche et al. 2003: 670, Semaw et al. 2009: 186) as well as in much later Acheulean sites (i.e., Santonja and Villa 2006), a high number of LCTs are produced on sources other than large flake blanks (cobbles, tablets, and other nodular forms), suggesting that LCT large flake-blank reduction is far from the only knapping pathway that should potentially be used to technologically define Mode 2. The excessive simplicity with which the different modes are defined limits the technological applicability of the TMM.

For the specific case of Mode 2, this dependence on one or two object markers shows that Clark's model is similar to traditional type-fossil approaches, ironically the very perspectives that it intended to overcome (Carbonell et al. 1996: 89). Instead of relying on the understanding of whole sequences and contexts (in the framework of the contextual and technological approaches that followed the typological paradigm), a single object is used as the exclusive means to scientific inference. This object, the hand ax, is generally assessed only from its final production and operational stages. A final or shaped object is equivalent to the type in the traditional typologies, and *typology*, as a system of concept formation (Adams and Adams 1991), can be defined as the study and definition of descriptive categories, forms, or types (Otte 1996: 238). In lithic studies, typological lists usually include categories that are located at the end of the operational sequence (Bordes 1961) and show less interest in other aspects of the sequence.

In some ways, then, the TMM is actually a more limited and to a certain extent a less efficient approach than the type-fossil approach. If we assume that the type-fossil approach and the TMM share a common foundation based on type-forms and type-techniques, it should be acknowledged that the complex web of facies and regional groups that characterized the cultural-historical perspective, driven by descriptive and particularistic goals (Vega 2003), was at least more suited to the variability and diversity of real technological behaviors. The TMM, precisely because of its simplification, disregards all exceptions to its normative model and is thus narrower and more inflexible than its predecessors.

This inflexibility can be seen in the way in which the model is used in current practice. Recently, Clark and Schick (2000), when defining Mode 2 lithic assemblages recovered from the Middle Awash (Ethiopia), state that, "As at other Acheulean sites, those containing large number of Mode 2 handaxes and/or cleavers also contain quantities of the ubiquitous Mode 1 cores and flakes" (Ibid.: 197). They go on to state: "... both predominantly Mode 1 and Mode 2 assemblages co-occur throughout the Bouri Formation, so that ecological change is unlikely to provide the whole answer to explain why some assemblages continue to be of Mode 1 style." Certainly, by acknowledging that the same archaeological assemblage contains Mode 1 and Mode 2 items, these authors are using the modes model as a significantly rigid framework in which artifacts, even coexisting in the same archaeological context, can be identified and described in distinctive and independent packages (i.e., Mode 1 or Mode 2). Would it not be more plausible, however, to interpret artifacts recovered from the same contextual provenience as complementary fractions of the same technocomplex rather than as independent entities? Is it not possible that lithic artifacts, representing both core/flake and hand ax reduction sequences and discarded in the same location, could be produced by the same knappers? Is it not possible that a variety of operational processes or sequences (*sensu* Boëda 1991) could be knapped by the same "people" – those who shared identical mental capabilities, social contexts, and environments?

If a group of knappers, as part of their technological routines, were able to both undertake lithic reduction (cores exploitation and flake production) and tool configuration (hand ax shaping) strategies at the same time, then separating the result of a coherent and diversified technological behavior in different independent containers would underestimate the variability that actually existed. Interassemblage variability might be seen as a response to various needs and constrains co-occurring at the same time and space (raw material quality, availability, site functionality, different tasks carried out at the same or different times but in the same spot) rather than as independent segments of an evolutionary continuum. This limitation arises when a model that was intended to interpret the global picture of the technological continuum is used locally, at a site scale. In this particular case, the additive premise that constructs the TMM (Clark 1977: 24) masks the diversity and richness of technological behaviors at local scales.

One of the most obvious facts suggesting that the TMM has failed to constitute an alternative and original methodology for characterizing ES/Paleolithic lithic behaviors is that disparate terms (rooted in different intellectual backgrounds) have been made equivalent. Some authors have already noted that the concept of Mode 2 is not synonymous with the term *Acheulean* (Tryon and McBrearty 2002). Nonetheless, by identifying Mode 1 as Oldowan or Mode 2 as Acheulean (Camps and Chauhan 2009, Clark and Shick 2000, Foley and Lahr 2003, Moncel 2010, Schick 1998, Schick and Clark 2003), other authors are putting at the same level conceptual terms created in disparate historical situations, referring to different contexts and scientific backgrounds and formulated to serve different programmatic agendas (i.e., the cultural-historical and ecological-functionalist schools of thought, Trigger, 2006). It seems obvious that when such equivalence is made explicit in scientific discourse, researchers are declining to use the TMM as an alternative to the type-fossil paradigm, even though it was constructed to be just that. Conversely, they are reinforcing the traditional paradigm, and more important, they are doing it in an impoverished way, because they are reducing the classical taxonomic spectrum to a single type (e.g., Oldowan = Mode 1, Clactonian = Mode 1, but see Lycett 2007: 567–568 for a suggested solution to this problem by combining various taxonomic units in a hierarchical manner, recognizing the limitations and utility of various classificatory units at differing scales of taxonomic organization). It is difficult to understand then to what extent the TMM adds new or better taxonomic tools to the present understanding of hominin technological behaviours. Adding even more confusion to terminological issues, some authors have also suggested that both Mode 1 and Mode 2 at some sites might be lumped within the Acheulean technocomplex, since both might be indicative of Acheulean activity variation (Schick and Clark 2003: 26). Others have included the controversial Developed Oldowan concept within the Mode 1 without considering the intense debate in which it is involved (Foley and Lahr 2003: 119),

Despite its assets, we think that if the main goal of the TMM is to overcome the framework imposed by the cultural-historical type-fossil paradigm, it needs to be reformulated. Present use of the model and the variable equivalences made to concepts created within the cultural-historical paradigm (i.e. Mode 1 = Oldowan, Mode 1 = Developed Oldowan, Mode 2 = Acheulean, Acheulean = Mode 1 and Mode 2) evidence a relevant lack of terminological consistency. Until improvements are made to the TMM, we very much agree with Bar-Yosef (2006), who recommends abandoning it for current use.

Are hand axes imperative to identify the Acheulean?

As we have mentioned in previous sections, some of the problems and limitations related to what archaeologists classify as the Acheulean technocomplex are related to the overestimation of the hand ax in relation to other lithic components also associated with the Acheulean (Klein 2000: 22–23). Certainly, as pointed out by Lycett and Gowlett (2008: 296), "the Acheulean is recognized by the presence of bifaces rather than any other criterion." Taking into consideration how deeply the type-fossil approach is rooted in technological characterizations, the use of one type as the "measure of all things" (with or without the TMM) is fundamentally flawed. Years ago Gowlett (1986: 248) recognized this taxonomic shortcoming when remarking that "the Acheulean … is defined according to a single tool-type … and without reference to the limits of this class." Certainly, and quoting the same author, "There is a major difficulty inherent in any classification which actually depends on a single artefact category that can be locally absent for all kinds of reasons" (Gowlett 2009: 70). Gowlett (1986: 249) posed what, seen from the current state of research (to a great extent centered on the hand ax as synthesis of the Acheulean), a courageous and challenging question: Would it be possible for archaeologists to envision the existence of an Acheulean without formal hand axes? How could archaeologists identify or define any factual connection with the Acheulean technocomplex if this type-fossil is absent from any given assemblage? Considering issues already discussed, would it be appropriate to lump assemblages that lack LCTs but are penecontemporaneous or synchronous with other sites bearing this type of implements into the Mode 1 concept, or is it possible to define non-hand ax Acheulean technological markers that relate to innovation and volumetric complexity? Do these markers even exist?

