

THE BORDES-BINFORD DEBATE: TRANSATLANTIC INTERPRETATIVE
TRADITIONS IN PALEOLITHIC ARCHAEOLOGY

by

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Presented to the Faculty of the Graduate School of
The University of Texas at Arlington in Partial Fulfillment
of the Requirements
for the Degree of

DOCTOR OF PHILOSOPHY

THE UNIVERSITY OF TEXAS AT ARLINGTON

August 2009

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ACKNOWLEDGEMENTS

I have had the good fortune to be mentored in my academic and professional life by a few truly dedicated and generous souls. Dr. Frank Harrold, whose patience and kind nature is legendary, has had such a positive influence on my life -- I cannot thank him enough. Dr. Karl Petruso, an extraordinary scholar, has provided invaluable mentoring and advice to me over the years and I am so grateful for his current role on my dissertation committee. Dr. Steven Reinhardt is worthy of so many accolades. I so appreciate his patience and intellect as I have taxed the limits of what most students have likely asked of their dissertation advisors. Thank you! I would be most remiss if I did not extol the personal and professional virtues of Dr. Elisabeth Cawthon, who has a special talent for making each person in her orbit feel special and gifted. Her subtle, but insistent, prodding for me to just get this dissertation done was exactly what I needed, and for that I am most grateful.

I would also like to extend my deepest gratitude to several individuals at Western Carolina University who supported me in completing this dissertation. Dr. Kyle Carter gave me the gift of time; for all those Thursdays in the library, thank you. Drs. Carol Burton, Beth Lofquist, and AJ Grube, gave me the pep talks I needed when I was down. Their support has been invaluable. Finally, I wish to thank my family and friends. Donald Mitchell, in particular, knows what this accomplishment means to me. My parents, Earl and Cathy Canady, nurtured my love of education. I wish I could show them just how much I appreciate them. I am most grateful, however, to my husband, Steve Wargo, who deserves so much more than a thank you. Truly, I couldn't have done this without him. Lastly, I would like to dedicate this work to my children, Shea and Fin. They are the reason I could not give up on seeing this task to the end.

July 10, 2009

ABSTRACT

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In the 1960s, Lewis Binford, a young American archaeologist, challenged François Bordes, a venerable French prehistorian, over the interpretation of a taxonomy Bordes had developed to describe stone tools of the European Middle Paleolithic period (Mousterian). Ostensibly about the meaning of variability in Mousterian stone tool assemblages, the Bordes-Binford debate exposed a deep rift in the field of archaeology about how the deep past should be studied and interpreted. The intellectual clash has been cast subsequently in dichotomous terms: old versus young, descriptive versus explanatory, idiographic versus nomothetic, Old World versus New World. The Bordes-Binford debate, however, was not merely a singular event in the intellectual history of Paleolithic archaeology. It is the main thesis of this work that the Bordes-Binford debate is emblematic of the differing traditions within the discipline of archaeology as it was practiced by American and French scholars and that an understanding of the debate furthers understanding of how archaeology developed and is practiced and conceptualized in those countries today. To that extent, the Bordes-Binford debate is best understood in its transatlantic context; that is, it grew out of an encounter and exchange

between protagonists who were profoundly influenced by their respective national and cultural experiences. The debate and its aftermath changed the practice of Paleolithic archaeology on both sides of the Atlantic.

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CHAPTER 1

INTRODUCTION TO THE BORDES-BINFORD DEBATE

We shall try to set out the facts; but scientific facts are always more or less coloured by interpretation, and each man's mind acts like a lens, which concentrates the rays according to its focus.¹

François Bordes

The archaeological record is static. It does not talk; it just sits there. It is contemporary. All my observations on the past; they are observations that I make in the present. And so if I am going to make any statements about the past, and those statements are about dynamics--are about events--then it is a problem of inference. And so the central problem is one of inference justification: not how to look but how to justify saying anything about what you have seen.²

Lewis Binford

The history of archaeology, like that of many disciplines, is punctuated by scholarly disagreements, large and small, most of which are lost and forgotten, their relevance reduced by the passage of time and the redirection of intellectual favor. A few of those disagreements, however, continue to resonate long after their resolution or conclusion either as cautionary tales or as emblems of pivotal shifts or seminal moments in the development of the field. One such disagreement that continues to merit attention in archaeology is the debate between American archaeologist Lewis Binford and French archaeologist François Bordes. Ostensibly about the meaning and interpretation of stone tool assemblages (or collections of associated tools) during the Middle Paleolithic (Mousterian), the Bordes-Binford debate, as it has come to be known,

¹ F. Bordes, *The Old Stone Age*, (New York: McGraw-Hill, 1968), 7.

² L. Binford as quoted in P. Sabloff, *Conversations with Lew Binford*, (Norman, OK: University of Oklahoma Press, 1998), 19.

actively spanned more than two decades during the 1960's and 1970's and has become a standard intellectual reference point in histories on the development of archaeology.³ Recent and current research on the variability within Middle Paleolithic stone tool assemblages continues to address the Bordes-Binford debate, breathing new life into the debate for each new generation of archaeologists.⁴

The Bordes-Binford debate has all the elements of a good yarn, capturing the interest of specialists and non-specialists alike; one could cast the story in so many ways. There are nationalistic overtones in the upstart American versus the courtly Frenchman, a May-December (maybe November) element with a Young Turk battling a tested old(er) warrior, and a sprinkling of the inevitability of change in the traditional view giving way to one that is more progressive and modern. In some ways, the Bordes-Binford debate is all of those things. But from a historical perspective, the importance of the Bordes-Binford debate has yet to be told. The contention of this author is that the Bordes-Binford debate still has much to tell us, although this research will, by no means, be the definitive work on the matter. Why, for example, do so many archaeologists reference the debate yet, beyond a few specialists, know so little about it? Why was Binford, an American archaeologist not even initially interested in the Paleolithic, the one to initiate the debate with Bordes? And, why does the debate still merit reference from the community of archaeologists conducting research on Mousterian variability when the substance of the debate has moved well past the simple dichotomous view of style versus function? The intent of this research is to posit an answer to those questions. It is the main thesis of this work that the Bordes-Binford debate is emblematic of the differing traditions within the discipline of archaeology as it was practiced by American and French scholars and that an understanding of

³ See B. Trigger *A History of Archaeological Thought*, 2nd ed. (New York: Cambridge University Press, 2006); A. Wylie, *Thinking from Things: Essays in the Philosophy of Archaeology* (Berkeley, CA: University of California Press, 2002); G. Willey and J. Sabloff, *A History of American Archaeology*, 3rd ed. (San Francisco, CA: Freeman, 1993); and A. Kehoe, *The Land of Prehistory: A Critical History of American Archaeology* (New York: Routledge, 1998).

⁴ See N. Rolland and H. Dibble "New Aspects of Middle Paleolithic Variability in Western Europe," *Nature* 255 (1977): 251-252; and D. Gordon "Mousterian Tool Selection, Reduction, and Discard at Ghar, Israel," *Journal of Field Archaeology* 20, no. 2 (1993): 205-218.

the debate furthers understanding of how archaeology developed and is practiced and conceptualized in those countries today. To that extent, the Bordes-Binford debate is best understood in its transatlantic context; that is, it grew out of an encounter and exchange between protagonists who were profoundly influenced by their respective national and cultural experiences. The debate and its aftermath changed the practice of Paleolithic archaeology on both sides of the Atlantic.

Professional archaeologists in the United States and in France would appear, upon cursory examination, to share the same disciplinary goals and objectives, a standard toolbox of practices, and a common intellectual framework. Scratch beneath the surface, however, and one reveals distinct, although not necessarily conflicting, historical trajectories for the practice of archaeology on each side of the Atlantic. These distinct historical trajectories tracked each other in a parallel and largely unacknowledged fashion throughout the formative development of archaeology as a discipline from the mid-nineteenth to mid-twentieth century. The obstacles of geography and language precluded frequent transatlantic intellectual exchange and collaborative work, which limited opportunities for examining the divergent approaches to archaeology. These barriers began to drop in the decades following the Second World War, when American archaeologists flush with resources and access to convenient, cheap travel options sought out research opportunities in Europe.⁵ Amidst this new atmosphere of cooperation, the intellectual differences were politely ignored until the 1960s when the Bordes-Binford debate exposed the fundamental and foundational differences inherent in the scholarly approaches to archaeology as practiced in France and in America.

On a superficial level, Bordes and Binford shared the same objectives, to explain human behavior and to identify the engines of cultural change through time. Each protagonist, however, was a product of his distinct, nationally influenced disciplinary tradition with its own values and scholarly practices. Bordes, steeped in the French archaeological tradition born in

⁵ F. Harrold, "Transatlantic Prehistory: Thoughts on the Encounter Between American and European Paleolithic Prehistory." in *The Role of American Archeologists in the Study of the European Upper Paleolithic*, ed. Lawrence G. Straus (Oxford, England: Archaeopress, 2002), 28.

the natural sciences, valued and respected attention to the craft of the discipline, that is, methodology and practice. Binford, in contrast, strongly identified archaeology in the American tradition as a behavioral science, emphasizing the role of theory and process in understanding the past.

The debate has its origins in a publication by François Bordes in 1953 titled *Essai de classification des industries "moustériennes"*, in which he proposed a typology for the different tool assemblages frequently associated with Neanderthals of Western Europe. These tool assemblages, known as the Mousterian after their initial discovery at the site of Le Moustier in southwestern France, were highly variable both between and within archaeological sites. An understanding of that variability was of great interest to Paleolithic researchers in the first half of the twentieth century, and Bordes' typology was a revolution in the description and classification of Mousterian tools, offering a seemingly objective and systematic framework for analysis. Bordes continued to refine his typology over the next decade, eventually offering an interpretation of the variability as the reflection of the activities of four distinct cultural groups or traditions, or in his word 'civilisations.'

By the early 1960s Bordes' typology was well known and widely accepted as was his identification of Mousterian cultural groups as the root cause of the interassemblage variability. Then, in 1966, Lewis Binford published an article with his then wife Sally, in which he proposed a complete interpretive revisioning of Bordes' typology. In *A Preliminary Analysis of Functional Variability in the Mousterian of Levallois Facies*, the Binfords rejected Bordes' interpretation of distinct Mousterian cultures as the basis for the variability, arguing instead that the variability was a reflection not of *cultural* differences but rather one of *functional* differences. In and of itself, this challenge of an accepted interpretation in archaeology was not unusual; the archaeological literature is littered with numerous examples of rebuttals and disputes over

methods and interpretations.⁶ The context of the Binfords' challenge of Bordes, however, was anything but typical. In a very deliberate fashion, Lewis Binford had strategically selected Bordes' interpretation of Mousterian stone tool variability as a test case for his new ideas about archaeology.⁷ In earlier publications Binford had assertively proposed that if there was any hope that the field of archaeology would fulfill its primary purpose, archaeologists would need to shed their focus on descriptive culture histories and turn instead to the application of testable hypotheses derived from a theoretical position in which culture is conceptualized as an external adaptive mechanism.⁸ Binford was convinced that archaeology could in fact *explain* the past and not merely *describe* it.

Binford's theoretical approach represented a significant departure in the discipline's approach to the study of the deep past and was part of his larger critique of archaeology and advocacy of a new approach that he dubbed the New Archaeology. His challenge of Bordes was thus part of a deliberate and systematic effort to showcase how the traditional culture-historical approach to the past limited the scope of the discipline, relegating it needlessly to mere description of the past and preventing advancement of research into questions of the how and the why of the past. In this context, any treatment or analysis of the Bordes-Binford debate necessitates study of the broader trends in the history of archaeology contemporary with the times.

Thus one of the primary goals of this research is to contextualize the Bordes-Binford debate in the broader history of archaeology and related social scientific disciplines from the latter nineteenth through the mid-twentieth centuries. This serves to advance understanding of the intellectual traditions that characterize the study of the deep past with specific emphasis on

⁶ The Spaulding-Ford debate over the nature of artifact 'types' in American archaeology and the Battle for the Aurignacian in French Paleolithic archaeology are two excellent examples.

⁷ Although Sally Binford was instrumental to Lewis Binford's early challenge of Bordes and later conducted significant work in her own right on the Upper Paleolithic, this research will focus on Lewis' interaction with Bordes and his work on Mousterian variability.

⁸ L. Binford and J. Sabloff, "Paradigms, systematics, and archaeology," *Journal of Anthropological Research* 38 (1982): 137-152.

the nature and extent of the oft-invoked paradigmatic differences between American and French archaeologists over the past fifty years.⁹ As will be demonstrated, the Bordes-Binford debate is best understood as an emblem of a mid-century intellectual narrative constructed around stereotypical notions of a young, progressive, empirical, new (i.e., uniquely American) approach to understanding the deep past and a limited, outdated and, perhaps more importantly, descriptive approach to prehistory typical of Old World (particularly French) archaeologists. Repercussions of this mid-twentieth century intellectual split continue to be felt well into the twenty-first century and to shape research questions and color interpretations on both sides of the Atlantic.

To accomplish the aims of this research, intellectual trends and prevailing scholarly thought relative to the study of the deep past in the few decades immediately before and after the Bordes-Binford debate are considered with a specific focus on how these trends were influenced by the unique national contexts from which they arose. Further, the convergence and divergence of these intellectual trends and their reflection of the paradigmatic or theoretical currency of their time are specifically addressed. And, finally, the research will explore how the narratives themselves used by scholars in the discipline to give meaning to the Bordes-Binford debate were shaped and influenced by the debate. This research not only highlights how Bordes and Binford dramatized the narrative to construct and advance their respective arguments, but focuses on how subsequent generations of scholars have selected particular narrative strategies to describe and make meaning of the debate in later contexts, that is, how archaeologists either represent the debate as a critical intellectual turning point in the development of the discipline or deemphasize/underplay the debate's importance.

⁹ See Binford and Sabloff, "Paradigms, Systematics, and Archaeology"; G. Clark and J. Lindly, "On Paradigmatic Biases and Paleolithic Research Traditions," *Current Anthropology* 32 (1991): 577-587; L. Straus, "Paradigm lost: a personal view of the current state of Upper Paleolithic Research," *Helinium* 27 (1987): 157-171; and F. Harrold, "The Elephant and the Blind Men," in *Perspectives on the Past*, ed. G.A. Clark (Philadelphia: University of Pennsylvania Press, 1991).

The research is organized as follows. Chapter One examines fundamental intellectual trends and shifts among French Paleolithic archaeologists in the decades prior and subsequent to World War II. In Chapter Two, the development of the so-called 'straight' archaeology that has come to be associated with French and other European, particularly continental, archaeologists, is explored within the context of the development and affiliation of Paleolithic archaeology within the natural sciences.¹⁰ In this chapter, François Bordes and his work are situated within the broader history of Paleolithic archaeology as practiced in France during the early to mid-twentieth century.

Chapter Three concerns the primary intellectual trends and shifts among American archaeologists in the same period prior and subsequent to World War II. Binford's ideas about how to study the past are addressed within the American anthropological tradition. This approach is necessary to contextualize Binford's challenge to Bordes' ideas. There is particular emphasis in this chapter on the development of American archaeology within an anthropological, or social science, tradition and the impact of that affiliation on theory and method specifically within Paleolithic archaeology. In addition, this chapter examines the social history of archaeology in America during the mid-twentieth century. This chapter also discusses the extent to which larger social issues -- the wave of national optimism fueled by faith in technology, science and American cultural hegemony -- may have contributed to how American archaeologists approached the study of the deep past and their perceptions of Europe and their European colleagues.

