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.

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