Although LCTs were probably only a fraction of Acheulean toolkits, most authors have hardly paid attention to these questions in their exploration and conceptual definition of the Acheulean. Wenban-Smith (1998: 93) has pointed out that, as a technological complex characterized by a varied set of technological improvements, the Acheulean must be associated with other technological traits complementary to the LCT production. If the emergence of the Acheulean is understood as the domain of a specific conceptual universe related to a collection of technological changes (Hodgson 2009, Toth and Schick 1993, Wynn 1989, 2002), it follows that other representative technological signals of this system might also be recognized in artifacts other than hand axes.

Boëda (1991) might provide a way to identify other technological aspects of the Acheulean. In his lucid analysis on the variability of the Paleolithic technical systems, Boëda introduced an important distinction between two different types of technical operations: *débitage* or knapping (operational sequences aimed at exploiting cores) and *façonnage* or shaping (operational sequences aimed at shaping forms). Each of these operations includes a variety of methods aimed at two generic objectives: producing flake blanks or shaping flake/nodular supports (Ibid.: 40). Boëda suggests that the archaeological record can contain four possible combinations of these two operations, not linked to any specific chronological framework, regional context, or technocomplex: (1) an exclusive conception of *façonnage*; (2) an exclusive conception of *débitage*; (3) an interactive conception of *débitage* and *façonnage*; (4) two independent conceptions of *débitage* and *façonnage* (Ibid.: 42).

A type-fossil approach (and by extension the TMM approach) would assume that the Acheulean is identified, described, and understood within the frame of the first option cited here. This is to say, the whole range of technological systems of the Acheulean is to be found in the study of LCTs, and there are not other relevant technological concepts associated with the Acheulean technocomplex. From this point of view, all the non-LCT artifacts recovered from an Acheulean site would bear no information of particular relevance to our understanding of the Acheulean and could be dismissed (or included within the hermetic container labeled as Mode 1). Boëda suggests that such a reduced conception of the Acheulean is basically a typological constraint. It does not take into account that, at interactive or independent levels, débitage and façonnage operations are interrelated parts of the Acheulean technological system and that both operations can be relevant sources for describing the technological patterns undertaken by hominins within this technocomplex. Boëda firmly insists that, ≪ . . . *avant d'affirmer l'exclusivité d'une conception de taille dans un gisement, il est nécessaire qu'une analyse technologique prenne en compte l'ensemble du matériel, et donne une perception réelle du ou des systèmes de production en présence.*≫ This statement makes clear that to understand the technological systems that define the Acheulean at local and global scales, it should be critical to count on the study and description of all the operational principles involved in it. Various authors have been explicitly aware of this multicomponent characteristic of the African Acheulean (Isaac 1977; Texier and Roche 1995). Villa (2001: 121), for instance, stresses that after their appearance in the African record, LCTs "become simply a component of a variable technological repertoire."

Recently, some authors have employed Boëda's conception and started to respond to Gowlett's challenging questions by examining non-hand ax tool categories from Acheuelan assemblages, such as cores (Torre et al. 2008; Torre 2009; Torre and Mora 2005). This procedure is uncommon among most scholars interested in the definition of the Early Acheulean. A growing number of researchers, however, have pointed out the connection of some complex reduction strategies and the volumetric conception implicit in the Acheulean technocomplex (Rolland 1995; Schick 1998; DeBono and Goren-Inbar 2001; Lycett et al. 2010). A recent contribution to a most holistic view of Acheulean lithics came from ST site Complex in the type section of Peninj, where no formal LCTs have been recovered, and researchers there instead focused on cores (de la Torre 2009). De la Torre suggests there exists a technological and volumetric link between those hominins implementing core "bifacial hierarchical centripetal reduction" in the type section (where almost exclusively débitage components are found) and those partaking in hand ax production in the escarpments (where formal LCTs are present). De la Torre argues that this link between apparently disparate assemblages (where the divergence is obviously related to the bimodal expression: presence/absence of LCT) can be found in the fact that hominins would be able to implement the same volumetric principles in their knapping (débitage) of hierarchical centripetal cores and in their shaping (*façonnage*) of LCTs (Ibid.: 103).

A comprehensive critique to de la Torre's diagnosis of the so-called bifacial hierarchical centripetal core is presented elsewhere this text (*Chapter* 9). That alternative interpretation of the reduction strategies observed in the type section core assemblages will cast significant doubt on the specific link as so claimed by de la Torre in the particular case of Peninj. Although unsupported conclusions are provided in his analysis of the Peninj assemblages, it is important to acknowledge the merit of de la Torre's approach, because it has been one of the few attempts to put into practice Boëda's conceptions on the complex web of technical interrelations that form the Acheulean knapping structure.

Further efforts are needed to enlarge and complete our understanding of what constitutes the technological universe of the Acheulean technocomplex. This means that to overcome the limitations imposed by the type-fossil approach, researchers must focus on a holistic definition of the technical procedures that are behind the Acheulean technocomplex, in which principles of predetermination and standardization could have been present at various levels of core exploitation and tool production (Diez-Martín 2005). This would include a closer look at other components of Acheulean assemblages: the débitage operational sequences (the reduction methods for the production of both large and small-to-medium-sized flakes) and the fraction of the *façonnage* operational sequences aimed at shaping small artifacts (Diez-Martín 2002). Further investigation into the way in which the concepts of standardization and configuration are applied to the shaping of small objects must be conducted to elucidate the equivalence of these principles in the large and small fraction of *façonnage* Acheulean sequences (Diez-Martín 2005: 206).

Current facts about the Early Acheulean

If we need to overcome the fossil-*directeur* and TMM approaches for holistically understanding Acheulean technological behaviors, then a critical inventory of the current information available should help to point the way. Such a review, including references to what we know and what still needs further investigation, is critical to establish future research goals in Early Acheulean studies. Traditionally, the continuum of the African Acheulean has been divided in three phases: Earlier, Later or Middle, and Terminal Acheulean (Clark 1994). This tripartite division is mostly based on the increasing progression in the symmetry of hand axes (Bar-Yosef 2006:

480; Schick and Toth 2001: 72). Taking into account the high variability, long temporal scale, and broad regional range of the Acheulean, the evolutionary validity of this sequence based on stylistic perceptions (and not variability conceptions) has been a matter of discussion and debate among archaeologists (at least in the European case, where this tripartite scheme has already been abandoned, Santonja 1995. We acknowledge that it could still be useful for the African case).