Chapter Four explores directly the Bordes-Binford debate, its meaning and context, with particular concern for how Binford's arguments for function over style as an interpretive device to understanding the material culture of the European Paleolithic were not merely more reflective of the new methodology than that practiced by Bordes. This chapter also posits that the debate also represented an approach to interpreting the deep past based on an

¹⁰ 'Straight' archaeology is a term coined by American archaeologist James Sackett to describe French archaeology.

anthropological perspective. Further, this chapter suggests that the Bordes-Binford debate is best seen as an inevitable outgrowth of the American and French approaches to archaeology in the mid-twentieth century as addressed in Chapters Two and Three. This chapter examines the reaction to the debate in scholarly circles on each side of the Atlantic and explores its immediate impact (or lack thereof) on the practice of Paleolithic archaeology.

Chapter Five addresses the long-term impact of the Bordes-Binford debate on the practice of Paleolithic archaeology. More specifically, this chapter examines the efficacy of the notion of representing the Bordes-Binford debate as the result of incommensurable paradigmatic differences. Finally, Chapter Six concludes with a discussion of the how the narrative surrounding the Bordes-Binford debate has evolved over the past forty years. This chapter explores the underlying subtexts of this narrative and how it developed and continues to be communicated to the next generation of archaeologists.

1.1 The Literature

A brief synopsis of the Bordes-Binford debate is standard in introductory archaeology texts. Most American undergraduates in anthropology and/or archaeology are expected to have at least a passing familiarity with the question of 'style versus function' in the interpretation of material culture. And yet surprisingly little attention in the scholarly record has addressed the intellectual skirmish beyond a superficial level. Much of the extant literature is limited to the details of the debate itself, that is, the meaning of the variability in Mousterian stone tools or on the broader theme of the history of archaeology. Additionally, there is a growing body of literature on the role of theory and methodology in archaeology. These texts, however, are aimed more at the practitioner in the field and address the research questions pertinent to this dissertation only to the extent that they provide a means to understand how recent and current archaeologists were influenced by the debate and the surrounding intellectual currents of the time.

The literature for this dissertation does not rely on primary texts as conventionally defined. Of greater interest and relevance are the secondary and tertiary literatures, that is, those works produced by individuals who wrote about the debate directly or indirectly or about the intellectual trends and currents preceding and subsequent to it. The literature addressing this topic is complex and varied and spans more than a hundred and fifty years. Selectivity in the use of sources has been inevitable and the interested reader will likely disagree with some of these selections. Many of the sources are, of course, in French, although it is recognized that most readers of the present study will be English speakers only. With that in mind, every effort has been made to use translations where possible and to avoid quotations in French.

1.1.1 The Primary Literature

Conventionally defined, the primary literature of the debate is composed of the publications of Bordes and Binford. Both were extremely prolific writers, each having many dozens of publications. This literature begins in 1950 when Bordes first outlined his approach to construction of a typology of Mousterian tools in his article “Principles d’une méthode d’étude des techniques de débitage et de la typologie du Paléolithique ancien et moyen.” But in this original publication he had not yet fleshed out his construct of what he later termed the four ‘facies’ of the Mousterian. It was not until he published “Essai de classification des industries ‘moustériennes’” in 1953 that Bordes began to refine his typology and offer the first hints of his interpretation of Mousterian variability. Bordes’ work at this time was largely unknown outside France. Awareness of his research among American archaeologists came via Hallam Movius, an American archaeologist active in French Paleolithic archaeology in the years just after the Second World War.¹¹ Movius offered the anglophone world the first detailed description of Bordes’ pioneering work in 1952 in his contribution, “Old World Prehistory: Paleolithic,” to the International Symposium on Anthropology held in New York of that year. The papers from that symposium were subsequently edited and published by Alfred Kroeber, a widely respected and

¹¹ H. Movius, “Old World Prehistory,” in *Anthropology Today; an Encyclopedic Inventory*, ed. Alfred Louis Kroeber (Chicago: Univ. of Chicago Press), 1953.

influential American archaeologist, in 1953. Binford noted later that the Movius paper in Kroeber's edited volume was of seminal importance to New World archaeologists at the time, and it excited a new appreciation in him and others for study of the Old World.¹²

Bordes in the meantime continued to refine his typology. He eventually offered his definitive interpretation of Mousterian variability as stemming from different cultural traditions to those outside the inner circle of French Paleolithic research in a 1961 article in *Science* titled, "Mousterian Cultures in France." With the publication of the article in *Science*, Bordes solidified his reputation in Europe and America as the leading authority of his time on the French Paleolithic. It was in that same year (1961) that Binford began his career in earnest, publishing his first paper. Less than a year later, in 1962, Binford established himself as a rising star in archaeology with the publication in the journal *American Antiquity* of "Archaeology as Anthropology." This paper was a passionate critique of archaeology; it was Binford's first attempt to outline his ideas of a New Archaeology. Binford's challenge of Bordes (at least in print) did not come until 1966 with the publication of "A Preliminary Analysis of Functional Variability in the Mousterian of Levallois Facies" with Sally Binford. As we will examine in subsequent chapters, Binford's rejection of Bordes' interpretations was part of a larger critique of the state of the discipline and was quite sensitive, given the fact the two were personally as well as professionally friendly.

Bordes did not offer direct reference to Binford's disagreement with his work for several years, although he makes vague references to it in his 1968 publication, *The Old Stone Age*, in which he remarks that "not everything set down here will have the approval of our colleagues."¹³ He goes on to allude in that publication to a 'variety of schools in the field of prehistory,' each associated with a different country and perspective, noting that American archaeologists "with primitive peoples still living among them" tended to focus on ecological and functional rather

¹² Sabloff, *Conversations with Lew Binford*, 28-29.

¹³ Bordes, *The Old Stone Age*, 7.

than cultural explanations.¹⁴ Even more oblique were Bordes' references in *The Old Stone Age* to 'certain anthropologists' that were 'reluctant' to accept his ideas. One can only assume that these were references to Binford. Binford continued to press for functional interpretations in his 1968 article "Archaeological Perspectives," part of a volume he edited with Sally Binford titled *New Perspectives in Archaeology*. It was not until four years after Binford's original challenge in 1966 that Bordes offered a direct print rebuttal to Binford. In a 1970 publication with his wife Denise de Sonneville-Bordes titled "The Significance of Variability in Paleolithic Assemblages" in *World Archaeology*, Bordes laid out in great detail his defense of the cultural explanation for Mousterian variability. The fact that he published his rebuttal in English communicated his intention to meet Binford's challenge head on and on Binford's own turf. Over the next two decades, Bordes and Binford continued to offer versions of their interpretations in a variety of publications with Binford continuing to press his case well after Bordes' death in 1981.¹⁵

In the years following Binford's original critique of Bordes, there were numerous discussions in print on the nature of the debate with many of the most notable Paleolithic researchers weighing in. In the period immediately following Binford's first challenge to Bordes' 'cultural' interpretation, there was a clear division between those siding with Binford and those with Bordes. Several of the so-called New archaeologists, primarily American, began publishing functional studies of stone tool variability akin to Binford's in a variety of other archaeological contexts. Leslie Freeman in 1966 offered a functional explanation of the Mousterian in northern Spain.¹⁶ Sally Binford offered a functional analysis of the Near Eastern Mousterian in 1968.¹⁷ Even further afield, but influenced by Binford, was G. Frison's functional analysis of stone tools

¹⁴ Ibid.

¹⁵ See F. Bordes 1981, 1978, 1974, 1973, and 1972 and L. Binford 1989, 1983, 1973, and 1972.

¹⁶ L. Freeman, "The nature of Mousterian facies in Cantabrian Spain," *American Anthropologist* 68, no.2 (1966): 230-237.

¹⁷ S.R. Binford, "Early Upper Pleistocene Adaptations in the Levant," *American Anthropologist* 70, no. 4 (1968): 707-717.

at a Late Prehistoric site in northern Wyoming and E. Wilmsen's analysis of Paleolndian stone tool assemblages from a variety of sites in North America.¹⁸ Paul Mellars, a close colleague of Bordes, on the other hand, in 1970 published a wholesale critique of the notion of functional variability and analysis as applied to the Paleolithic, noting specifically the limitations of 'testability' within the archaeological record.¹⁹ This was a direct rebuttal not just of Binford's interpretation of Mousterian variability but also of the explicitly scientific approach endorsed by Binford's New Archaeology.

1.1.2 *The Secondary and Tertiary Literature*

The remaining literature pertinent to this research may be categorized in three different but overlapping categories. First, there are the broad comprehensive histories of archaeology. Second, there is the literature that focuses on the development and history of Paleolithic archaeology generally, and that of French Paleolithic archaeology specifically. And, third, there is the literature dealing with the history and development of American archaeology with a subset devoted to the rise of the New Archaeology. It was necessary to review all three categories to understand the historical contexts of the Bordes-Binford debate and, for the later publications, to analyze their treatment of the debate in order to determine how archaeologists have interpreted the debate from their own perspectives.

For a relatively young discipline (dating formally only to the mid-nineteenth century by most estimations), archaeology has a deep reserve of material that explicitly addresses the history and development of the field. As early as the 1920s and 1930s, archaeologists had begun to document the development of their own field, mostly in the form of autobiographies and biographies of early luminaries, along with a few early attempts to chart shifting views and

¹⁸ See G. Frison, "A Functional Analysis of Certain Chipped Stone Tools," *American Antiquity* 33, no. 2 (1968): 149-155 and E. Wilmsen, "Functional Analysis of Flaked Stone Artifacts," *American Antiquity* 33, no. 2 (1968): 156-161.

¹⁹ P. Mellars, "Some Comments on the Notion of 'Functional Variability' in Stone-Tool Assemblages," *World Archaeology* 2, no. 1 (1970): 74-89.

approaches to the study of prehistory.²⁰ By the 1950s, nationalistic works that outlined the development of archaeology in certain countries or regions were quite popular, some of the most notable being Colin Simard's *Découverte Archéologique de la France* (1955), Sir Thomas Kendrick's *British Antiquity* (1950), and J.H. Egger's *Einführung in die Vorgeschichte* (1959). Also in the 1950s, archaeologists began to actively develop comprehensive histories of the discipline.

One of the first self-conscious attempts to systematically address how the field developed in a variety of periods and national contexts was that of Glyn Daniel in his book *A Hundred Years of Archaeology* (1950). Daniel's history of archaeology represented something new at the time of its publication, an assertion that archaeology, specifically prehistoric archaeology, was a bona fide academic discipline worthy of attempts to chart its historical development. His *A Hundred Years of Archaeology* is a useful and informative account of the development of archaeology out of a gentlemanly interest in antiquarianism to a respected academic pursuit by the mid-twentieth century. It was Daniel who first traced the influence of geology and paleontology on interpretive traditions in European Paleolithic research, an association that was to have lasting and profound effect on the discipline in France. Daniel's survey of the history of archaeology has become a classic, its success spawning a revised edition in 1975 and an international conference on the history of archaeology in Aarhus, Denmark in 1978. Daniel himself became the first widely accepted historian of archaeology, publishing numerous articles and books on the subject until his death in 1986.²¹

The first edition of Daniel's history of archaeology is important to this author's research as it provides an excellent framework for understanding the development of the field in its infancy and how a practitioner in the mid-twentieth century perceived the evolution of methodology and theory up to that point. In his second edition, *A Hundred and Fifty Years of*

²⁰ See S. Piggott 1935, 1950 and 1958 and V. Childe 1935.

²¹ G. Daniel, *A Hundred Years of Archaeology* (London: Duckworth, 1950).

Archaeology (1975), Daniel augments his view of the historical development of the discipline since 1950, which includes the pivotal years encompassing the rise of the New Archaeology, which he discusses at length, and the Bordes-Binford debate, which he never directly references. In this work, Daniel provides some interesting and contemporary perspectives on the rise of the New Archaeology in America, a movement of which he was highly critical. Daniel believed the New Archaeology to be more about posturing than the pursuit of science and was particularly disturbed by the opacity of language utilized by the movement's protagonists, which he described as unnecessarily full of jargon and deliberately polemical – although it should be noted he was not immune from this himself. Regarding the New Archaeology, Daniel observed:

This new movement in America stems, of course, from the bareness of the pre-Columbian record of archaeology: for centuries nothing happened of general interest to the student of world history – no Stonehenge, no Maltese temples. American archaeologists, dismayed by their archaeological record, have sought refuge in theory and methodology and spend their time talking about “the elucidation of cultural process” and the production of “laws of cultural dynamics”. There is much to praise in this 1960 American movement which stems back to Taylor's *A Study of Archaeology*, a book that was, in the previous chapter, recognised as of such importance: the application of statistics, environmental archaeology, geographical patterns. This new movement of the 1960s needs to be absorbed into standard thought and work: at the present it is, especially for non-American workers, bedevilled by jargon and by people who, apparently unable to speak and write in clear English, use such phrases as “the logico-deductive-evolutionary systems paradigm.”²²

The current standard for comprehensive histories of archaeology is Bruce Trigger's *A History of Archaeological Thought*, first published in 1989 and later revised in 2006. The scope of Trigger's work was staggering, chronicling the development of thought on the human past from its humble medieval beginnings right through the theory ‘wars’ of the latter twentieth century. Trigger's ambitious work was not limited by geographical or chronological parameters; indeed, his study explicitly sought to contextualize the worldwide history of the discipline from its earliest beginnings.

²² G. Daniel, *A Hundred and Fifty Years of Archaeology*, (Cambridge, Mass: Harvard University Press, 1976), 371-372.

Trigger devoted a considerable amount of space to the rise of the New Archaeology in the 1960s in his volume, which makes it directly applicable to this work. Like other historians of the discipline, he attributed to the proponents of the New Archaeology (Binford being merely one prominent member) a considerable amount of planning in their efforts to remake the discipline. Describing the New Archaeology of the early 1960s as an 'all-out attack' on the archaeological establishment, which was portrayed as lacking in scientific rigor, Trigger reinforced the notion that the intellectual divide was marked by a bright line between older established professionals in the discipline and up-and-coming scholars eager to portray themselves as exacting practitioners of science, who wielded powerful explanatory tools extracted from a coherent body of theory.²³ He did acknowledge, however, that much of the new and improved rhetoric of the New Archaeology was promulgated by a fairly small coterie of archaeologists associated with Binford. Interestingly, Trigger only briefly addressed the Bordes-Binford debate in one paragraph out of some 600 pages of text; his limited consideration is/was curious given his interpretation that the Bordes-Binford debate epitomized the break between Old and New Archaeology. He said nothing about the transatlantic context of the debate or its impact on the practice of Paleolithic archaeology in Europe then and now.

A subset of these broader histories of archaeology is the growing number of works that address the philosophy of archaeology. The interest in the role and use of philosophy in archaeology dates back to the 1950s and perhaps earlier. Binford himself noted that his mentor, Leslie White, a renowned anthropologist at the University of Michigan, explicitly encouraged him to take up study of the philosophy of science.²⁴ Attempts to illustrate and clarify the philosophy of archaeology began in earnest in the 1970s, particularly among American archaeologists influenced by the New Archaeology. One of the most enduring of these was the work *Explanation in Archaeology* (1971) by Patty Watson, Steven Le Blanc, and

²³ B. Trigger, *A History of Archaeological Thought*, 2nd ed. (New York: Cambridge University Press, 2006), 386.

²⁴ L. Binford, *An Archaeological Perspective* (New York: Academic Press, 1972), 7-8.

Charles Redman, in which they defined the philosophy, heavily influenced by Carl Hempel's work in the 1960s, underlying the New Archaeology.²⁵ More recently, attention has shifted towards efforts to synthesize the role of philosophy in archaeology with an understanding of its shifting influences over time. In *Philosophy and Archaeology* (1982), Merrilee Salmon deliberately avoided offering a 'history of archaeological theorizing,' but rather documented the efforts of the New Archaeologists to incorporate the philosophy of science into the intellectual framework of archaeology.²⁶

The most comprehensive synthesis to date on the philosophy of archaeology is Alison Wylie's *Thinking from Things*.²⁷ Wylie, like Salmon, initially was interested in the philosophical influences of the New Archaeology, but she parted ways from Salmon by bringing a deeper understanding of the historical influences on the philosophy of archaeology. Wylie offered an authoritative explication of the rise and fall of New Archaeology supported by an extensive bibliography rooted broadly in the philosophy of science. Her command of the archaeological literature is robust, and she does address the Bordes-Binford debate in some detail; however, she, like the others, did not specifically acknowledge the transatlantic context of the debate or the influence that it might have had on the substance of the disagreement.

In addition to the broad histories of archaeology and syntheses of the philosophy of archaeology, another subset of the secondary literature is that of histories of the development of French Paleolithic archaeology. For purposes of this research, the literature on French Paleolithic research spanned much of the twentieth century and some of the twenty-first. One of the first efforts to document the story of Paleolithic discoveries in France was offered by the Abbé Henri Breuil. His original history, *Découvertes Paléolithiques en France* (1934), was

²⁵ P. Watson, et al. *Explanation in Archeology; An Explicitly Scientific Approach* (New York: Columbia University Press, 1971).

²⁶ M. Salmon, *Philosophy and Archaeology*. (New York: Academic Press, 1982), 2-7.

²⁷ A. Wylie, *Thinking from Things: Essays in the Philosophy of Archaeology*. (Berkeley, CA: University of California Press, 2002).

subsequently given as the Huxley Memorial Lecture in 1946. In it, Breuil outlined the major discoveries in France of Paleolithic sites and the development of thought on the chronology of the Paleolithic with specific interest in the Périgord region. Breuil's history, while informed by first-hand knowledge of the sites and some of the major players involved in the discoveries, tended to focus on the implication of discoveries for establishing the antiquity of man; he was less concerned with the development of thought in any sort of archaeological research tradition. A similar work was offered by Jean-Claude Colin-Simard in 1955.²⁸

One of the more influential histories in this vein was Annette Laming-Empeaire's authoritative and rich account of the formative years of Paleolithic archaeology in France in her *Origines de l'Archéologie Préhistorique*. Laming-Empeaire charted the emergence of the body of thought and works that by the mid-nineteenth century culminated in a distinct and self-conscious discipline of Paleolithic archaeology. She was one of the first to document the profound effect that entrenched notions of unilineal evolution had on interpretation of the Paleolithic in France, initially through the work of Boucher de Perthes, one of the first to offer evidence of the great antiquity of man, and later through the work of its main proponent, Gabriel de Mortillet, a later nemesis of the Abbé Breuil. She also documented how unilineal evolutionary interpretations of the Paleolithic began to lose favor by the turn of the twentieth century in the wake of the rise of a culture-historical approach in archaeology. Laming-Empeaire also teased out the relationship between paleontology and archaeology in these early years. She noted how Mortillet in particular adopted paleontological conventions in nomenclature and methodology and applied them extensively to the archaeological record.²⁹

In 1978, James Sackett, with his publication of "From Mortillet to Bordes: a century of French Paleolithic research," picked up where Laming-Empeaire left off. His account traced the impact and later rejection by Breuil of Mortillet's unilineal evolutionary archaeology on

²⁸ J.-C. Colin-Simard, *Découverte archéologique de la France* (Paris: Amiot-Dumont, 1955).

²⁹ A. Laming-Empeaire, *Origines de l'archéologie préhistorique en France, des superstitions médiévales à la découverte de l'homme fossile*. (Paris: A. et J. Picard, 1964).

French Paleolithic archaeology. In this work, Sackett also outlined the persistence of the use of paleontological interpretive models in the Mortilletian tradition in French Paleolithic research even though the evolutionary framework of those models was discarded. Sackett authoritatively outlined the emergence and development of the traditional 'straight' archaeology, which has become the quintessential, albeit stereotypical, view of French archaeology.³⁰

In the two decades between the late 1970's and 1990's there was a significant increase in the literature on the history and development of French Paleolithic archaeology. Some of that work was in the form of comprehensive histories such as that of Bruce Trigger, but the majority was composed of the retrospectives, musings, and contemplations of a generation of American and Canadian archaeologists specializing in the French and European Paleolithic. Individuals who had been mentored by those actively involved in the development of the New Archaeology, included Geof Clark, Harold Dibble, Lawrence Straus, James Sackett, Nicolas Rolland, Randall White, Olga Soffer, and Frances Harrold, to name a few. These scholars, many of whom had active research agendas in France or in the neighboring region of northwestern Spain, sought to make sense of their working relationships with their French and other continental European, counterparts in the context of the New Archaeology, the aftermath of the Bordes-Binford debate, and the wake of a renaissance of transatlantic collaborations in Paleolithic archaeology. Members of this group conceived the idea of 'paradigmatic' differences to characterize the distinction between how American and French (as well as other western European) archaeologists approach the interpretation of the Paleolithic archaeological record.

Geoffrey Clark has been the most vocal among this generation of American Paleolithic archaeologists in advancing the notion of a French-dominated historical approach to the Paleolithic at odds with the anthropological approach favored primarily in America, but also in Canada and Britain. In his widely cited 1991 article, "Paradigmatic biases and Paleolithic research traditions" in *Current Anthropology*, Clark, along with co-author John M. Lindly, argued

³⁰ J. Sackett, "From de Mortillet to Bordes: a century of French Paleolithic research," in *Towards a History of Archaeology*, ed. G. Daniel (London: Thames and Hudson, 1981).

strongly that there were fundamental and incompatible transatlantic differences in research traditions stemming from the continued use of a 'natural science paradigm' in Europe and that of a 'social science paradigm' in America. Until those differences were acknowledged, Clark concluded that the field of archeology would see little in the way of increased understanding of the deep past.³¹

From the French perspective, there has been less concern in the literature with the topic of differences in research traditions between France and America in interpreting the Paleolithic. Most French Paleolithic archaeologists never address or acknowledge the differences, while a few give a nod to American influence in the form of methodology or increased resolution of proffered explanations of the past, but remain unconvinced or unconcerned with putative interpretative differences.³² A notable exception to this dearth in the French literature was the essay by Laurent Olivier and Anick Coudart, "French tradition and the central place of history in the human sciences". Olivier and Coudart largely agreed with Clark and other American archaeologists that French interpretive traditions in Paleolithic archaeology were (and are) heavily influenced by the central place of history (as opposed to anthropology) in the development of ideas about the past. They claimed that what they termed Anglo-American archaeological 'theories' never took hold in French archaeological debates. Olivier and Coudart also maintained that France and Western Europe with their long history of geographical, cultural, and historical conflict, promoted among Paleolithic researchers an appreciation that geography, culture, politics and history are at the core of every past event, a sentiment heavily influenced by the work of scholars that came to be known as founders of the *Annales* School

³¹ G. Clark and J. Lindly, "On Paradigmatic Biases and Paleolithic Research Traditions," *Current Anthropology* 32 (1991): 577-578.

³² M. Otte, "Le rôle des américains dans la recherche du Paléolithique en Europe," in *The Role of American Archeologists in the Study of the European Upper Paleolithic*, ed. Lawrence G. Straus (Oxford, England: Archaeopress, 2002) , 39-42.

such as Lucien Febvre, Marc Bloch, and Fernand Braudel.³³ As addressed later in Chapter Five, this sentiment has implications for the development of French Paleolithic archaeology in the aftermath of the Bordes-Binford debate.

The literature on the history and development of Paleolithic archaeology, particularly the American, continued to reflect a concern with the larger issues of differing interpretive traditions. In 2001, a symposium was held in Liège, Belgium, to address the role and influence of American archaeologists on European Paleolithic archaeology. The papers, edited and published by Lawrence Straus in 2002, did not address the Bordes-Binford debate per se, but did explore from a variety of European and American perspectives the extent and influence of American archaeologists on the practice of Paleolithic archaeology in Europe and, more importantly, perhaps, the interpretation of the European Upper Paleolithic archaeological record. The topics ranged from chronologies of American involvement in collaborative excavations in Europe to further thoughts on perceived paradigmatic biases in American and French archaeology as proposed earlier by Clark.

Of critical importance for this author's research, these essays provide an important insight into the narrative used by practitioners in the discipline to describe and structure their interactions. By virtue of their unique approaches to the topic of the American presence in the study of the European Paleolithic, these essays also represent the complex nature of intellectual exchange across nationalist traditions. Such studies serve as a reminder that overly simplistic explanations of a bright line dividing American and European archaeologists, while seductive, are not ultimately meaningful. Indeed, a few authors in this collection (e.g., Harrold and Bicho) see many instances of intellectual exchange between American archaeologists and their European peers, including a recognition of the importance of careful stratigraphic observation in excavation (passed from France to America in the early twentieth century).

³³ L. Olivier and A. Coudart, "French Tradition and the Central Place of History in the Human Sciences: Preamble to a dialogue between Robinson Crusoe and his Man Friday," in *Theory in Archaeology: A world perspective*, ed. Peter Ucko (London: Routledge, 1995).

Moreover, they see how the integration of physical anthropology and archaeology (paleoanthropology) has furthered our understanding of human adaptations during the Paleolithic (passed from America to France in the 1950s and 1960s), while recognizing that the more abstract research questions tend to provoke more predictable reactions related to nationalist intellectual traditions. Others, however, (e.g., Clark and Kozlowski) see fundamental and innate differences in national research traditions that permeate not only their divergent interpretations of the past, but also the very questions that are accepted as viable avenues of research.

Another subset of the secondary literature with implications for this author's research is histories regarding the development of archaeology in the United States. Unlike the situation in France, archaeology in America with its holistic, four-field approach is not compartmentalized by time period, making it difficult to separate the history of Paleolithic archaeology as practiced in America from the general development and history of the field. Thus, in order to address the development of Paleolithic research in an American tradition one must also tackle the history of American archaeology in total. As previously discussed, the histories of archaeology offered by Daniel and later by Trigger did include significant discussion of the development of archaeology in America; that, however, was not the main purpose of those works. It is to those works that specifically address the history and development of archaeology as a discipline in America that we now shift attention.

Although there were likely numerous unpublished attempts to document the history of American archaeology and certainly there were several short published essays on the topic it was not until Walter Taylor's *A Study of Archaeology* (1948) that any professional archaeologist attempted to provide a comprehensive approach to the development of the discipline in America.³⁴ Although primarily a critique of the current state of American archaeology in the 1940's, Taylor offered the first large-scale view of American archaeology from a historical

³⁴ Nels Nelson's *The Antiquity of Man in America in light of Archaeology* in 1933 is an early attempt at a history of archaeology that predates Taylor.

perspective. Of greater importance for this research, Taylor's work was also an early salvo in the movement that would culminate in what was later termed the New Archaeology. In his work, Taylor delineated the early influence of the unilineal evolutionary approach on American archaeology, which by the late nineteenth and early twentieth century had fallen completely out of favor, replaced by the culture-historical approach. Taylor was the first to identify and promote the unique relationship between American archaeology and the social sciences, particularly anthropology. Taylor decried that relationship, however, to the extent that he believed that archaeology had been unduly influenced by the notions of historical particularism advanced by anthropologist Franz Boas in the early twentieth century. Taylor hoped to advance what he termed a "conjunctive approach," whereby archaeology would move beyond its concern with descriptive culture-histories and incorporate those aspects of anthropology that would allow a more holistic approach to the past. He specifically identified with the emerging theoretic schools within anthropology such as functionalism in the tradition of Bronislaw Malinowski and structuralism as espoused by A.R. Radcliffe-Brown and Claude Lévi-Strauss. Taylor's view in retrospect seems prophetic given the strong and enduring influence of anthropology on American archaeology in the twenty-first century.³⁵

The current standard among histories of American archaeology is Gordon Willey and Jeremy Sabloff's *A History of American Archaeology*, first published in 1974 with subsequent editions in 1980 and 1993. Heavily influenced by Taylor, Willey and Sabloff were also prompted in their work by the intense self-appraisal among American archaeologists that resulted from the emergence of the New Archaeology. Even more so than Taylor, Willey and Sabloff were entering uncharted territory with the publication of *A History of American Archaeology*. In 1974 there was no comprehensive work that addressed the development of the discipline in America chronologically. Willey and Sabloff went beyond mere chronology, however, instead opting to organize various stages of American archaeology within both a chronological and intellectual

³⁵ W. Taylor, *A Study of Archeology* (Menasha, WI, American Anthropological Association Memoir 69, 1948).

framework. Willey and Sabloff tracked the development of thought within the field from its early speculative period through what they termed the classificatory-descriptive period, which they argued was heavily influenced early on by the beginnings of professional archaeology in Europe and then later by its divergence from European trends and its alliance with anthropology, through the modern period, which was the first real attempt to put the New Archaeology, then still quite current, into historical perspective.

With their great command of the literature and first hand knowledge of the turbulent period of the 1960s, Willey and Sabloff's research is quite relevant to this author's work. The authors themselves are interesting reflections of two distinct generations of archaeologists. Willey began his career in the 1930s and was a front line spectator and participant in the peak years of the discipline when culture-history reigned supreme through the early days of the New Archaeology. Sabloff, on the other hand, came of age professionally in the midst of the 1960s and was heavily influenced by the emerging concern with theory and method. Presenting as they did a work on the history of American archaeology, Willey and Sabloff had nothing to say of the Bordes-Binford debate specifically, but their authoritative account of the years just prior and subsequent to the emergence of the New Archaeology offered a relatively balanced perspective on Binford and his influences, a topic we will revisit in Chapter Three.

There have been few attempts to match the comprehensive work of Willey and Sabloff in documenting the history of American archaeology, but a few additional works provide information of importance to this research. Primarily comprised of efforts to focus on key aspects of the history of American archaeology after 1960 or to situate the rise of the New Archaeology in social and political contexts, these works are relevant not so much for their contribution to the body of knowledge about the history of archaeology in America, but rather for their reflection of how the rise of the New Archaeology (and indirectly the Bordes-Binford debate) was represented in the years subsequent--a topic addressed in Chapter Six. These works offer time capsules of the deliberately chosen narrative strategies that were employed to

inform and instruct students of archaeology as to the nature and context of the New Archaeology, its influence and persistence in the field.

Of particular utility in this regard is a collection of essays that were compiled for the fiftieth anniversary of the Society for American Archaeology in 1985 and edited and published in 1986 by David Meltzer, Don Fowler, and Jeremy Sabloff under the title *American Archaeology: Past and Future*. These collected essays were written by a virtual Who's Who in mid-1980s American archaeology such as Robert Dunnell, Patty Jo Watson, George Cowgill, Mark Leone, and even Lewis Binford, including topics ranging from general overviews of the history of American archaeology, discussions of current advances in methodology, and contemplations on the future of the discipline. Most of the authors of these collected essays consider the implications that the New Archaeology had for the practice of archaeology and the extent to which its influence will persist in the future.