Taking into account that these types of boundaries are somewhat arbitrary, our inventory focuses on the so-called Early Acheulean, bracketed between the origin of the technocomplex and 1 Ma (Clark 1994: 454). Several sites in East Africa have been claimed to be the earliest examples of the Acheulean. These sites can roughly be dated to ∼1.7 to 1.6 Ma, including Konso (Asfaw et al. 1992; Beyene 2003; Suwa et al. 2007) and Busidima Formation at Gona (Quade et al. 2004; Semaw et al. 2009) in Ethiopia, and Kokiselei 4, West Turkana (Lepre et al. 2011; Roche and Kibunja 1994; Roche et al. 2003) in Kenya. Recent data suggest a similar chronological framework for the Acheulean occurrences located in the Vaal River of South Africa (Gibbon et al. 2009). Between 1.5 and 1 Ma, there are sites in Peninj (Diez-Martín et al. 2009a; Domínguez-Rodrigo et al. 2009a, 2009b; de la Torre 2009; de la Torre et al. 2008) and in the Middle and Upper Bed II in Olduvai Gorge (Leakey 1971; de la Torre and Mora 2005) in Tanzania; localities within the Chari Member in Koobi Fora (Isaac and Behrensmeyer 1997; Isaac and Harris 1997) in Kenya; Gadeb (Clark 1987; Clark and Kurashina 1979) in Ethiopia; and Sterkfontein (Kuman 1998; Kuman and Clarke 2000; de la Torre 2011) and probably Swartkrans (Clark 1991) in South Africa. Finally, although documented in secondary positions, the localities of Mwanganda and Chitimwe, in Malawi, have also been included in the Early Acheulean (Clark 1990).

Following current research trends (Semaw et al. 2009), it would be appropriate to include in the Early Acheulean all the sites traditionally labeled as Developed Oldowan (and other regional equivalences, such as the Karari industry, Ludwig and Harris 1998: 99). An in-depth reassessment of the multiple localities ascribed to the Developed Oldowan within an Acheulean framework is beyond the scope of this chapter, although a representative list of sites would probably include localities in Middle and Upper Bed II of Olduvai Gorge (Leakey 1971) in Tanzania; the Karari industry of the Okote Member in Koobi Fora (Braun and Harris 2003; Isaac and Harris 1997; Ludwig and Harris 1998; Rogers et al. 1994; Toth 1990) and various localities in the Chesowanja Formation (Gowlett et al. 1981; Ludwig and Harris 1998) in Kenya; the Developed Oldowan sites in Gadeb (Clark 1987;

Clark and Kursahina 1979), Melka-Kunture (Chavaillon and Piperno 2004; Chavaillon et al. 1979) and Bodo (Clark and Schick 2000), in Ethiopia; and Nyabusosi, in Uganda (Texier 1995, 2005) and Palmeirinhas (Clark 1990) in Angola.

Finally, it is worth mentioning other African sites located toward the upper boundary of the Lower Pleistocene (≤1 Ma). Many of these assemblages have been traditionally attributed to a middle stage of the Acheulean development (Clark 1994: 458) and apparently related to more complex mobility patterns, including more open and drier environments (Cachel and Harris 1998). A nonexhaustive list would include the lowermost stratigraphic members at Olorgesailie (Potts 1989), Kilombe (Gowlett 1991) and Kariandusi (Gowlett and Crompton 1994) in Kenya; various localities in the Daka Formation of Bouri (Schick and Clark 2000), various sites in the Melka-Kunture archaeological complex (Chavaillon and Piperno 2004, Chavaillon et al. 1979), Kesem-Kebana (Wolde-Gabriel et al. 1992) in Ethiopia, and Thomas Quarry 1 (Raynal et al. 2001) in Morocco. Figure 10.1 shows the geographical location of all the sites cited here.

We will now present a brief review of the current available information for the sites formally included within the Early Acheulean (1.7–1 Ma), organized by the following generalized topics: technological characterizations; palaeoenvironmental settings and regional analyses; site formation contexts; lithic functional studies; and experimental approaches to understanding the Acheulean. For the sake of brevity, this review will exclude in-depth comment on sites formally attributed to the Developed Oldowan and will exclude final Lower Pleistocene Acheulean sites.

Technological characterizations

Technological descriptions of the oldest sites formally included within the Acheulean technocomplex are far from being comprehensive. Most of the publications reporting on the Early Acheulean include brief summaries and reports of these findings (Asfaw et al. 1992, Beyene 2003, Lepre et al. 2011; Roche et al. 2003) and acknowledge an early phase of study (Quade et al. 2004: 1538). Certainly this situation is related to the meager collections reported from most of these sites (Roche et al. 2003: 670) and the lack of publications devoted to an in-depth technological treatment of these assemblages. To date, the basic trait reported from the oldest Acheulean sites point to the appearance of the first LCT (“crude” hand axes, cleavers, and picks) in the archaeological record. A variable percentage of these artifacts have been shaped on large flake blanks (Roche et al. 2003, Semaw

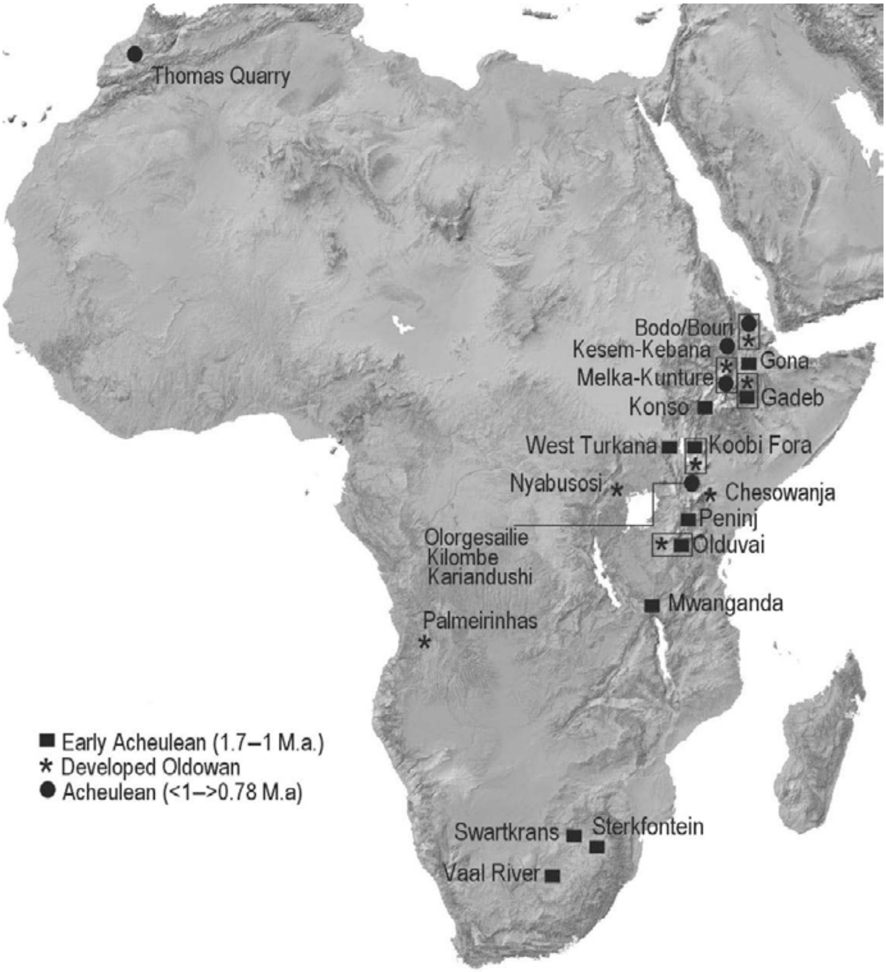

FIGURE 10.1. Geographical location of Acheulean sites between 1.7 Ma and 1 Ma.

et al. 2009), confirming the idea that one of the most relevant technical advances related to the new technocomplex is the ability to detach large flakes from large cores (Isaac 1972: 409). This evidence supports the fact that at this point hominins were already aware of the advantages of using these large flakes for shaping the new forms they were interested in: relatively thin forms related to almost completely peripheral natural cutting edges that needed limited subsequent knapping (thinning and peripheral edge configuration).