In the mid-1990s, two publications were released that were typical of a new emphasis in the history of archaeology, that is, an effort to link the development of archaeology in America explicitly to larger sociological and ideological trends in American history. Thomas Patterson in *Toward a Social History of Archaeology in the United States* (1995) and Alice Kehoe in *The Land of Prehistory* (1998) provided explicitly Marxist analyses of the historical processes that have shaped the discipline of archaeology in the U.S. over the past century, concentrating on describing the social and political influences on the theories and methods that rose and fell in popularity. Despite their differing styles (Kehoe is far more polemical), both attempted to situate the changing intellectual landscape of archaeology in the mid-twentieth century in its unique socio-cultural context. From these works, it is possible to obtain a clearer picture of the many influences acting on Binford and other New archaeologists of his generation. Patterson and Kehoe both focus on the New Archaeology as a uniquely American phenomenon, in some ways driven by an American belief in the power of science and technology to achieve progress.

When this research was initiated, one of the most glaring oversights of many of the recent publications addressing the history of archaeology or the rise of the New Archaeology was the lack of attention devoted to the Bordes-Binford debate. There was and continues to be little interest on the part of general historians of American archaeology in the debate, its implications or its meaning in the context of the larger arguments within the discipline at that time. Also, there is evidence of little interest in teasing out the significance, or lack thereof, of the fact that one of Binford's most vocal opponents was a French archaeologist, operating not only from professional disagreement, but from a decidedly non-American perspective, without the intellectual baggage one might expect of an American archaeologist at the time. It is also the case that the literature in France, too, is largely bereft of efforts to address the implications and meaning of the Bordes-Binford debate. The next few chapters of this dissertation will attempt to close that gap.

Certainly the Bordes-Binford debate was not a singular event in the intellectual history of Paleolithic archaeology, and to view it as such would be simplistic. Rather, the Bordes-Binford debate is reflective of latent tensions in the discipline and in the larger socio-cultural landscape in which Bordes and Binford operated. Thus the primary research question to be explored in this dissertation is whether these divergent intellectual trends discernible in the Bordes-Binford debate reflected fundamental paradigmatic differences, irrespective of the national origin of the protagonists, or were merely a part of a larger, more complex narrative about the study and interpretation of the deep past, influenced by the conceptualization of the discipline in French and American intellectual contexts. The Bordes-Binford debate serves as a useful backdrop against which we can examine these larger intellectual trends and the allegorical narrative that has grown up around them.

CHAPTER 2

THE FRENCH CONTEXT

In 1953, when François Bordes published his slim article, “Essaie de classification des industries ‘moustériennes,’” in the *Bulletin de la Société Préhistorique Française*, Paleolithic research in France was barely a century old. In that article, Bordes first proposed the application of a new method he had developed to type and classify Mousterian stone tools, one that used as the basis for analysis the relative frequency of variable tool types within an overall assemblage. Bordes’ work built on the conventionally accepted idea of using variation in diagnostic tool types as the basis for ordering artifact assemblages within the context of time and space, while at the same time recognizing that variation must include those characteristics that had not been previously recognized as diagnostic. Bordes’ article was curiously and simultaneously a continuation of a long tradition of uniquely French archaeological method and thought and a new and original approach to understanding the Paleolithic. To understand how Bordes found himself in that position, it is first necessary to understand the historical context in which Paleolithic research in France developed.

2.1 Human Antiquity and the Notion of Cultural Evolution

For all practical purposes, the beginning of Paleolithic research in France began in the summer of 1847 when Jacques Boucher de Crèvecouer de Perthes (1788-1868) proposed that stone tools recovered from gravel deposits on the Somme River in northwestern France were of equivalent antiquity to the remains of extinct animals located in the same deposits. Boucher de Perthes’ discoveries were an outgrowth of an intense interest during the early nineteenth

century in the question of human antiquity. Advances in the natural sciences, particularly in geology and paleontology, had established the great antiquity of the Earth. These revolutions understanding the natural world were accompanied by an emergent body of work that sought to ascertain the antiquity of humans within this newly recognized prehistory.

Initially, Boucher de Perthes was roundly criticized and his claim rejected by most scholars in France as well as England. Yet, by the summer of 1859, further discoveries in England prompted a review of Boucher de Perthes' initial discoveries by a prestigious set of British scientists including geologist Joseph Prestwich, and archaeologist John Evans. In a remarkable turnaround, Prestwich reported to the Geological Society of London that "the occurrence of genuine Flint Implements at Moulin Quignon, the Champ de Mars, and Menchecourt receives additional confirmation from every fresh investigation, and places M. Boucher de Perthes's important original discovery beyond all doubt."³⁶ Boucher de Perthes received additional vindication when the prominent geologist Charles Lyell also subsequently gave his imprimatur to the notion of considerable human antiquity in his 1863 publication *The Geological Evidences of the Antiquity of Man*.

In these early years, the goal of most scholars was to develop a body of evidence for human antiquity. Thus began a concerted effort to develop an archaeological record for this period of prehistory, although it would be several years before the period was even given a name. In 1865, John Lubbock was the first to describe this newly recognized period as the Paleolithic or 'old' stone age. As a new avenue of research, Paleolithic archaeology lacked a framework in which discoveries could be situated and interpreted. In the first of many examples of intellectual exchange in archaeology, these early researchers adopted a framework from the related disciplines of geology and paleontology. This relationship was to have a long-standing impact on the field.

³⁶ J. Prestwich, "On the Section at Moulin Quignon, Abbeville, and on the peculiar Character of some of the Flint Implements recently discovered there" *Quarterly Journal of the Geological Society* (1863): 505.

Similarly, the influence of the social sciences on the development of Paleolithic research cannot be overlooked. The developmental trajectories of each converged with regularity in response to similar ideological needs. Interwoven with the quest to establish the antiquity of humanity was the reemergence of the notion of cultural evolution led by the British social theorist, Herbert Spencer (1820-1903). The Spencerian philosophy of social progress as the advancement from the simple to the complex in a unilineal, deliberate fashion resonated with a newly affluent and optimistic middle class, fueling the conventional wisdom that European civilization was the inevitable product of the trajectory of human (pre)history.³⁷ It was in this context that archaeological research into the human past became of intense interest to a general public eager to know the particulars of how humans had progressed through time. The intensity of the public debate on cultural evolution was stoked further by the publication in 1859 of Charles Darwin's essay, *On the Origin of Species*, despite the fact that Darwin's ideas of biological evolution differed significantly from those of Spencer's cultural evolution. Unlike Spencer, Darwin did not see biological evolution as a unilineal process, nor did he ascribe deliberate or conscious purpose to the process of natural selection. Yet the public (encouraged by Spencer) routinely conflated Darwin's idea of biological evolution with that of Spencer's idea of cultural evolution.³⁸ The result of this conflation was an increasing visibility and recognition of research into the human past; Paleolithic research was stepping into the limelight.

2.2 Legacy of Lartet and Mortillet

Two distinct but related trends emerged in the latter half of the nineteenth century that were to have significant and lasting implications for French Paleolithic research. One was the emergence of France as the main locus of Paleolithic research, and the other was the affiliation between Paleolithic archaeology and the related disciplines of geology and paleontology. The reason for the first trend was twofold: the geology of France resulted in excellent preservation

³⁷ M. Harris, *The Rise of Anthropological Theory*, (New York: Thomas Y. Crowell Co., 1968): 108-142.

³⁸ H. Spencer, *The Principles of Biology* (New York: D. Appleton and Company, 1866).

of Paleolithic sites in both the river valleys of the north and the limestone caves and rockshelters (abris) in the southwest; and with the earlier vindication of Boucher de Perthes, scholarly interest in the region increased significantly. The second trend, and arguably the more important, was largely the result of the work and influence of two scholars, Édouard Lartet and Gabriel de Mortillet. Lartet and Mortillet emerged and dominated Paleolithic research in France from 1860 to the turn of twentieth century. Both men, trained as paleontologists, exerted tremendous influence on the study of Paleolithic archaeology at this nascent and formative point, a legacy which lasted long after their deaths. Interestingly, each could be seen as the embodiment of the two main stereotypes of Paleolithic researcher: Lartet the 'digger' and Mortillet the 'thinker.' Together these two individuals forged a new field of study, and both provided a link in the chain that continues to bind Paleolithic research in France to the natural sciences.

Édouard Armand Lartet (1801-1871) was a lawyer who turned later in life to the study of paleontology to indulge an interest in fossil bones. Lartet made several significant paleontological discoveries including those of early fossil apes, *Pliopithecus* and *Dryopithecus*. An early advocate of Boucher de Perthes, Lartet brought a paleontologist's eye for classification to the study of the Paleolithic. His "Sur l'ancienneté géologique de l'espèce humaine dans l'Europe occidentale" published in 1860 outlined his thoughts on the antiquity of man based on paleontological evidence. Lartet provided a compelling argument that humans had in fact been contemporaries of extinct animals and that the great antiquity of man was now an established fact.

Lartet spent much of his second career excavating the caves and rock shelters in southwestern France with the support of his partner and financial patron, the English banker Henry Christy. Lartet discovered and excavated many of the best known Paleolithic sites in France, including the sites of Le Moustier, Laugerie Haute, and La Madeleine. Based on this work, Lartet developed a chronology of the Paleolithic based on the association of fossil animals

with archaeological artifacts. His stages included (from youngest to oldest) the Bison Age, Reindeer Age, Mammoth/Woolly Rhinoceros Age, and Cave Bear age. His work was initially announced in a preliminary publication, *Reliquiae Aquitanae*, in 1865 and expanded upon in a subsequent, posthumous publication in 1875, *Reliquiae Aquitanae; Being Contributions to the Archaeology and Palaeontology of Périgord and the Adjoining Provinces of Southern France*. Despite his interest in human antiquity, Lartet was first and foremost a paleontologist. Nowhere is this evidenced more than in his use of fossil animals (type fossils), a paleontological construct, to describe the stages of the Paleolithic, an archaeological construct. A typical passage from Lartet is as follows:

Palaeontologically, the human race of Aurignac belongs to the remotest antiquity. ... contemporary with the Aurochs, Reindeer, Gigantic Elk, Rhinoceros, Hyena, etc.; and what is more with the Great Cave Bear (*U. spelaeus*), which would appear to have been the earliest to disappear in the group of great mammals, generally regarded as characteristic of the last geological period.³⁹

Lacking a framework in which to classify archaeological material, Lartet turned to the intellectual tool box that had served him so well, that of paleontology. In Lartet's work we see the formation of what some have termed the phylogenetic paradigm in French Paleolithic archaeology, that is, the interpretation of cultural artifacts as the functional equivalent of paleontological type fossils.⁴⁰

Lartet's contributions to Paleolithic research are often eclipsed in the literature by those of another paleontologist turned archaeologist, Gabriel de Mortillet (1821-1899). Mortillet, like Lartet, was heavily influenced by his training in geology and paleontology and this was reflected in his research on the Paleolithic. Moreover, his research was limited largely to museum collections; he rarely attempted excavation himself. He used type specimens to organize and distinguish archaeological time periods, a common paleontological approach. Unlike Lartet,

³⁹ E. Lartet, "Nouvelles recherches sur la coexistence de l'homme et des grands mammifères fossiles réputés caractéristiques de la dernière période géologique," *Annales des Sciences Naturelles (Zoologie)* 15 (1861): 204.

⁴⁰ See Sackett 1981 and 1991.

however, he deliberately chose cultural artifacts as his diagnostic types, not index fossils, going so far as to note:

Leaving aside the paleontological method, I have called upon the archaeological method. Indeed, in archaeology is it not always by the industrial products that one determines the epochs? ...We seek to retrace the various phases of the development and of the history of man. Is it not more natural to characterize these phases by the works of man himself than by external events?⁴¹

Using this approach, Mortillet offered a chronology of the Paleolithic that has persisted to some extent well into the modern era (see Figure 2.1).

Although some have argued that Mortillet was less influenced by his background in geology and paleontology than has been traditionally proposed, Mortillet certainly relied on that framework in conceptualizing and organizing his thoughts on the chronology of the Paleolithic.⁴² Artifacts may have gained a primacy of place in Mortilletian systematics, but his treatment largely amounted to no more than as a substitution for type fossils. Mortillet's adherence to a paleontological framework extended to his use of nomenclature convention adopted directly from paleontology and geology that assigned the names of type sites to designated time periods. Mortillet is to be thanked for the enduring names of archaeological time periods such as the Acheulean (after the site of Saint-Acheul), the Mousterian (Le Moustier), the Solutrean (La Solutré), and the Magdalenian (La Madeleine), names all instantly recognizable and accepted by Paleolithic researchers today. Mortillet also pioneered the use of experimental and quantitative studies in testing the authenticity of artifacts as well as manufacturing techniques.

A fundamental distinction between Lartet and Mortillet was Mortillet's penchant for applying a unilinear evolutionary approach to Paleolithic research, an abstraction that never explicitly found its way into Lartet's work. Mortillet's adherence to the idea of the inevitability of human progress predated his interest in archaeology and permeated his work throughout his

⁴¹G. de Mortillet, "Classification des ages de la pierre," *Revue d'Anthropologie* 1 (1872): 435.

⁴² Chazan, Michael. "Conceptions of Time and the Development of Paleolithic Chronology," *American Anthropologist* Vol. 97, No. 3 (1995): 457-467.

TEMPS	AGES	PÉRIODES	ÉPOQUES	
Quaternaires actuels.	Historiques.	du Fer.	Mérovingienne. <i>(Waben, Pas-de-Calais.)</i>	
			Romaine.	Champdoliennne. <i>(Champdolent, Seine-et-Oise.)</i>
				Lugdunienne. <i>(Lyon, Rhône.)</i>
	Protohistoriques.	du Fer.	Galatienne.	Beuvraysienne. <i>(Mont-Beuvray, Nièvre.)</i>
				Marnienne. <i>(Département de la Marne.)</i>
				Hallstattienne. <i>(Hallstatt, haute Autriche.)</i>
	du Bronze.	Tsiganienne.	Larnaudienne. <i>(Larnaud, Jura.)</i>	
			Morgienne. <i>(Morges, canton e Vaud, Suisse.)</i>	
	Quaternaires anciens.	Préhistoriques.	Néolithique	Robenhausienne. <i>(Robenhausen, Zurich.)</i>
				Campignyenne. <i>(Campigny, Seine-Inférieure.)</i>
Tardenoisienne <i>(Fère-en-Tardenois, Aisne.)</i>				
Paléolithique			Tourassienne. <i>(La Tourasse, Haute-Garonne.)</i>	
			Ancien Hiatus	
			Magdalénienne. <i>(La Madeleine, Dordogne.)</i>	
			Solutréenne. <i>(Solutré, Saône-et-Loire.)</i>	
			Moustérienne. <i>(Le Moustier, Dordogne.)</i>	
Acheuléenne. <i>(Saint-Acheul, Somme.)</i>				
Chelléenne. <i>(Chelles, Seine-et-Marne.)</i>				
Tertiaires.		Éolithique	Puycournienne. <i>(Puy-Courny, Cantal.)</i>	
			Thenaysienne. <i>(Thenay, Loir-et-Cher.)</i>	

Figure 2.1 Mortillet's Paleolithic Chronology, from *Le Musée Préhistorique*, 1881.

long career. Mortillet had a lifelong personal antipathy toward the religiously conservative French monarchy and an avowed adherence to radical socialism. The assumption by Mortillet, and of many other Paleolithic researchers of the time, was that human prehistory would follow the same progressive stages that they saw in documented historical contexts. Mortillet and other radical thinkers explicitly rejected the creationist notions promoted by the monarchy and the clergy which, in their eyes, existed solely to justify the continued control of the church. For Mortillet, the goals of Paleolithic research were to document and provide evidence for the great antiquity of humanity and to refine our understanding of how far into the past technological and material progress could be tracked. Only through this could he demonstrate that progress was natural and inevitable, thereby providing the rationale that the next stage in human progress would be made via those same sorts of technological and material changes. Mortillet was not interested in environmental and ecological adaptations by past humans or in trying to understand how past humans lived and behaved. As a result, Mortillet saw artifacts less as manifestations of past behavior and more as diagnostic devices for purposes of dating or establishing stages of technological development, much like index fossils in paleontology.