Hominins also used quite large (particularly in the case of Konso, where LCT could reach maximal lengths in excess of 250 mm), relatively flat, cobbles for crude hand ax or trihedral pick production, however (Asfaw et al. 199; Roche et al. 2003: 670; Semaw et al. 2009: 186). When other components of these Earliest Acheulean assemblages are turned to, information is more generic and less specific. Cores, sometimes of big dimensions, are briefly mentioned in some cases, although explicit information regarding reduction strategies carried out by hominins at these Acheulean spots is not available. Flake samples are constituted by a varied morphometric range, from small to larger than 10 cm, although relevant technological information relevant to our understanding of exploitation processes is lacking. Medium-sized retouched artifacts, such as scrapers, have been cited in Kokiliselei 4 (Roche et al. 2003: 670), although almost nothing is known about the medium-sized retouched component of the Early Acheulean, if such a thing does exist as a coherent entity.

Regarding assemblages ≥1.5 Ma, the Early Acheulean of Koobi Fora is basically defined by the production of flakes up to 164 mm from large boulders, which follow a single platform strategy (Ludwig and Harris 1998: 101), and by the paucity of LCTs (Isaac and Harris 1997). A substantial amount of what we know about the Early Acheulean comes from the Tanzanian regions of Olduvai Gorge and Lake Natron. At Olduvai Gorge, Mary Leakey excavated and studied an impressive collection of lithic artifacts related to the Developed Oldowan/Acheulean interface in Middle and Upper Bed II sites, a stratigraphic interval critical to our understanding of the origin of the Acheulean in the Olduvai Basin (Leakey 1971).

Recent comprehensive reanalysis of Leakey's collections by de la Torre and Mora (2005), from more technological and operational perspectives, has added a fresh and valuable look to the Early Acheulean in Olduvai. As other authors previously did (Gowlett 1986; Stiles 1979; Davis 1980; Jones 1994), these authors underpinned some of the weaknesses of Leakey's model, particularly evident in their reanalysis of the normative Acheulean site of EF-HR (De la Torre and Mora 2005: 95–115) and the normative Developed Oldowan B site of BK (Ibid.: 177–189). In agreement with their study, these authors have been very critical with the current validity of the Developed Oldowan concept (de la Torre and Mora 2005: 228); however, and this is particularly evident in the case of BK (Kyara 1999, de la Torre and Mora 2005) and SHK (Leakey 1971: 167), old lithic collections are partially or incompletely preserved. In fact, de la Torre and Mora (2005: 177) recognized that, owing to the contextual shortcomings observed in BK, they could not undertake a comprehensive analysis of the BK collection.

Recent fieldwork in BK has confirmed that old excavations did not collect some valuable lithic information related to the smaller components of assemblages. This bias is relevant to an accurate assessment of the technological variability seen in Middle and Upper Bed II sites (Diez-Martín et al. 2009b; Domínguez-Rodrigo et al. 2009c). These new findings indicate that to offer new views related to the most relevant technological issues in Olduvai Gorge, it is urgent to have access to new and unbiased lithic samples retrieved from modern and controlled extensive archaeological excavations. At present, a new round of fieldwork undertaken by The Olduvai Paleoanthropological and Paleoecological Project (TOPPP) focuses on this goal. In-progress fieldwork is targeting several of the most relevant sites located above Tuff IIB sediments, to understand local variability from an interactive perspective that includes technological, taphonomical, and paleoenvironmental aspects thought to have shaped each of the various lithic aggregates.

One of the most comprehensive technological analyses devoted to the Early Acheulean comes from Peninj (West Lake Natron, Tanzania), where Isaac first undertook various rounds of intensive fieldwork and research (Isaac 1965, 1967; Isaac and Curtis 1974). Between 1995 and 2005, a research team lead by M. Domínguez-Rodrigo carried out a second round of fieldwork in this region (Domínguez-Rodrigo et al. 2009d). In the framework of this research project, the reexcavation of the two most representative Acheulean sites located in the Sambu escarpment was undertaken: ES2 or Lepolosi as locally known by the Maasai (MHS-Bayasi, following Isaac's nomenclature) and EN1 or Noolchalai (RHS-Mugulud). Although some general details have already been published (Domínguez-Rodrigo et al. 2009b; de la Torre et al. 2008), a comprehensive study on the technology of ES2-Lepolosi is still lacking. After more recent excavations between 2007 and 2010 (Diez-Martín 2008; Diez-Martín et al. 2009c) added to previous work (Domínguez-Rodrigo et al. 2009b), however, this site has produced a large lithic assemblage that is now under study.

The lithic collection retrieved from EN1-Noolchalai (RHS-Mugulud) has been the subject of two technological analyses (Domínguez-Rodrigo et al. 2009a; de la Torre et al. 2008) showing slight differences in their conclusions, mostly based on the disparate samples studied and on the divergent interpretation of large cores. Although de la Torre et al. (2008: fig. 9) have identified some surface specimens as examples of prepared cores for the production of large flake supports, Domínguez-Rodrigo et al. (2009a) stress the ambiguity of these technical patterns and the uncertainties related to such interpretation. De la Torre et al. (2008), in the same line as de la Torre and Mora (2005) for the case of Olduvai, must be credited for applying in their analysis a more comprehensive approach than other studies devoted to the study of the Early Acheulean. This approach has been sensible to the identification and reconstruction of complete operational sequences (both LCT and small flake production) observed in the Early Acheulean sites and to the complementarity of débitage and façonnage knapping conceptions.

Unfortunately, their assessment of EN1-Noolchalai cannot be taken as representative of the technological behaviors carried out by hominins at this site. The 508 lithic objects included in their study are far from representing a homogeneous sample (de la Torre et al. 2008: table 3). Some of these materials are only part of Isaac's collection, retrieved in the 1964 excavations, because the archaeological material currently stored in Dar es Salaam is an incomplete fraction (41%) of the total sample retrieved in the course of Isaac's fieldwork (Isaac 1967). Other objects were retrieved on surface during the 2001–2002 campaigns. Most of the LCTs (83%) and all of the large flake blanks studied by these authors come exclusively from these two sources. Conversely, most of the lithic objects retrieved from modern excavations (70%) are small flakes or undetermined fragments.

In 2007, a new project aimed at excavating in extension both Lepolosi (Bayasi) and Noolchalai (Mugulud) sites was started under the direction of F. Diez-Martin (Diez-Martín 2008, Diez-Martín et al. 2009, 2010), and fieldwork is still in progress. The reexcavation and further stratigraphic analysis carried out in Noolchalai have clearly shown that the 2002 excavation did not unearth the whole deposit, however, because it was focused on the overbank area and it did not reach the basal area of the channel, where most of the archaeological materials are vertically distributed. All of the LCTs and other heavy pieces in Noolchalai (i.e., cores and large flake blanks) occur precisely on the base of the channel infill, as it was already suspected in 2002 and widely documented in our 2009 and 2010 field seasons. The three LCTs reported in the excavation of 2002 (De la Torre et al. 2008) were retrieved precisely from the small area in which the irregular base of this channel was exposed. The area studied in 2009 and 2010 (12 m^2) has permitted us to retrieve a much larger collection of lithic implements, including large numbers of LCTs. This collection, which includes very good examples of standardized cleavers and symmetrically shaped hand axes, casts doubts on previous characterizations of the LCT sample, simplified toward the production of massive and crude scrapers, in which the principle of volumetric symmetry was not applied (de la Torre et al. 2008: 256–257, 262). That diagnosis is now compromised, because most recent fieldwork is unraveling a much more varied array of technological behaviors linked to the Early Acheulean of the escarpments in Peninj, including the ability or interest in configuring symmetric bifacial volumes and shaping normative cleavers. This more diversified pattern is in accordance with interpretations that stress the functional connection between different LCT groups, such as massive scrapers and hand axes (Goren-Inbar et al. 2008).

Paleoenvironmental settings and regional analyses

It has been generally accepted that the Acheulean technocomplex is part of a new home-range model, in which new environmental settings (including increasingly open and varied habitats) were frequented by *Homo ergaster* (Cachel and Harris 1998; Klein 2000; Potts 1998). In Olduvai Gorge, Richard Hay (1976: 181) was the first to point out that the Acheulean sites were preferentially located in fluvial contexts and much closer to inland areas, away from the lake floodplain where Oldowan sites occurred. Although the same model has been reported in other sites and regional contexts (Clark 1987; Clark and Schick 2000; Domínguez-Rodrigo et al. 2005), our understanding of the regional integration of environmental contexts and archaeological evidence during the Early Acheulean is rather poor, as has been recently acknowledged by Semaw et al. (2009: 187). Certainly, beyond comprehensive environmental descriptions of locales (Clark 198; Domínguez-Rodrigo et al. 2009a, 2009b), little is known about dynamic lithic flow systems (Isaac 1986) in a regional scale during the Early Acheulean. This kind of regional and spatial analysis of archaeological data has been successfully applied to other Oldowan contexts, however (Blumenschine and Masao 1991; Blumenschine et al. 2008).

Hay's identification of the preference for fluvial contexts in more open and less vegetated landscapes during the Early Acheulean has been supported by other evidence (Domínguez-Rodrigo et al. 2001a; Harris and Capaldo 1993: 212; Rogers et al. 1994). The oldest Acheulean examples seem to match this ecological setting. Kokiselei 4 is located on the swampy floodplain of a watercourse draining toward a paleo-lake in a relatively open environment (Roche et al. 2003: 670). A similar setting, in open grasslands adjacent to secondary drainage systems of the paleo-Awash, has been suggested for the Early Acheulean sites at Gona (Quade et al. 2004: 1538).

One of the most relevant cases in which paleonvironmental information is integrated within a comprehensive regional framework during the Early Acheulean comes from Peninj. The archaeological work undertaken in Lake Natron from 1995 to 2005 was intended to continue the original "landscape archaeology project" envisioned by Isaac for the Peninj region (Domínguez-Rodrigo et al. 2005, 2009d). Continuous fieldwork in the three areas and two depositional moments documented within the upper sands with clays unit in the upper section of the Humbu Formation (north and south escarpments, and the type section area) have produced what can be currently considered to be the only regional integrative spatial approach of archaeological and paleoecological data (*sensu* Isaac) for the Early Acheulean (Domínguez-Rodrigo et al. 2001a, 2005, 2009d).

In Peninj, the regional technological system appears to be a complex spatial structure that co-varies with changes in landscape. The paleosurface located on top of Tuff 1 (the ST site complex) includes several sites in the type section area deposited in the channel system of an alluvial fan area, in a deltaic environment relatively close to the paleo-lake (Domínguez-Rodrigo et al. 2002, 2005, 2009e: 105). Hominin impact on this alluvial landscape, related to occasional processing of herbivore carcasses, must have been relatively low. Lithic distributions show a low-density pattern in which lithic collections are predominantly related to the production of small, nonretouched flakes. This expedient knapping behavior is in accordance with a context of sporadic hominin presence in this alluvial landscape. Despite the absence of LCTs in the ST site complex, these sites can be related to the Acheulean technocomplex owing to various technical criteria observed in the lithic collections (Diez-Martín et al., this book, chapter 9): homimins were here, using large flakes as core blanks and several hand ax resharpening flakes have been documented. This evidence indicates that the lithic assemblages documented here are the result of an intense lithic flow, in which LCTs have been the subject of a remarkable lithic transfer (input for use and maintenance and output for maintenance and discard) driven by locational and functional constraints (i.e., type of activities carried out here within the regional spatial structure, *sensu* Foley 1981).

In a later depositional moment, between Tuffs 4 and 5, the regional lithic spatial structure changed in conjunction with other environmental transformations (Luque et al. 2009). This period corresponds with another alluvial phase, in which a more structured and larger fan delta expanded toward the interior of the basin. During the Tuffs 4–5 depositional interval, the deltaic system was more distant to the lacustrine margin than in the ST site complex depositional moment. This more distant location to the lake floodplain correlates with the classic Acheulean sites (i.e., assemblages where formal LCTs have been recorded) located both in the Sambu escarpment (e.g., ES2-Lepolosi and EN1-Noolchalai) and the type section area. In the Natron area, a clear correlation seems to exist between the presence of LCTs and a more distal location relative to the lake. This observation has been the basis for the formulation of the *ecological hypothesis of the Early Acheulean* (Domínguez-Rodrigo et al. 2005), which defends the premise that the LCTs were used in alternative ecological contexts (away from the lake margins and carcass processing) for alternative purposes. This perspective envisions a clear functional frame for the appearance of LCTs in the lithic repertoires, in the context of enlarged home-ranges and more varied economic activities. This interpretation is in accordance with other perspectives that see LCTs as multifunctional artifacts suitable for hunting-gathering parties away from home bases (Keely 1980: 161; Jones 1994: 295; Potts 1994: 20).

Currently, Peninj constitutes an exceptional window to the landscape organization of the Early Acheulean, showing similar qualities than other slightly younger records, such as Olorgesailie, where analogous relationships between specific paleoecological contexts and the presence of LCTs have been unraveled (Potts 1989, 1994, Potts et al. 1999). This spatial quality is due to the uncommon preservation of patterns of intersite and regional technological variability. This variability is archaeologically expressed through the observation of different technical solutions in different ecological contexts in the same region and broad chronological segment (i.e., 1.5–1.2 Ma). Further landscape research in the Peninj area is in progress, particularly focused on a closer understanding of the spatial and technical interconnections during the T4-T5 depositional interval in both the type section and the escarpments (Diez-Martín 2008; Diez-Martín et al. 2009, 2010).

Site formation contexts

The close relation between Early Acheulean sites and alluvial contexts make it necessary to evaluate the influence of postdepositional processes in the formation of these aggregates and thus the effect of natural forces in the behavioral patterns (i.e., technological, economic, or spatial) that we intend to reconstruct through the study of occurrence distributions (Isaac 1984). It has been acknowledged that many Acheulean patches, particularly those in which the LCT component is predominant in connection with scarce presence of faunal remains, might have been heavily influenced by hydraulic transport (Isaac 1977; Schick 1992). Not all of the studies devoted to the analysis of Acheulean occurrences have properly considered the role played by taphonomic processes in the archaeological record, however (Eren et al. 2010; Petraglia and Potts 1994: 239).