Social theorists and ethnologists such as Lewis Henry Morgan and E.B. Tylor in the mid-to-late nineteenth century reinforced this image of the past as a series of unilineal stages by Paleolithic researchers with their formulations of general stages of development (savagery, barbarism, civilization) through which they speculated that all human societies had progressed.⁴³ In an ironic example of intellectual reciprocity, early work in Paleolithic research like that of Mortillet that stressed technological advancements, emboldened purveyors of these evolutionary schemas by providing a seemingly scientific body of supportive evidence, which in turn promoted the interpretation of archaeological evidence within an evolutionary framework.

⁴³ See L.H. Morgan, *Ancient Society* (Cambridge: Belknap Press of Harvard University Press, 1964 first published 1877) and E.B. Tylor *Primitive Culture: Researches into the Development of Mythology, Philosophy, Religion, Language, Art and Custom* (London: Murray, 1871).

Mortillet was convinced that his work revealed an immutable law of unilineal progress that had roots along with humanity itself in the deep past.⁴⁴

The legacy of Lartet and Mortillet is one primarily of the continuing alignment of Paleolithic archaeology with the natural sciences of geology and paleontology. But at the time and still today, that association was a double-edged sword. On one hand, Paleolithic research was elevated in status by its association with geology and paleontology, so-called hard sciences. During the nineteenth century, optimistic views of science seemed to show potential to solve a raft of medical and social ills. Science also might provide a knowable past that conceded the pivotal and special role of European civilization and, in particular, its large and affluent middle class. Thus it accorded scientific disciplines a privileged place in the social and intellectual fabric. Paleolithic researchers reaped the many social and financial benefits of that association throughout the nineteenth century. On the other hand, the uncritical application of the methodology and practice of natural sciences like geology and paleontology (which in the nineteenth century focused almost exclusively on establishing the antiquity of humanity and documenting evolutionary progress in the human past) prevented Paleolithic researchers in France from addressing questions of specific human adaptations and cultural processes and behaviors. This intellectual delay was to play itself out in many other future contexts.

2.3 The Court of Abbé Breuil (1905-1940)

Shortly after the turn of the twentieth century, Paleolithic research in France departed in significant ways from that of Lartet and Mortillet, yet at the same time their legacy still exerted considerable influence in terms of basic chronology and methodology. French prehistorians, led by the Abbé Henri Breuil (1877-1961), rejected the unilineal evolutionism of Mortillet, which they viewed as an anticlerical and crassly materialistic view of the past. The new path, which has been variously termed traditional or 'straight' archaeology, was associated with an emerging

⁴⁴ G. de Mortillet, *Le Préhistorique: Antiquité de l'Homme* (Paris: Reinwald. 1883).

professionalism in the field.⁴⁵ Breuil and his contemporaries – a group including Marcellin Boule, the noted physical anthropologist; Raymond Vaufrey, editor of *L'Anthropologie*; Pierre Teilhard de Chardin, renowned paleoanthropologist; and Hugo Obermaier, a respected Swiss archaeologist -- were self-described prehistorians, not geologists or paleontologists, and they undertook to reshape Paleolithic research in France into a model of detailed and systematic inquiry. Research strategies shifted from the larger picture of documenting the grand progress of human societies to focus on the development of regional archaeological sequences. For the first time, the emphasis was on the delineation of specific and localized cultural traditions. Particularistic study of the stratigraphy of sites and the development of minutely detailed typologies of artifact types characterized the approach of researchers in this traditional era.

The unilineal evolutionists, however, did not give up easily or quietly. Mortillet and his successors, Adrien de Mortillet and Paul Girod most notably, clashed repeatedly with Breuil.⁴⁶ The arguments were bitter, polemical attacks from both sides over interpretations of archaeological sequences and Paleolithic chronologies. Ultimately, however, a quiet revolution took place. As the discipline of Paleolithic archaeology began to focus on regional research, the gaze of the discipline turned more frequently inward and away from the grand theorizing typical of the Mortilletians. This is evidenced by the emergence of professional organizations dedicated to promoting prehistoric research on a regional and national scale. In French Paleolithic research, the most important of these new professional bodies was the Société Préhistorique Française, founded in 1904. The Société offered for the first time to prehistorians, both professional and vocational, a sense of collaboration and identity, organizing national congresses and meetings on the state of prehistoric research in France and showcasing

⁴⁵Sackett 1981 and 1991.

⁴⁶ See P. Smith, *Le Solutréen en France* (Bordeaux: impr. Delmas, 1966) and M. Chazan, "Conceptions of Time and the Development of Paleolithic Chronology," *American Anthropologist* (1995) for detailed studies of the battle between Breuil and Mortillet over time and its control in the development of a Paleolithic chronology.

examples of regional work. In its first edition, the *Bulletin de la Société Préhistorique Française* outlined its purposes as follows:

1. To organize paleolithic archaeologists and, generally, anyone interested in the study of the most remote periods of the history of France and its colonies;
2. To publish documents that will reconstruct this history,
3. To address the conservation of stone monuments;
4. To encourage research related to prehistory;
5. To organize conferences in Paris or in the provinces or in the more interesting parts of France; and
6. To facilitate exchanges between collectors.⁴⁷

The *Bulletin* provided an outlet to the numerous local and regional scholars who had had little voice during the period dominated by Mortillet. The research of these individuals, many of whom were local doctors, school teachers, and government workers, served in the decades just before and after World War I to subdue the loud voices of Mortillet and others who were concerned with questions for which they had little interest or patience. These local researchers were interested primarily in documenting the sites and outlining the prehistoric chronologies in their local villages and provinces. They were not concerned with larger issues of human progress and evolutionary stages of mankind.

Although it would be misleading to say that any one person solely defined this period of development in French Paleolithic research (the figures of Denis Peyrony, Louis Capitan, and Emile Carthailac are frequently cited and their work stands the test of time), it was the Abbé Henri Breuil who wielded the most considerable influence. Lawrence Straus refers to Breuil as “the central character in the development of Paleolithic prehistory in the first half of the 20th century.”⁴⁸ Bruce Trigger identifies Breuil as the first archaeologist in France to apply a culture-historical approach to the Paleolithic, according him primacy of position and influence during this period.⁴⁹ Sackett, on the other hand, downplays his influence as one of many traditional

⁴⁷ Société Préhistorique de France, *Bulletin de la Société préhistorique française*, 1904, n° 1: 1.

⁴⁸ L. Straus, "L'Abbé Henri Breuil: Archaeologist", *Bulletin of the History of Archaeology* 2, no. 2 (1992): 5.

⁴⁹ Trigger, *A History of Archaeological Thought*, 254.

researchers active just prior and subsequent to World War I.⁵⁰ Regardless of these characterizations, to ignore Breuil would lead to an incomplete understanding of the context of French Paleolithic archaeology leading up to Bordes.

Breuil's influence waxed and waned over a lengthy career.⁵¹ His passion for prehistory began in seminary and, although he was ordained as a priest, he never practiced. Instead he became a professional prehistorian. Breuil initially made his mark in studying Paleolithic cave art, but it was a very public battle with Mortillet that brought him to the fore of Paleolithic research. As a young man, Breuil initiated a confrontation with Mortillet over the chronology of the Paleolithic that Mortillet had proposed in late nineteenth century (see Figure 2.1). Breuil, in a series of articles published between 1905 and 1913, argued that Mortillet had grievously overlooked a significant Paleolithic epoch known as the Aurignacian, which accounted for a substantial amount of what we now refer to as the Upper Paleolithic. In what is sometimes called the Battle for the Aurignacian, Breuil systematically applied his intimate knowledge of the regional chronological sequences in southwestern France to dismantle and reassemble Mortillet's original chronology.⁵² At stake in this battle was not merely a refined chronology; as it turned out, Mortillet's general chronology was preserved in the nomenclature, which has given it longevity in spite of its inaccuracy. Breuil represented the new breed of Paleolithic researcher for whom the armchair theorizing of the Mortilletians about human progress was mere speculation. There was likely personal animosity, too, given Mortillet's politics and views of the Church.

Breuil and his contemporaries are largely responsible for bringing professionalism to Paleolithic research in France, and they brought many young students into their sphere of

⁵⁰ Sackett, "From de Mortillet to Bordes: A century of French Paleolithic research," 87.

⁵¹ See A. Broderick, *Father of Prehistory* (New York: William Morrow & Company, 1963) for a comprehensive biography of Breuil's life.

⁵² There are interesting parallels between the Breuil-Mortillet debate over the Aurignacian and the Bordes-Binford debate.

influence, including François Bordes. Breuil's success in shaping the careers of many French archaeologists and, indeed, many archaeologists outside France as well, stemmed mainly from his position as the founding professor of the Institut de Paléontologie Humaine (1910) and as the first professor of prehistory at the Collège de France (1929). One aspect of this professionalism was the refinement in excavation methodology and an overriding emphasis on the craft of archaeology that Breuil so highly valued. His battles with Mortillet left him skeptical of theory. He avoided tackling larger questions related to culture change and urged his students to avoid them as well.

In the years between 1900 and 1940 the alignment of Paleolithic archaeology with the natural sciences was cemented and recast as Pleistocene studies. In addition to Breuil was another figure less well known to Americans, but who played an influential role in this: Raymond Vaufrey, an expert on Quaternary faunal remains. Vaufrey's contributions to and influence on French Paleolithic archaeology came in the form of defining the field in terms of Pleistocene studies, which included an integration of prehistory, paleontology, and Quaternary geology. Vaufrey's long-held position as editor of the respected French journal, *L'Anthropologie*, gave him a level of influence unmatched by others of the time and a powerful platform from which to promote an integrated field. The current alignment of Paleolithic archaeology with the natural sciences in French universities and its focus on culture history is a legacy begun by Breuil and solidified by Vaufrey. It was in the shadow of these two individuals that Bordes came of age personally and professionally and his approach to archaeology was shaped to a large extent under their theoretical and methodological tutelage.⁵³

2.4 The Postwar Years

The postwar years in French Paleolithic archaeology were in many respects a seamless transition from the decades prior. Breuil was still exceptionally influential and because of him a focus remained on the development of regional chronologies. Two trends, however, began to

⁵³ F. Bordes, "In Memory of Raymond Vaufrey," *Current Anthropology* 9, no. 4 (1968): 337-338.

emerge and set the stage for Bordes and his later contemporaries. One was the solidification of the so-called phylogenetic paradigm that was peculiar to French Paleolithic research; and the other was the introduction of Americans into the landscape of French archaeology in a visible and active role. The two trends were not always parallel, converging at times in a way that affected practitioners on both sides of the Atlantic.

Despite the distancing from the Mortilletian period in Paleolithic research that Breuil aimed for and thought he had achieved, many of the assumptions and interpretations that were first put forth by Lartet and Mortillet persisted. This is most apparent in the continued acceptance of a general paleontological framework in the ordering and interpretation of the archaeological record, what Sackett, Straus and others have termed the phylogenetic paradigm.⁵⁴ Breuil and others, notably Denis Peyrony, a renowned excavator in southwestern France, relied on the concept of the *fossiles directeurs* as the basis for their chronologies. Much like index fossils in the paleontological record, *fossiles directeurs* were assumed to be discretely indicative of both time and space within the archaeological record. Moreover, in a uniquely French interpretation, these *fossiles directeurs* were interpreted as discretely indicative of unique cultural traditions as well, which led to the unquestioning interpretation of any variance in assemblages as the result of differing cultural traditions or the movement of Paleolithic 'tribes'.⁵⁵ As Sackett has noted, this paleontological approach resulted in the assumption that "culture history can be regarded and accounted for in essential *organic* terms."⁵⁶ In other words, if specific cultural traditions are associated with only one type of artifact tradition, then artifacts can be understood to 'behave' like paleontological phyla. This assumption became a key

⁵⁴ J. Sackett "Straight Archaeology, French Style: The phylogenetic paradigm in historic perspective," (1991) and L. Straus, "Paradigm found: a research agenda for study of the Upper Paleolithic and post-Paleolithic in SW Europe," (1992).

⁵⁵ H. Breuil, "Les subdivisions du Paléolithique supérieur et leur signification," *C.R. 14^{ème} Congrès Préhistorique Française*. Geneva (1913): 165-238.

⁵⁶ Sackett, "From de Mortillet to Bordes," 90.

determinant in how Bordes interpreted the archaeological record and is likely the root of his eventual clash with Binford.

Hallam Movius, an American archaeologist working in France in the 1940s and 1950s, provided an influential overview of the weaknesses of the type of 'organic' or paleontological models that were routinely invoked in French Paleolithic archaeology.⁵⁷ In discussing Breuil's proposal that two distinct but parallel phyla of tool traditions (which Breuil equated with different cultures) accounted for variability in Lower Paleolithic tools in France, Movius observed that "although the parallel phyla concept is not supported by the field evidence, it is still generally accepted by the majority of workers in the field of Paleolithic archaeology."⁵⁸ In other words, French prehistorians conceived of tool traditions as being associated with only one culture and any recognition of contemporaneity of cultures was assumed to be an anomaly that could only be explained by invoking the concept of distinct but parallel cultural groups operating much like species or phyla. Because of this organic concept of culture, alternative explanations for co-occurrence and variability within tool traditions were simply not recognized. This would remain the case until Bordes' work began to chip away at the concept and underlying assumptions of the *fossiles directeurs*.

The reliance on this organic model of culture produced in France a different interpretation of the notion of culture history. Culture was conceived, in this context, as a purely material expression; thus, when a change in technology or material artifacts was documented in the archaeological record, it was interpreted as the expression of a new culture. This view, in turn, reinforced and promoted a concern with refinement of methods and techniques to increase the resolution within the archaeological record. Following from this concept and operationalized in the discipline of archaeology in France, the more precisely one could define and describe artifacts, the better one could 'know' the past culture. Hence the main goal of French culture-historical archaeology was that of description.

⁵⁷ Movius, "Old World Prehistory," 163-164.

⁵⁸ *Ibid.*, 164.