Regarding the oldest Acheulean sites, for example, little is known about their in-situ or surface context in relation to fluvial environments (Quade et al. 2004) or other disturbances (Roche et al. 2003). This situation makes it difficult to evaluate how much behavioral integrity exists in Early Acheulean assemblages. Although a wealth of referential frameworks on the influence of fluvial traction (i.e., displacement, spatial redistribution, reaccumulation, or abrasion) on archaeological materials has been gathered in a variety of experimental, observational, and actualistic studies (Cheetham 1976; Eren et al. 2010; Grosman et al. 2010; Harding et al. 1987; Schick 1987a, 1987b, 1991; Turnbaugh, 1978), taphonomic and geoarchaeological assessments are rarely made for Early Acheulean sites.

The work by Schick (1997) stands as the most comprehensive approach devoted to the effect of fluvial forces on the lithic component of the archaeological record. Shick's work established an experimental model as a reference for the interpretation of archaeological sites in Koobi Fora, which included some Early Acheulean examples (i.e., FwJj33 and 63). At Olduvai Gorge, Petraglia, and Potts (1994) evaluated the effect of water flow in several archaeological sites of Bed I and Bed II; however, this analysis is limited to few examples selected throughout the sequence. To understand the link between depositional history and behavioral inference in the context of the Early Acheulean at the Olduvai basin, renewed fieldwork should be undertaken targeting Middle and Upper Bed II sites (those formally ascribed to the Developed Oldowan/Acheulean interface). Work in progress by TOPPP is examining this very issue with a close look at taphonomic and geoarchaeological factors present within various sites located in this stratigraphic interval.

Currently, fine-grained geoarchaeological interpretations are under way in the Acheulean sites located on the escarpments in Peninj. These analyses support previous taphonomical perspectives for ES2-Lepolosi as a site devoid of intense fluvial traction (Diez-Martín et al. 2008; Domínguez-Rodrigo et al. 2009d), although they emphasize the influence of postdepositional hydraulic disturbance for EN1-Noolchalai (Diez-Martín et al. 2010).

Lithic functional studies

To date, the function of the LCTs within Acheulean toolkits is still a matter of debate (Villa 2001). There are two basic and interrelated levels of hypothetical interpretation of LCTs. The first interpretation links these artifacts to the spatial component of hominin behavior, arguing that LCTs, regardless of function (including as a possible source of flakes), are related to an increase in hominin regional mobility (Hay 1976; Keely 1980; Jones 1994; Potts 1994). The *ecological hypothesis* formulated from the Natron evidence (Domínguez-Rodrigo et al. 2005) and indirectly supported by the paleoenvironmental location of other Early African Acheulean evidence envisions a close connection between LCTs and activities undertaken in close connection with the regional landscape. The second level of interpretation has focused on the specific tasks undertaken with these artifacts. The presence of LCTs in Old World lithic assemblages has been explained by its advantage in the butchery and processing of large herbivore carcasses (Bello et al. 2009; Jones 1980, 1994; Schick and Toth 1993; Yravedra et al. 2010), or in vegetal tissue processing (Clark 1975, Jones 1994).

Direct functional evidence supporting either of these two alternatives in the Early Acheulean record is very meager. Microwear analyses are not yet available for Early Acheulean artifacts, probably because of the scarcity of well-preserved in-situ collections. At the ES2 site of Peninj, however, a phytolith sampling of both the paleosol sediment and three lithic artifacts showed that different types of plant residues were preserved on the cutting edge of the stone tools (Leguminosae of the *Acacia* genus) and the paleosol in which they were sedimented (mostly a variety of grasses). This sharp difference was interpreted to be evidence of tool use, suggesting that the artifacts of ES2 were used in plant processing activities, particularly for chopping acacia wood (Domínguez-Rodrigo et al. 2001b, 2009b). This evidence of plant processing with lithic implements coincides with previous microwear analyses of several stone tools carried out in Koobi Fora (Kenya) in a similar chronological context (Keely and Toth 1981).

Some authors have cast doubt on the validity of these results, however, by arguing that the ES2 site was formed in an alluvial context with a considerable level of water traction and postdepositional disturbance that would be at odds with a reliable preservation of phytoliths on the stone tool edges (de la Torre et al. 2008: 246). Although further phytolith analyses in ES2 validating the original results are needed (work is in progress), this criticism lacks a solid empirical basis when considering current sedimentary information. Recent sedimentary and soil micromorphology analyses undertaken at both ES2 and EN1 reinforce previous interpretations (Domínguez-Rodrigo et al. 2009d) and suggest, *contra* de la Torre et al. (2008: 247–248), that although a low-energy sedimentary context of fine-grained mud in a deltaic floodplain in the vicinity of a channel is clearly observed in ES2 (Diez-Martín et al. 2009; Domínguez-Rodrigo et al. 2009b), the lithic accumulation of EN1 was mostly recovered from within a channel representative of a relatively high-energy water flow that might have been responsible for relevant postdepositional bias in the archaeological collection (Diez-Martín et al. 2010). Thus, the woodworking evidence for some Early Acheulean artifacts suggested by the phytolith analyses carried out in ES2 is still supported. Furthermore, contamination (from where, because the soil does not contain the same type of phytoliths as the tools) does not selectively accumulate phytoliths on only the edges of the tools.

This woodworking evidence offers a very specific and local explanation for the functionality of LCT implements. In the type section area and during the T4-T5 depositional interval, the production of LCTs is also associated to large herbivore carcasses and probably meat processing (Diez-Martín 2008; Domínguez-Rodrigo et al. 2005). This evidence supports the interpretation of LCTs as multipurpose tools within a complex web of functional contexts across the regional landscape.

Experimental approaches to understanding the Acheulean

The term *experimental* used here follows the bounded definition of Eren (2009), who for the sake of discussion limits an archaeological experiment to the realm of actualistic study involving the manufacture, use, and discard of replicated prehistoric tools. Adhering to this bounded definition, some classic experiments exist in the anglophone literature about Acheulean industries. Although these experiments are "classic" in the sense of being innovative for their time (e.g., Bradley and Sampson 1986; Newcomer 1971), they are also classic simply because experimental approaches to the Acheulean are fairly uncommon, at least relatively to the number of experiments conducted involving the Oldowan or late Pleistocene industries. Thus, researchers looking for middle-range data sets to guide their behavioral interpretation of Acheulean sites and assemblages traditionally have had few published experimental collections at their disposal.

Speculatively, the lack of experimental study into the Acheulean ultimately might lie in the amount of skill needed to successfully proper hand axes successfully (Edwards 2001: 606). Novice knappers correctly pursue core-flake or retouch replication experiments, whereas expert knappers often engage in experiments on more challenging reduction sequences. Thankfully, this trend seems to be waning, as evidenced by a surge in replicative studies involving Acheulean tool manufacture, use, and taphonomy (e.g. Machin et al. 2007; Shipton et al. 2009).

The experimental manufacture of hand axes. Owing to the dearth of Acheulean replication studies, a plethora of elegant models regarding Acheulean stone tool reduction have yet to be tested (e.g., White 1998b), although some have done this (e.g., Shipton et al. 2009). This is unfortunate because "in seeking to ascertain the method by which the stone implements and weapons of antiquity were fabricated, we cannot, in all probability, follow a better guide than that which is afforded us by the manner in which instruments of similar character are produced at the present day" (Evans 1872: 13). What does experimental replication tell us about Acheulean hand ax production?

Experiments have shown that hand axes and other large cutting tools can be made from large flake-blanks produced during large nodule reduction (through direct hard-hammer percussion, throwing, or fracture by fire, see Madsen and Goren-Inbar 2004; Toth 2001) or from raw corticated nodules (Bradley and Sampson 1986). Some have suggested that hard-hammer percussion (i.e., hammerstones) might have been used in the early Acheulean, whereas soft-hammer percussion (i.e., organic percussors like antler or bone) was used during the later Acheulean (Toth and Schick 2009: 272). Others have likewise argued that both soft- and hard-hammer percussion could have been used in a single hand-ax reduction sequence depending on the objective to be achieved or the particular toolstone being knapped (Edwards 2001: 608; Madsen and Goren-Inbar 2004). Experimental evidence for platform preparation and setup (e.g., through battering, see Baker 2006), as well for constraints stemming from tool stone quality and morphology, have also been put forward (Knowles 1953; Shipton et al. 2009; Toth and Schick 2009: 272).

Broadly, the tool production experiments conducted hitherto in the Acheulean offer inferences into how prehistoric hominids might have acted. It is necessary to emphasize that this is a vital endeavor. When carefully assessed with a critical eye, these experimentally derived inferences help to establish parameters of production variability that can be compared to and tested against the archaeological record, to better understand prehistoric behavioral variability (Shea, 2011). In our view, experiments involving Acheulean tool manufacture should continue to expand along three specific avenues of inquiry: (1) the continuation of tool production parameter exploration and modeling; (2) the establishment of more robust tool production markers to strengthen the inferences dependent on them; and (3) the investigation of experimental tool production and reduction through quantitative rather than qualitative assessment, to interpret the archaeological record more objectively.

Exploration of tool production parameters. The Acheulean industry, and hand axes in particular, beg for a more complete understanding of the factors that might influence artifact morphological and technological variability. Despite the extensive debates regarding the influence of reduction intensity, tool stone type, and nodule morphology on Acheulean artifact form, the dearth of experimental work that systematically examines each of these factors both in isolation and collectively is bewildering. Positively, Archer and Braun's (2010) recent three-dimensional morphometric analysis of archaeological and experimentally knapped hand axes is a promising start. This analysis showed that "while several different factors influence LCT morphology . . . none in isolation can fully explain the variation captured using a morphometric approach" (ibid. 208). This result highlights the need for more experimental work that not only examines the collective and interactive influence of multiple production factors on artifact form and variability but also the impact of one factor on another. For example, is the reduction intensity of a hand ax influenced by toolstone type or nodule morphology?

One additional Acheulean tool production factor that has largely been ignored is knapper skill, although two studies are relevant. Geribas et al. (2010) conducted a flintknapping experiment to better understand the mental and technical gestures novices must learn to become experts. Although the participants in the study were asked to make "hand axes," they were required to make them of rectangular bricks. Despite the authors' assurances that "bricks have the same mechanical properties (choncoidal fracture) as stone" (Ibid., 2858), we remain skeptical. Furthermore, the preponderance of square edges that bricks possess requires extra expert knowledge that the novices in the study did not have (Ibid.). Thus, these factors probably limit the implications of their study to the more generalized realms of cognition, manual dexterity, and craft production, rather than to the anything about the Acheulean in particular.

Winton (2005) conducted a more pertinent experiment on the influence of skill and Acheulean hand ax production. Comparing the products of skilled and less-skilled knappers, she documented a number of morphological and manufacturing differences between the two groups. Most interesting, "deliberate asymmetry imposed by skilled knappers as in the case of plano-convex handaxes was quite distinct from asymmetry resulting from incompetent knapping efforts" (Winton 2005: 112). She is in no way convinced that differing prehistoric skill level among individual knappers alone is a sufficient explanation for variability in hand axes, but she does demonstrate that it is substantial contributing factor. Thus, like other Acheulean tool production parameters, the experimental examination of skill should be examined to better understand how it influences, and how it is influenced by, artifact form as well as other tool production parameters. For instance, by understanding the relationship between tool stone type and knapper skill, it might be possible not only to explain artifact form better but also to explain why particular forms appear the way that they do. Hypothetically, there might be more morphological variability in a set of hand axes not simply because they are made on a difficult tool stone, like quartz, but because novices produce more variability on a tougher tool stone than they do on a easier tool stone, whereas experts produce similar (smaller) amounts of variability regardless of the tool stone used. Nuanced understanding of all the variables that go into hand ax production and their interrelationships will hone our models of Acheulean material culture.

There are some Acheulean tool production parameters that are difficult, if not impossible, to extract directly from the archaeological record and thus must be gleaned experimentally. One example is manufacturing time for a hand ax (e.g., Edwards 2001; Madsen and Goren-Inbar 2004). Some researchers eschew the investigation of this variable, such as Barkai (2009), who states

> The efficiency of highly skilled and experienced modern knappers equipped with modern Western conceptions of time, energy, and progress cannot and should not be used as a standard for Acheulian knappers. Modern experiments are extremely useful in reconstructing reduction sequences, decision-making processes, and technological in-sights, but they have nothing to do with the role of time and efficiency in the Acheulian world. The fact that a modern knapper is able to produce 10 large blanks in 10 minutes and then shape a blank into a biface in 1.5 minutes is irrelevant and should not be used to describe the production of Acheulian bifaces as a "very speedy process." We know nothing about the time it took Acheulian hominins to produce a biface, their conceptions of time and efficiency, or the meaning of these concepts in the Acheulian. We had better not apply our modern concepts to Acheulian knappers or, even worse, use these concepts to compare the "efficiency" of Lower Paleolithic biface producers with twentieth century adze producers.

The problem with Barkai's view is that he thinks that the measurement of production time makes an assumption about native Acheulean knappers' time and efficiency, but it does not. A paleontologist can estimate how fast a *Tyrannosaurus* rex can run but does not assume or know whether that maximum speed was ever achieved or how often. The estimation of *T. rex* maximum speed is still valuable as an upper limit in the creation of models for hunting or other aspects of behavior and evolution. By estimating that maximum *T. rex* speed falls somewhere between 25 and 40 miles per hour (mph), the paleontologist can hone his/her behavioral models rather than be forced to consider "what if" and produce models that take into consideration speeds of 50, 60, 70 mph, and so on.

Similarly, that a modern flintknapper makes a refined hand ax in two hours serves has a valuable benchmark that reveals that in some cases hand ax production was not always an "expedient" technology (Edwards 2001: 609), but that it did not *necessarily* encompass a full day's work either, as some other technologies might (e.g., Solutrean laurel leaves). The work of Madsen and Goren-Inbar (2004: 42) demonstrates for Acheulean giant core and hand ax reduction that hand axes and cleavers can indeed be made quickly, but large flake-blank production can be more costly in time (ca. 6 hours). Viewpoints like Barkai's fail to understand that *production time* (or efficiency, or functionality, or skill, or tool stone constraints) for a hand ax is simply a modern measurement to be used as a parameter for refining our correspondingly modern models and reconstructions of the Acheulean. When it comes to the study of the Paleolithic, the only possible intellectual pursuit is an etic one.