In this intellectual climate French Paleolithic archaeology acquired its focus on practice and method rather than theory. The key to understanding the deep past from a culture-historical perspective was greater clarity in the description of the archaeological record, and the way to greater clarity lay in the refinement of method and field practices, specifically in terms of understanding stratigraphy and typology. This disciplinary concern with method or field practices, sometimes known as “straight archaeology, French style” or ‘methodolatry’, a slavish devotion to method at the expense of or detriment to theory, ultimately became the defining characteristic of French Paleolithic archaeology.

CHAPTER 3

THE AMERICAN CONTEXT

Archaeology in the United States arose from a curious blend of Enlightenment zeal, racist ideology, and American optimism and romanticism about the past. Until the late twentieth century, archaeology in America had been primarily an activity practiced by individuals of European heritage (i.e., Euro-Americans). The peculiarity of American archaeology is that in the beginning -- and, one could argue, well into the latter twentieth century-- archaeological research was a colonial enterprise. As such, it bore the mark of an outsider looking in, attempting to explain the past activities and behaviors of other people. This outsider perspective remains particularly acute when the topic of study is the prehistory of the Americas prior to European conquest. Unlike in Europe, early European settlers in the United States contended with a population whose very existence had to be explained in a manner consistent with the prevailing European worldview. Some of the earliest questions asked by European settlers were about the origins of Native Americans and their relationship to the peoples of the Old World; ironically, those same questions continue to be asked by archaeologists today. This unique context had a significant and profound impact on how archaeology was/is conceptualized and practiced in the United States as opposed to Europe. American archaeologists had to account for the role of living peoples and their ancestors in the creation of the record they were attempting to study and explain. But because of the divide between the researcher and the object of study, the predominant influence on the fledgling discipline of archaeology was anthropology (initially called ethnology). In the late nineteenth century, American archaeology did not appear to differ to any great extent from archaeology as practiced in Europe, especially in France, although there were some hints of the division to come. It was not until near the turn of the twentieth century that American archaeology formally coalesced

around the subfield of anthropology; this is the most typical affiliation for the discipline in American universities.

3.1 Evolutionary Archaeology and the Synthesis Of Race (Mid-Late Nineteenth Century)

American archaeology in the mid-nineteenth century was difficult to distinguish from that of Europe. Many of the same trends (influence of geology and paleontology and the evolutionary models, both social and biological) were evident in the few systematic studies of American prehistory of the day, such as those undertaken to address the Moundbuilders.⁵⁹ The tie to European archaeology was explicitly promoted by influential men such as Joseph Henry, the first secretary of the Smithsonian Institution, in the publication of his annual reports.⁶⁰ As in Europe, the more romantic ideals of the Enlightenment -- built on the notion that all humans, regardless of ethnicity, are capable of the same intellectual achievements -- were giving way to a new perspective that posited a linear view of human cultural progress leading, of course, to the purportedly superior societies of Europe. All technological change in the past was thought to be unidirectional and reflective of stages of progress. In the mid-nineteenth century, unilinear evolutionism became a standard interpretation of the change documented in the archaeological record.

American archaeology, however, was different in one significant respect from that of Europe. For Euro-Americans, the archaeological record was not the product of their distant ancestors. It was different, it was other. But was it the record of extant Native Americans? Or was it the record of some long-vanished peoples? For Euro-Americans, explaining the archaeological record was part of the larger question about how to explain Native Americans,

⁵⁹ See E.G. Squier and E.H. Davis, *Ancient Monuments of the Mississippi Valley*. (New York: Bartlett & Welford, 1848), H. Schoolcraft, *Historical and Statistical Information Respecting the History, Condition, and Prospects of the Indian Tribes of the United States. Collected and Prepared Under the Direction of the Bureau of Indian Affairs Per Act of Congress of March 3rd, 1847*. (New York: Paladin Press, 1852, reprinted, 1969), and D. Wilson, *Prehistoric Man; Researches into the Origin of Civilization in the Old and the New World* (Cambridge: Macmillan & Co, 1862).

⁶⁰ See Trigger, *A History of Archaeological Thought*, 2006 and Willey and Sabloff, *A History of American Archaeology*, 1993.

people viewed in the nineteenth century as a primitive race. There was great reluctance on the part of Euro-Americans to accord any recognition of cultural progress to Native Americans. Many of the nineteenth-century views about Native Americans held by Euro-Americans were shaped by the then precarious state of those groups that had suffered the tolls of disease and war brought by European conquest. Indeed, many archaeologists of the day shared the sentiment of Henry Rowe Schoolcraft, an Indian agent and early ethnologist, that Native American groups were a vanishing race, soon to be known only through ethnographic accounts and the archaeological record.⁶¹ In the United States, this prevailing view of Native Americans as static and lacking in the intellectual ability to progress was soon allied with the notion of unilineal evolution.

As discussed in Chapter Two, the individual most associated with unilineal evolution as applied to prehistoric archaeology in France was Gabriel de Mortillet; in the Anglophone world that role was played by John Lubbock (1834-1913). A contemporary and neighbor of Charles Darwin, Lubbock was the first to synthesize formally the concept of race with that of cultural evolution in his landmark works *Prehistoric Times* (1865) and *The Origin of Civilisation and the Primitive Condition of Man* (1870). His influence on nineteenth-century archaeology in the United States was unmatched in his time. His two seminal books went through numerous editions and were among the few reference texts for young researchers taking up archaeology as an avocation. Further, Lubbock was pivotal in shaping the thoughts of American anthropologists such as Lewis Henry Morgan (1818-1881), who distilled much of Lubbock's synthesis in his highly influential text *Ancient Society* (1877).

The pairing of the concept of race, and, in the case of Native Americans, racial inferiority, with that of unilineal cultural evolution and its application to prehistory resulted in what has come to be known as evolutionary archaeology. Evolutionary archaeology was particularly seductive for Euro-American researchers because it provided a framework in which

⁶¹ C. Hinsley, *Savages and Scientists: The Smithsonian Institution and the Development of American Anthropology, 1846-1910* (Washington, D.C.: Smithsonian Institution Press, 1981).

their preconceptions of Native Americans as savage and barbaric could be justified and explained.⁶² It is likely not a coincidence that evolutionary archaeology reached a peak in the United States at a time in which westward expansion of Euro-American settlement was seen as a moral imperative.⁶³ In the context of evolutionary archaeology, Native Americans assumed roles of living, but unchanging, examples of the evolutionary stages of human societies. If one could describe and understand Native American societies, these insights could illuminate how people lived and behaved in past societies irrespective of cultural or geographical context. Evolutionary archaeologists routinely applied analogies drawn from Native American ethnographic or archaeological contexts to explain Old World prehistory.⁶⁴ And, since Native American societies were assumed to be fixed and unchanging, migration and diffusion were the explanations of choice among evolutionary archaeologists for any discernible change documented in the archaeological record and they were employed regularly to account for any evidence of creativity or innovation.

As a result of the influence of unilineal evolutionism, ethnology became part and parcel of the American archaeological landscape. Daniel Wilson (1816-1892) states in the preface to the first edition of his widely read *Prehistoric Man: Researches into the Origin of Civilisation in the Old and the New World* (1862),

These researches into the origin of civilisation have accordingly been pursued under the belief which influenced the author in previous inquiries, that the investigations of the archaeologist, when carried on in an enlightened spirit, are replete with interest in relation to some of the most important problems of modern science. To confine our studies to mere antiquities is like reading by candle-light at noon-day; but to reject the aid of archaeology in the progress of science, and

⁶² Trigger, *A History of Archaeological Thought*, 177.

⁶³ The Moundbuilder debate is an excellent example of the changing notions of archaeology and how it was part of the larger national debate on policy concerning treatment of Native Americans. Willey and Sabloff (1993: 45) note that the debate was “more than pure curiosity ... the more primitive [Native Americans] were thought to be, the easier it apparently was to justify their destruction or displacement.”

⁶⁴ J. Lubbock’s volumes of 1865 and 1870 use such examples extensively as did Wilson (1862), and Mortillet (1872 and 1883).

especially of ethnological science, is to extinguish the lamp of the student when most dependent on its borrowed rays.⁶⁵

Wilson, like other evolutionary archaeologists of the time, clearly saw the purpose of American archaeology in terms of how it could be used to understand the prehistory of Europe and the prehistory of humankind in general. In the second edition of *Prehistoric Man* (1865), Wilson clarifies that role even further, noting that

During the same period many zealous observers have been striving to recover the traces of Man in that strange era of Europe's unchronicled centuries, which long preceded all beginnings of history. But while their researches are being rewarded by discoveries of the profoundest interest, every fresh disclosure confirms the impression produced on the Author's mind in reference to the aboriginal tribes, and the native arts and customs of the American continent: that he had previously realized much in relation to a long obliterated past of Britain's and Europe's infancy, which he has there found reproduced as a living reality. The Western Hemisphere is only now beginning to be historical; yet it proves to have been the theatre of human life, and of many revolutions of nations, through centuries reaching back towards an antiquity as vague as that which lies behind Europe's historic dawn; and the study alike of the prehistoric and the un-historic races of America is replete with promise of novel truths in reference to primeval man.⁶⁶

It should be noted that Wilson, unlike Lubbock, did not accept the inferiority of Native Americans on racial grounds, but he, like many other archaeologists of the nineteenth century, did credit the notion of Native Americans as culturally inferior to Europeans.

The evolutionary approach relegated archaeology in United States to the role of a descriptive science. Indeed, in their landmark history, Gordon Willey and Jeremy Sabloff even define this era in American archaeology as the Classificatory-Descriptive period.⁶⁷ That is not to say, however, that American archaeology did not make great strides in the latter half of the nineteenth century. The great debate over the origins of the immense and spectacular mounds of the Eastern United States drew the interest of some of the pioneers in American archaeology including Ephraim Squier (1821-1888), Edwin Davis (1811-1888), Samuel Haven (1806-1881),

⁶⁵ Wilson, *Prehistoric Man*, xviii.

⁶⁶ *Ibid.*, xvii.

⁶⁷ Willey and Sabloff, *A History of American Archaeology*, 1993.

Cyrus Thomas (1825-1910), and Frederick Putnam (1839-1915), who collectively contributed to a body of work that eventually attributed these rightfully as the cultural heritage of extant Native Americans. These men and others worked to develop standards in excavation, and their concerted “archaeological-ethnological attack” on the question of the origins of Native Americans solidified the link between archaeology and anthropology in the United States.⁶⁸ Similarly, the numerous scientific expeditions to the American West, largely sponsored by the American military and the United States Geological Survey, generated a tremendous amount of description and artifact collections of Native American groups both extinct and extant. The processing of these materials by museum curators necessitated the development of complex typological and classificatory schemes.

The net effect of these activities served to legitimize the fledgling discipline of archaeology in the United States. The establishment of the Smithsonian Institution in 1846, with its Bureau of American Ethnology (founded in 1879), was largely the result of the public fascination with antiquarianism, the origins of Native Americans, and the desire to develop a national identity in the United States to rival that of Europe. The publicly supported Smithsonian acted as sponsor, benefactor, and repository of artifacts to many of the most notable archaeologists of the nineteenth century. The Peabody Museum at Harvard University, established in 1866, served a similar, albeit private role, as did the Field Museum in Chicago when it was established in 1893. Professional associations and journals were also established, including the American Anthropological Association (begun as the Anthropological Society of Washington in 1888) and the Archaeological Institute of America (founded in 1879), which provided an outlet for practitioners eager to be seen in a more legitimate light. By the latter nineteenth century, American universities began to create programs in anthropology and archaeology. Daniel Brinton (1838-1899) established one of the first programs in archaeology at University of Pennsylvania in 1886 as part of the larger field of anthropology; this set a

⁶⁸ Ibid., 90.

precedent that was to fix archaeology as an anthropological pursuit in the organizational structure of most American universities. Harvard followed suit in the same year when Frederic Putnam (1839-1915) established the Department of Anthropology as a teaching arm of the Peabody Museum.

Despite the advances in American archaeology during the later nineteenth century, there were clear gaps in knowledge and practice. Questions of behavioral inference and cultural change were not considered by most archaeologists; consequently, there was a downside to this willful neglect. If the driving force behind American archaeology was to illuminate general human (i.e., European) prehistory, as Wilson and Lubbock theorized and as field archaeologists in the United States acknowledged, then its value was limited once that prehistory had been clearly defined. In other words, once archaeology had exhausted its ability to provide more information via descriptive means, then modern ethnographic data would be sufficient to supply the rest of the answers. As Trigger notes, by the turn of the twentieth century, “the problem with unilineal evolutionary archaeology was that it had become too integral a part of anthropology and too dependent on ethnology.”⁶⁹ Archaeology needed to find an identity of its own.

3.2 Culture-Historical Archaeology and the Concept Of Culture (Pre-World War II)

The association of archaeology in the United States with anthropology at the turn of the century was one with positive and negative consequences. As aided by the financial support of the Bureau of American Ethnology and other entities such as the Peabody and Field museums, archaeology in the United States was relatively well-funded for a nascent discipline. Also, archaeology was a field that had a built-in problem to solve: to advance the understanding of Native American groups as a means to understanding the prehistoric human past. Archaeology benefited by its association with anthropology because it was viewed as a critical

⁶⁹ Trigger, *A History of Archaeological Thought*, 209.

helpmate in filling out the general chronology of human prehistory. The chinks in the armor of unilineal evolution were significant, however, and as anthropologists in the later nineteenth century began to abandon it as a legitimate approach to understanding the past, archaeology, too, fell from favor. As Willey and Sabloff observed, “the strength of archaeology is its perspective, in which it examines culture change and development through time. These were definitely not the objectives of the American archaeological-ethnological establishment as it emerged into the twentieth century.”⁷⁰

The driving force behind this shift in American anthropology was the emergence of historical particularism, a school of thought most closely associated with Franz Boas (1858-1942). Marvin Harris, author of a seminal history of anthropological theory, has called Boas “one of the most influential figures in the history of the social sciences.”⁷¹ Boas, strongly influenced by the emerging anti-evolutionary philosophy in his native Germany during the late nineteenth century, confronted a discipline that was overburdened by more theories than facts, a situation he found intolerable.⁷² Boas espoused an approach to anthropology that was free from armchair speculation and grounded in the historical and/or ethnographic particulars of a given society. He eschewed generalities in favor of studying specific human groups (cultures) in their unique historical and geographical contexts, an inductive approach to the past. The impact of Boasian anthropology on American archaeology should not be underestimated.⁷³

Early in the twentieth century and largely as the result of Boasian influence, American archaeology had to reinvent itself. Lacking the ready-made problem orientation of unilineal evolution, archaeology in the United States had to be reconceptualized to answer a new set of

⁷⁰ Willey and Sabloff, *A History of American Archaeology*, 90.

⁷¹ Harris, *The Rise of Anthropological Theory*, 250.

⁷² See Harris, *The Rise of Anthropological Theory*, 267-277 and Trigger, *A History of Archaeological Thought*, 180-81.

⁷³ For excellent summaries of Boas' influence on the field see Harris, *The Rise of Anthropological Theory*, Trigger, *A History of Archaeological Thought*, and Willey and Sabloff, *A History of American Archaeology*, 1993.

problems, which in the Boasian era were questions related to the development of regional chronologies and histories of specific human groups. In response, American archaeology entered a new phase that is defined by most as the culture-historical period.⁷⁴ Two trends are worthy of note here. First, it was during this period that archaeology in the United States acquired its focus on chronology and stratigraphic control. Second, archaeology in the United States accepted a new primary directive described by Robert Dunnell as the 'culture history paradigm.'⁷⁵ Each trend is addressed in turn below.