Establishing more robust tool production markers. Our reconstructions of how Acheulean hand axes were produced depend partly on *canonical variates* (Wenban-Smith 1989) such as platform *lipping*, platform dimensions, or bulb diffuseness. Unfortunately, and to the dismay of flaked stone analysts the world over, there are few if any flaked stone attributes that are mutually exclusive to any one knapping technique, and vice versa. As Tony Baker (2006, see also Bradley and Sampson 1986: 43) notes, modern knappers can produce similar platform attributes with both soft- and hard-hammer percussion, depending on where the percussor makes contact with the core and how the percussor is held by the knapper. This means that future experimental work into the Acheulean should focus on discovering and establishing robust tool production markers that are exclusive to the knapping technique that created them. To do so, flaked stone analysts might have to improve their understanding of fracture mechanics substantially (see www.ele.net), examine attributes that they might not normally study (e.g., ventral ripples), use newer techniques (e.g., is there a geometric morphometric difference between hard-hammer platform lipping and soft-hammer platform lipping?), or develop new techniques (could a microtopographic analysis of ventral ripples through GIS yield patterns diagnostic of soft- versus hard-hammer percussion?). The benefits of such work should be self-evident the discovery of more robust production markers will not only give us greater confidence and detail in our interpretation of hand ax production but also allow for much more nuanced comparison of Acheulean assemblages chronologically and temporally.

Quantitative assessment and comparison of experimental and archaeological assemblages. Although there is a dearth of replication experiments involving the Acheulean, those that do exist show the synergistic association that can arise from combining experimental replication and quantitative assessment. In Bradley and Sampson's (1986) landmark paper, the authors used tabulated traits, numeric indices, and analytical formulas to quantitatively compare Acheulean archaeological assemblages from Caddington, U.K., to experimentally replicated ones. Their "small but important advance in methodology" (Bradley and Sampson 1986: 42) was more than important and anything but small. By using quantitatively analyzed experimental assemblages as benchmarks, they were able to create expectations of knapping debris and test these against the archaeological record. Ultimately they could prove that only two reduction sequences were present in their archaeological assemblages, hand ax manufacture, and Levallois flake production.

Recent work using Bradley and Sampson's (1986) now standard approach include, among others (e.g. Archer and Braun 2010; Baker 2006; Madsen and Goren-Inbar 2004), Shipton and colleagues' (2009) experimental analysis of hand ax production in India. By experimentally replicating hand axes and cleavers on the exact limestone blocks that would have been available to Stone Age knappers, they were able to deduce two separate reduction sequences for hand axes versus cleavers. Statistical testing then empirically verified that patterns that they inferred from the experiments were mirrored in the archaeological record, namely that hand axes were predominately made on slabs, whereas cleavers were generally made on flakes. Their work revealed that Acheulean hominins in India organized their manufacture strategies, which in turn implies a significant amount of forward planning and organization.

The experimental use of hand axes. Thus far experiments in the use of the hand ax have been mostly limited to (1) the demonstration of what a hand ax could have been used for (Jones 1981, 1994; O'Brien 1981, 1984; Schick and Toth 1993) and (2) the experimental falsification of those potential uses (Machin et al. 2007; Whittaker and McCall 2001). Although these sorts of experiments, especially the latter, are invaluable, it should be noted that comparisons between experimental and archaeological hand ax use patterns are few and far between. Although several uses have been attributed to hand axes (e.g., Wayman 2010), archaeologically we definitively know of only two: butchery (Keely 1980; Mitchell 1995) and woodworking (Dominguez-Rodrigo et al. 2001). Of these, experiments played a part in proving only the former (they were not necessary for the latter), and until recently, it was a meager part at that. The recent comparative experimental and archaeological analyses conducted on hand axes from Boxgrove (Bello et al. 2009) and Aridos (Juana et al. 2010; Yravedra et al. 2010) have significantly bolstered the evidence for hand axes as butchery tools. The idea of the hand ax as a Lower Paleolithic Swiss army knife requires a substantial amount of work and highlights the glaring need for more experimentally created use-wear and taphonomic reference collections involving Acheulean tools.

Experiments involving the contextual taphonomy of Acheulean assemblages. When sites that are hundreds of thousands, or even millions, of years old are being considered, accurate interpretation of contextual taphonomy is essential to understanding whether stone and bone associations and modifications are behaviorally meaningful, or a product of disturbance. Experimental replication of taphonomic disturbances has only recently been experimentally directly applied to the Acheulean and generally does not reveal behavioral patterns so much as questions them (e.g., Eren et al. 2010). For instance, the recent trampling experiments by Gaudzinski-Windheuser et al. (2010) caused them to question whether positional and numeric variability of bone modification marks can actually be diagnostic of behavioral patterns or of postdepositional processes at the Acheulean site of Gesher Benot-Yakov. Grosman et al. (2011) conducted a fascinating experimental replication of hand ax "rolling" and "tumbling," which demonstrated with 3-D scanning that archaeological specimens yielded the exact sorts of damage evidenced on the replicated specimens.

Conclusion

The type-fossil paradigm, formulated during the infancy of prehistoric science, has been very influential in archaeological analyses of flaked stone, and particularly in the formation of how we look at the Acheulean technocomplex. Some of the most relevant issues linked to the dawn of the Acheulean (the Developed Oldowan/Acheulean interface) originated around the near-exclusive use of LCTs as a source of cultural and evolutionary inference. The long-lasting use of the type-fossil paradigm as a tool of multiple inference, shaped by the cultural-historical approach of the twentieth century, highlights its lengthy role in shaping our understanding of human evolutionary processes.

This work has argued that the type-fossil paradigm has found a powerful contemporary sequel in the recent revitalization of Clark's TMM for the African Early Acheulean. The extensive use of Clark's nomenclature has reinforced yet again the equation that Acheulean = hand axes. This oversimplified equation, sometimes justified by its synthetic quality, diverts our attention from other technological components necessarily associated with the Acheulean concept. It is undeniable that the appearance of the first LCTs in the archaeological record, whether produced from large cobbles or flakes, marks a definitive milestone in the process of hominin technological evolution. This reduction sequence has been recognized in the oldest examples of the Acheulean innovative process, currently located in various East African sites at around ~1.7 Ma. It is also true that LCT reduction is intimately related to what we define as Acheulean at various levels. Therefore, a tremendous scientific effort has been put into the analysis of LCTs from a variety of perspectives to assess the technological, cultural, behavioral, economical, conceptual, and even symbolic facets of the hominins that produced them.

Few authors have recognized the need to balance a focus on LCTs, the most conspicuous signals of the Acheulean, with other interrelated components of Acheulean toolmaking, so that we may finally be able to explore a more holistic version of the Acheulean concept. Thus, it is probably time that renewed efforts are made in this direction. There still is much to learn about the hand ax type-fossil, however, and we must not abandon new and in-depth analyses of it (e.g., Lycett and von Cramon-Taubadel 2008).

Thanks to the last forty years of research, we now know much about the dawn of the Acheulean and its relationship to other fundamental events in human evolution. This chapter has tried to pinpoint those research avenues in which further work needs to be done to understand the Acheulean better. This work has argued that it is of utmost importance to overcome the dominance of fossil-type paradigm with a more balanced approach. Integrative technological, regional, economical, functional, and experimental analyses are vital to this effort. To be sure, early Acheulean researchers face a fascinating and promising time of scientific exploration.

Acknowledgments

MIE is financially supported by a Leverhulme Early Career Fellowship, but during the writing of this manuscript initial draft he was supported by a National Science Foundation Graduate Research Fellowship, by the Department of Anthropology and QUEST Archaeological Research

Program at Southern Methodist University, and by Mustafa, Kathleen, and Nimet Eren. He is grateful to Rebecca Catto for support. Both FDM and MIE are grateful to Stephen Lycett for reading over this chapter and for his insightful discussions about its contents. Although not all of his suggestions were included here, all were certainly appreciated.

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