Prior to the turn of the century, one of the great weaknesses of American archaeology was its inability to develop regional chronological sequences, which were at the heart of the new questions that arose in response to historical particularism. As explored in Chapter Two, regional chronology was an early and controversial topic in France; such was not the case in the United States, where adherence to a unilineal evolutionary approach that did not recognize cultural change within Native American groups delayed a concern with chronology. In other words, if Native American groups were incapable of change or progress, then chronologies were not a meaningful line of research. With the abandonment of the concept of cultural evolution, it became imperative to explain the archaeological record in terms reflective more of what was observable than what was assumed. Also, as the general locus of American archaeology shifted from the East to the West, the value of stratigraphy and chronology became much more important. Sites in the Southwest had deep, rich deposits that were largely undisturbed. In order to fully excavate these sites and obtain an adequate chronology, tight control over the stratigraphy was necessary and desirable. This level of control was not possible in the thin deposits typical of sites in the East and/or those with significant disturbance,

⁷⁴ Although most historians of archaeology (Trigger 2006, Meltzer 1986, and Dunnell, 1986) call this period in American archaeology the Culture-Historical period, Willey and Sabloff (1993) employ the term Classificatory-Historical.

⁷⁵ Dunnell, "Five Decades of American Archaeology," 29.

as was the case in many of the mound sites that had drawn so much archaeological attention.⁷⁶ In the American Southwest, archaeologists confronted many sites with stratigraphic sequences that demanded a level of fine control that was beyond the ken of most fieldworkers.

The first scholar to tackle the development of a regional chronology backed up by well-documented stratigraphy was Nels C. Nelson at the site of San Cristóbal, New Mexico.⁷⁷ Nelson benefited in his activities from two significant influences. He had been a student of anthropology under Alfred Kroeber, who was closely associated with Boas. From Boas Kroeber acquired an appreciation for archaeology's role in developing distinct, regional culture histories, an appreciation he passed on to Nelson. Nelson also experienced firsthand the utility of stratigraphy for establishing chronology under the tutelage of Henri Breuil at the site of El Castillo, Spain. Nelson noted later in life that "my chief inspiration to search for chronological evidence came from reading about European cave finds; from visiting several of the caves, seeing the levels marked off on the walls, and in taking part in the Castillo Cave in Spain for several weeks in 1913."⁷⁸ Nelson's work at San Cristóbal established the standard for controlled excavation that was to become *de rigueur* in American archaeology. With this newfound stratigraphic control, Nelson was able to conclusively demonstrate both cultural continuity and change within an archaeological sequence, the first time that this had been done at a Native American site.

Nelson's work was extremely influential in American archaeological circles; within several years the renowned Southwestern archaeologist Alfred Kidder had adopted Nelson's techniques and a new era was begun.⁷⁹ Kidder was the first to use Nelson's techniques to

⁷⁶ Willey and Sabloff, *A History of American Archaeology*, 98-99.

⁷⁷ N. C. Nelson, "Chronology of the Tano ruins", *American Anthropologist* 18, no. 2 (1916): 159-180.

⁷⁸ Personal communication to Richard Woodbury, quoted in "Nelson's Stratigraphy," *American Antiquity* 26, no.1 (1960): 98-99.

⁷⁹ Trigger, *A History of Archaeological Thought*, 2006.

establish a regional chronological sequence based on a classification system he developed to define archaeological cultures.⁸⁰ It was this use of stratigraphy by Kidder to develop a history of an entire regional cultural sequence that, it could be argued, was the first instance of culture-historical archaeology. Kidder's advancement of Nelson's original work and his synthesis of chronological control with a classification hierarchy for artifacts was itself influential, spawning a number of attempts to develop regional chronological and classification schemes, including the highly successful Midwestern Taxonomic Method.⁸¹

By the 1930s, as interest in chronological control took hold in American archaeology, the discipline began to frequently turn to a methodology designed explicitly to develop culture histories of specific regions. The primary objective within archaeology became the definition of time-space systematics. Cultural change was recognized in the archaeological record but was processed only in the context of refining the chronology. The overriding concern archaeologists in this period was the acquisition of more and better facts to serve the master of chronology; description was the only safe bet as the development of theories and hypotheses for cultural change or process was associated with armchair speculation and thus deemed beyond the bounds of the professional archaeologist.

The discipline during this period also continued its trend towards professionalization. The establishment of the Society for American Archaeology in 1935 grew out of such a new context. In its original charter, the Society stated its aims were:

to stimulate scientific research in archeology of the New World by creating closer professional relations among archaeologists and between them and others interested in American archaeology; guiding on request the research work of amateurs; advocating the conservation of archaeological data and furthering the control or elimination of

⁸⁰ A. Kidder, *An Introduction to the Study of Southwestern Archaeology With a Preliminary Account of the Excavations at Pecos*. New Haven: Pub. for the Department of Archaeology, Phillips Academy, Andover, Mass, 1924.

⁸¹ The Method proposed a schema by which to classify artifacts based on prescribed formal criteria and was applied to develop a chronological sequence for the entire eastern half of the United States. Its major proponent was William McKern. Willey and Sabloff (1993) provide a very good summary of the Method in their *History of American Archaeology*.

commercialization of archaeological objects; and promoting a more rational public appreciation of the aims and limitations of archaeological research.⁸²

One can discern in this charter the concern with establishing archaeology as a science with common goals and aspirations and, at the same time, with dampening the public expectations that were fueled by the rampant speculations of earlier generations of archaeologists.

The huge increase in archaeological activity during the 1930s -- largely attributable to the initiatives of the Works Progress Administration (WPA) and the Civilian Conservation Corps (CCC) -- spurred the trend toward professionalization of the field. Moreover, it also necessitated a more standardized approach to the practice of archaeology, which in the absence of an intellectual environment receptive to anything other than descriptive analyses of the archaeological record, settled on the collective goal of refining chronologies and building culture histories. In this intellectual climate, archaeology dedicated itself primarily to the excavation, description, and classification of artifacts, and did not generate larger questions of explaining change or process. Classifications were used to order the archaeological record into a chronological sequence that could then be applied, tested for its veracity at other sites, and adjusted as appropriate. This approach to archaeology in the 1930s was self-validating because it was empirically successful, or, as one historian of archaeology has noted, “[c]ulture history worked; other proposals, however attractive, were not operational.”⁸³

In this discussion of the development of culture-historical archaeology in the United States, the concept of culture and how it was conceived and used must be addressed. Culture as a concept is fundamental to both archaeology and anthropology. One of the first definitions for culture in an anthropological context was provided by E.B. Tylor in 1871 as “that complex whole which includes knowledge, belief, art, morals, law, custom and any other capabilities and

⁸² 1935 National Anthropological Archives at the National Museum of Natural History in Washington, DC.

⁸³ Dunnell, “Five Decades of American Archaeology,” 30.

habits acquired by man as a member of society.”⁸⁴ This conception of culture by Tylor was reshaped during the Boasian period, eventually coming to be seen in terms of a shared collection of common traits that is unique to particular groups with distinct ethnic identities. This was a view of culture that was consistent with the Boasian principles of historical particularism. These concepts, however, were of limited use when applied to the static archaeological record, in which the ethnicity of prehistoric groups was invisible. How should archaeologists reconcile the link between the artifact and the culture it represented?

The particularistic view of culture promoted by the Boasians posed a dilemma for archaeologists at the turn of the twentieth century. From a Boasian perspective, artifacts were singular material expressions of a living and unique set of cultural behaviors and traits. In order to fully understand the artifact, one must understand the particular and living circumstances from which it was generated. Divorced from that living context, artifacts were devoid of meaning. Meaning, then, had to be constructed and assigned. For archaeologists this meant providing a context that was a substitute for the vanished living system. At the turn of the century, archaeologists turned to the areas in which they could successfully contextualize artifacts within the archaeological record, time and space. As chronologies and classification systems became more complex in the early decades of the century, so did the idea of artifacts as a reflection of this new context. Artifacts became the material substitute for culture in archaeology, giving rise to the notion of the archaeological culture. Classifications based on artifact types reinforced the notion of an archaeological culture as the material expression or manifestation of dynamic cultural behaviors; artifact types, not cultures, were routinely shown as changing through time. By extension, archaeological assemblages or groups of related artifacts in time and space thus came to be seen as expressions of particular cultural groups, i.e., ethnic groups. A change in the relative frequency of artifact types in this construct, therefore, was interpreted variously in ways that required outside influence such as migration or diffusion.

⁸⁴ Tylor, *Primitive Culture*, 1.

V. Gordon Childe was one of the first archaeologists to attempt an explicit definition of an archaeological culture in 1929. For Childe an archaeological culture included:

certain types of remains -- pots, implements, ornaments, burial rites and house forms -- constantly recurring together; such a complex of associated traits we shall call a "cultural group" or just a "culture." We assume that such a complex is the material expression of what today we would call 'a people.'⁸⁵

Childe also used this notion of an archaeological culture to advocate the development of culture histories as the prime directive of archaeology. Childe's work was hugely influential in Europe. Yet, the concept of an archaeological culture, albeit ill-defined, had been alive and well in the United States many years before Childe published *The Danube in Prehistory*.⁸⁶ Nels Nelson and Alfred Kidder both relied implicitly on the notion of an archaeological culture in the development of their chronologies of the American Southwest between 1913 and 1927. This suggests that Childe's definition may have served to reinforce rather than spur the culture-historical approach within American archaeology.

By the late 1930s, the concept of an archaeological culture was well established and culture history was the most popular approach among American archaeologists. This led to a surprising amount of homogeneity within the discipline; archaeology was focused on a common vision, i.e., to identify and describe the chronologies and histories of prehistoric cultures. As Dunnell has described this period, "there was only one way to do archaeology, culture history" and virtually no one bucked the trend.⁸⁷ A perusal of common professional journals (such as *American Antiquity* and *American Anthropologist*) during this period reveals a primary concern with stylistic analyses of artifact assemblages that allowed creation of chronological and typological schemas. And, although American archaeologists searched for patterns in the archaeological record that would support their chronologies, typologies, and classifications, they rarely broadened that research to issues of cultural change or the dynamics inherent in the

⁸⁵ V.G.Childe, *The Danube in Prehistory* (Oxford: Oxford University Press, 1929), v-vi.

⁸⁶ Trigger, *A History of Archaeological Thought*, 2006.

⁸⁷ Dunnell, "Five Decades of American Archaeology," 31.

creation of the archaeological record. Discussions of archaeological theory and, for that matter, methodology, were noticeably absent. This window of disciplinary unity was short-lived, however, as dissatisfaction with the predominance of culture history began to emerge.

3.3 Cracks in the Culture History Façade (Post World War II-1960)

The foundations for what would later be termed the New Archaeology in United States were laid in the years during and after the Second World War. The homogeneity within American archaeology that peaked during the 1930s had begun to show evidence of weakness. Archaeologists who had been busy and content with the development of chronologies and typologies began to exhaust the lines of research that had sustained them previously. As the body of archaeological data grew exponentially due to large public works activities during the 1930s, questions that had been viewed previously as beyond the purview of archaeology were resurrected. The two decades from 1940 to 1960 are seen by most historians of American archaeology as being a period of great transition. Willey and Sabloff see it firmly entrenched in the culture-historical period (or what they term the Classificatory-Historical Period) but recognize the rumblings of dissatisfaction in newfound concerns for addressing context, function, settlement patterns, and ecological adaptation in the archaeological record.⁸⁸ Dunnell emphasizes that the divergence from cultural anthropology that emerged in American archaeology in the 1940s and 1950s forced a reexamination of the aims of the discipline for many American archaeologists.⁸⁹ Moreover, Trigger notes the confluence of emerging concerns for functional and behavioral questions with the refinement of methodology and techniques. Trigger identifies particularly the impact of absolute dating techniques, such as radiocarbon, as the basis for moving archaeology beyond the sterile emphasis on time and space. Absolute

⁸⁸ Willey and Sabloff, *A History of American Archaeology*, 152-153.

⁸⁹ Dunnell "Five Decades of American Archaeology," 33-38.

dating techniques allowed for more reliable and effective control over chronology, freeing archaeologists to pursue more substantive questions than had previously been feasible.⁹⁰

The first inklings of dissatisfaction with the state of archaeology came not from within archaeology, but from anthropologists who had had occasion or interest to be involved in archaeological fieldwork. There had been murmurs such as William Strong's exhortation that archaeology adopt a more theoretical stance akin to that of ethnology and Julian Steward's and Frank Setzler's relatively gentle reproach of archaeology for its failure to address issues of cultural change.⁹¹ Three watershed events best typify the shifts that were occurring in archaeology, events that were formative in their impact later on Lewis Binford and the New Archaeologists of the 1960s.⁹²

The first was the publication in 1940 of Clyde Kluckhohn's (1905-1960) severe admonition of archaeology in his essay "The Conceptual Structure in Middle American Studies."⁹³ Where Strong, Steward, and Setzler had respectfully taken American archaeology to task for its myopic vision, Kluckhohn excoriated the field and its practitioners for what he argued were gross oversights and overreliance on a historical approach to prehistory. In one passage Kluckhohn observed that:

I should like to record an overwhelming impression that many students in this field are but slightly reformed antiquarians. To one who is a layman in these highly specialized realms there seems a great deal of obsessive wallowing in detail of and for itself.⁹⁴

⁹⁰ Trigger, *A History of Archaeological Thought*, 361-369.

⁹¹ See W.D. Strong, "Anthropological Theory and Archaeological Fact," in *Essays in Anthropology*, ed. R. Lowie (University of California Press: Berkeley, 1936), 359-368, and J. Steward and Frank M. Setzler, "Function and Configuration in Archaeology," *American Antiquity*, Vol. 4, No. 1 (Jul., 1938): 4-10.

⁹² There are numerous histories of American archaeology that provide a detailed examination of archaeology during this period. Willey and Sabloff (1993) is the most comprehensive. This study selectively focuses on a few works that encapsulate the many and varied changes occurring in American archaeology during the postwar years.

⁹³ C. Kluckhohn, "The Conceptual Structure in Middle American Studies," in *The Maya and Their Neighbors: Essays on Middle American Anthropology and Archaeology*, ed. C.L. Hay, New York: Dover Publications, 1977: 41-51.

⁹⁴ *Ibid.*, 42.

Kluckhohn maintained that archaeology could contribute to an understanding of the past through a more scientific and comparative approach one that was more anthropological in nature and that addressed larger issues of cultural development and process. Anything less was purely pedantic. He also questioned its practical value. In his own words, Kluckhohn asked “[d]o researches which require large funds for their support require no social justification other than that of quenching certain thirsts for knowledge on the part of a relatively small number of citizens?”⁹⁵

Kluckhohn’s critique was a jolt to American archaeology and, although it did not have a large impact in the short term, it did crack the door for those whose dissatisfaction with the field was growing. Kluckhohn’s continuing influence on the field came in the form of one his students, Walter W. Taylor (1925-1983). In 1943 Kluckhohn supervised the completion of Taylor’s dissertation at Harvard titled “The Study of Archaeology: a Dialectic, Practical, and Critical Discussion with Special Reference to American Archaeology and Conjunctive Approach”. This publication, which Taylor later published in 1948 as *A Study of Archaeology*, constituted the second pivotal event in American archaeology between 1940 and 1960. Taylor’s critique of American archaeology went far beyond that of Kluckhohn’s, not only in tone but also in proposing a way forward for the discipline, which he termed a “conjunctive approach.”⁹⁶

Taylor, like his mentor, believed that culture-historical archaeology had failed in its objective to reconstruct prehistory, substituting instead a recitation of trait lists and chronologies. Taylor saw archaeology as a field that should be both history and anthropology. He argued that archaeology had become mired in the former and had allowed the latter to languish. Taylor saw the historical approach to archaeology, that is, the concern for typology and chronology, as a

⁹⁵ Ibid., 43.

⁹⁶ W. Taylor, *A Study of Archeology*, (Memoirs of The American Association for Anthropology (69), 1948).

first step in a comprehensive approach to understanding the past. He opined that most archaeologists had, in fact, accomplished the historical task, but avoidance of theory-development and an aversion to interpretation of past behavior from the archaeological record had prevented archaeologists from tackling the next step. As Taylor noted:

Why has revision become such a bugbear to archaeologists? Other disciplines are constantly reworking their hypotheses and formulating new ones upon which to proceed with further research. When these are found to demand modification and change they are altered. Why should archaeology assume the pretentious burden of infallibility?⁹⁷

In response, Taylor offered the conjunctive approach, which he described in terms of a sequence of steps that archaeologists should employ. First, archaeologists should address chronology; at this task American archaeologists had become quite adept. Beyond chronology, however, Taylor proposed that archaeologists engage in an ethnographic inventory of a site, compiling information on every conceivable aspect of behavior that could be inferred. This step would require a complete revision in the activities of archaeologists in terms of excavation strategy. Hitherto, archaeologists had ignored materials that were not useful in developing chronologies (such as floral and faunal remains, which could provide evidence of past subsistence strategies or environmental conditions). Lastly, Taylor proposed that archaeologists engage in historical considerations of how to account for cultural change, a radical proposal for most archaeologists who equated such lines of research with the old style evolutionary archaeology.

Taylor's conjunctive approach never caught on with his peers at the time his book was published, but it did presage the coming critiques by the New Archaeology. There were several likely reasons for Taylor's failure to gain traction with his approach. His polemical tone and tendency to attack individuals was off-putting for many archaeologists. More significant, however, was Taylor's failure to break away from the conceptualization of culture as an ideational concept, which was a cornerstone of culture history. Taylor was no different from

⁹⁷ Ibid., 157.

many of his contemporaries in assuming that artifacts were material expressions of cultural ideas and behaviors and, as such, archaeology was essentially a study of particulars. Thus, despite his entreaties to the contrary, Taylor remained boxed in by many of the same intellectual traps that held other archaeologists of the time.

Another reason for Taylor's inability to sell his peers on the conjunctive approach may have been his oversight of a significant trend that was beginning to emerge in American archaeology in the 1940s. This trend is best described as a new appreciation for function and context in the archaeological record, which represents the final watershed movement of this transitional period. Function and context refer to aspects of artifacts related to their use, purpose, provenance, and association. Functional and contextual concerns in archaeology predate this period considerably, but most early functional analyses were not much more than commonsense exercises by which certain uses were ascribed to artifacts. Explicit attempts to address function within the overall context of artifacts within the archaeological record did not emerge until the 1930s, and even then there is no one person with whom one can associate this growing trend. Trigger connects the new appreciation for function and context with the growing influence of functionalism within cultural anthropology that was attributed primarily to E.R. Radcliffe-Brown, who was at the University of Chicago throughout much of the 1930s, and to Bronislaw Malinowski, who was at Yale between 1938 and 1942.⁹⁸

The first attempt to synthesize the growing interest in functionalism in American archaeology was provided by John Bennett in 1943. In an essay titled "Recent Developments in the Functional Interpretation of Archaeological Data," Bennett attempted to identify and categorize the various archaeological activities of the preceding few years that represented to him a new direction of interest in the field.⁹⁹ He attributed this new trend to a logical progression in the advancement of the discipline. In his words,

⁹⁸ Trigger, *A History of Archaeological Thought*, 365.

⁹⁹ John Bennett, "Recent Developments in the Functional Interpretation of Archaeological Data," *American Antiquity* 9, no. 2 (1943): 208-219.

The reasons for this transition from specific to generalized statements about archaeological data can be found in the wearing out or exhaustion of the possibilities for interpretation along purely historical-specific lines. Once the temporal sequences in the various areas have been worked out, further contributions on pure chronology and typology become repetitive and somewhat sterile.¹⁰⁰

Bennett saw this trend in terms of a new era that archaeology was entering. He welcomed this new interest that went beyond the traditional interpretations that had characterized the previous few decades. Bennett's synthesis was very influential, encouraging young archaeologists to take up their studies with the explicit objective of functional interpretation.

By the late 1940s, functional analyses were increasingly common in the literature, and in the 1950s archaeologists had become quite adept in their application of functional interpretations, so much so that they spurred reconsideration of previously uncontested areas within the culture-historical framework. One such area was the controversy over the nature of artifact typology as immortalized in the Ford-Spaulding debate.¹⁰¹ The crux of the Ford-Spaulding debate was whether artifact types were artificial constructs or natural categories. James Ford, like many culture-historians, saw artifact types as useful constructs whose worth lay in their ability to delimit chronological sequences within the archaeological record. The adherence to this contention began to erode in the face of new functional concerns as archaeologists broadened their research questions beyond those of chronology. They insisted that artifact types should reflect the intent of those who produced them. A.C. Spaulding, in direct contradiction to Ford, proposed that artifact types reflective of the context and function in which they were made could be revealed through advanced statistical techniques designed to identify natural clusters or categories of artifacts. The debate between the two men dominated

¹⁰⁰ Ibid., 215.

¹⁰¹ Willey and Sabloff, *A History of American Archeology*, 165-169.

much of the early to mid-1950s and typified the shifting sands within the discipline.¹⁰² While a comprehensive treatment of the Ford-Spaulling debate is beyond the scope of this work, it is representative of the emerging tension within archaeology that accelerated rapidly with the rise of the New Archaeology in the 1960s.

Along with this tension within the discipline were some new realities stemming directly from a larger shift in the American social landscape. The post World War II years in the U.S. were characterized by rising affluence, increasing population, and optimism about science. The GI Bill and the rising affluence of the general population generated a substantial wave of students entering American universities. Departments of anthropology experienced this demand, necessitating an increase in the number of faculty and students. At the University of Chicago, for example, the size of the faculty -- which had remained steady throughout the 1930s and 1940s at seven -- doubled in size by 1960, as did its graduate student population, which grew from 44 to over one hundred.¹⁰³ Also, as the population increased in the United States, much of the increase was centered in the Midwest and West. Universities in these regions began to grow rapidly. Students interested in archaeology and anthropology now had choices other than the East coast institutions that had dominated the field up until World War II. Institutions like the University of Chicago and the University of Michigan were in ascendancy, and the balance of power in academia was shifting.

Along with the increasing population and affluence came a national trend in the U.S. that elevated science and scientific activities above those of the traditional humanities. Government support for science expanded considerably in the 1950s. The National Science Foundation, which was founded in 1950 with the explicit charge to maintain America's dominance in science, offered American archaeologists a new source of funding over the

¹⁰² A very good treatment of the Ford-Spaulling debate can be found in M. O'Brien and R. L. Lyman, *James A. Ford and the Growth of Americanist Archaeology* (Columbia: University of Missouri Press, 1998).

¹⁰³ George W. Stocking, Jr. *Anthropology at Chicago: Tradition, Discipline, Department*. (Chicago: Joseph Regenstein Library, University of Chicago, 1980).

traditional patronage of wealthy supporters. The NSF was aided by the passage of the National Defense Education Act of 1958, which held that the “security of the Nation requires the fullest development of the mental resources and technical skills of its young men and women.”¹⁰⁴ For American archaeologists, this began a boom period which promoted a continuing desire to align archaeology with the scientific disciplines so that it qualified for funding. Coupled with this was a new era in global mobility. The strong American economy, the end of hostilities in Europe, and a new age of cheap and rapid transatlantic travel all facilitated a new interest among American archaeologists in collaborating with European colleagues on new avenues of research.

By the end of the 1950s, American archaeology was on the cusp of a major and pivotal shift. The influence of anthropology on archaeology, which had waned in the 1920s and 1930s, was reemerging in new and syncretic ways. A new generation of archaeologists, much greater in number than in previous decades, were completing its studies, and many were emerging into a new sociocultural landscape that valued science and pushed the boundaries of existing knowledge. American archaeology with its newfound affluence also was expanding its research agendas beyond the traditional regions of North, Middle and South America. More American archaeologists were seeking new research agendas across the Atlantic. A new optimism about science renewed an aspiration in American archaeology to address the questions that had been deemed premature in the absence of fully realized culture histories. It was in this climate that Lewis Binford was immersed as a young graduate student in the late 1950s. In many respects, Binford’s work was a logical extension of the trends within archaeology in that period. His crafting of the New Archaeology and, most importantly, his debate with Bordes, have their origins in the larger context of American archaeology and its unique history and trajectory.

In the decades just prior and subsequent to World War II, the culture-historical approach to archaeology in the U.S. was the dominant approach in the field. Two fundamental differences, however, altered the trajectory of the discipline in a way that made archaeology

¹⁰⁴ National Defense Education Act (NDEA) (P.L. 85-864) United States Statutes at Large Vol. 72.

separate yet parallel to that practiced in France. First, culture-historical archaeology in the U.S. was heavily influenced by its association with cultural anthropology, particularly as defined by Franz Boas. Boasian cultural anthropology, with its emphasis on cultural relativism and recognition of the accomplishments of the indigenous native peoples of the Americas, allowed for the notion of cultural change by the same or similar groups through time in a way that was at odds with the understanding of archaeological culture as conceptualized by French and other European archaeologists. Second, the notion of culture in American archaeology developed out of culture area studies in a way that was less personal for most American archaeologists. Given that the overwhelming majority of American archaeologists active in the field were and continue to be of European heritage, the prehistory of the Americas and its native populations was not a highly personal narrative; for American archaeologists it was not 'their' history. This was and is a direct contrast to the reality for French and other European archaeologists. As Lawrence Straus, an eminent Paleolithic archaeologist, notes, "there is simply more emotional distance between most American archaeologists and the objects of their research than may be the case among many European prehistorians working within and attached to their own regions."¹⁰⁵

By the 1940s and 1950s, American archaeology and its own brand of the culture-historical approach had begun to diverge distinctly from that of France. The postwar years in the U.S., characterized by a wave of national optimism fueled by faith in technology and science, saw a rekindling of interest in the systematic application of anthropological and sociological method and theory to past human behavior to understand how prehistoric cultures operated and changed through time.¹⁰⁶ During this period, more and more American archaeologists began to express discontent with the limitations imposed by the culture-historical approach. No longer were they interested in descriptive studies of the deep past, the "what", if

¹⁰⁵ Straus, "American Perspectives on the European Upper Paleolithic? An introduction," 2.

¹⁰⁶ Patterson. *Toward a Social History of Archaeology in the United States*. 1995.

you will. Rather they explicitly advocated the application of social scientific method and theory to understanding the how and the why of human prehistory.

In the 1950s Binford emerged as a student and young professional. Binford and a few of his peers strongly advocated and promoted archaeology as a behavioral science with explanation of past cultural processes as the overarching goal. They explicitly rejected the ideational notion of culture, arguing instead that culture was a complex constellation of systems subject to a variety of processes that can affect change through time; in other words, culture is more a means of adaptation for humans not merely as individuals, but as a population. Cultural phenomena and artifacts, when viewed from this perspective, were not identified with the norms and ideas specific and attributable to a human social ethnic group or individual human agent. Thus variability in archaeological assemblages did not trigger a presumption of ethnic or social differences as it did in France, or for that matter in early American archaeology. Indeed, this type of presumption was antithetical to an explanatory model, with its emphasis on developing and testing alternative hypotheses, which came to be favored by Binford and other American archaeologists.

CHAPTER 4

THE DEBATE

In 1989, Lewis Binford offered a retrospective on significant archaeological controversies, which he prefaced by outlining a tongue-in-cheek “taxonomy of archaeologists.”¹⁰⁷ In his taxonomy, Binford provided a ‘field guide’ to ‘intellectual species’ in archaeology that included the Yippie, the Yuppie, the Guppie, and the Puppie, with an occasional Lollie and Jollie thrown in for good measure. He proceeded in this taxonomy to describe Yippies as navel-gazing postmodernists tilting at empiricist windmills, Yuppies as techno-geeks seeking enlightenment through the next best gadget, Guppies as the elder (or Canadian) sages who are the great synthesizers of material--filling up passports and notebooks with vast amounts of accumulated knowledge, Puppies as the proxy pawns of their affiliated Guppies, and Lollies as the jaunty appropriators and descriptors of clever monikers for interesting human conditions. Binford classed himself as a Jollie, the rare observer who sees these differing intellectual taxa in their true light.

While decidedly humorous, Binford clearly had a serious point and one which is necessary to understand before the details of his debate with Bordes can be examined effectively. For Binford, an “active, serious, and progressive scientific discipline is by nature disputatious.”¹⁰⁸ Even later in his career, Binford was skeptical that archaeology had achieved the requisite level of systematic give-and-take of a truly scientific field, instead concerning itself with pointless exchanges that failed to advance the discipline or result in a clearly defined

¹⁰⁷ L. Binford, *Debating Archaeology. Studies in archaeology* (San Diego: Academic Press, 1989), 5-10.

¹⁰⁸ *Ibid.*, 10.

resolution. Debate was characterized as an *ad hoc* exercise with no deliberate thought or planning; this was not so for Binford. Binford was a strategist, he undertook debate with tactical precision and an eye for engaging Guppies in substantive discussions often on matters of import beyond that of the putative subject matter. This was certainly the case in his debate with Bordes. As Binford noted:

It is interesting that most who read the literature of this controversy saw the argument in terms of a specific interpretation for a specific data set, the Mousterian. I always saw this as the key test case for learning about the limitation of our methodologies. It also provided the provocative “anomaly” described by Bordes, the investigation of which had the potential of increasing not only our skill as archaeologists, but also our knowledge of the past.¹⁰⁹

At least for Binford, then, this was no mere superficial disagreement over whether Mousterian lithic variability is attributable to different Neanderthal tribes or to different functional activities. There were larger stakes at risk: how was archaeology to tackle the questions of culture change and process in the deep past?

4.1 Bordes' Taxonomy

The recognition of variability in Mousterian tools predates Bordes' work by almost a century. Lartet and Christy noted the generally sloppy, less sophisticated nature of the tools at the Mousterian site of Combe Grenal in comparison with those of the site of Le Moustier.¹¹⁰ Mortillet observed the predominance of side scrapers in some Mousterian sites, but did not identify the high variability of tools within Mousterian sites.¹¹¹ Not until 1920 did Denis Peyrony, the prodigious excavator and protégée of Henri Breuil, offer the first systematic attempt to

¹⁰⁹ L. Binford, *Working at Archaeology* (New York: Academic Press, 1983), 66.

¹¹⁰ E. Lartet and H. Christy, “Cavernes du Périgord: Objets graves et sculptés des temps préhistoriques dans l'Europe Occidentale,” *Revue Archéologique* 1 (1864): 233-270.

¹¹¹ G. de Mortillet, “Classification des ages de la pierre. Classification des diverses périodes de l'age de la pierre.” *Congres International d'Anthropologie et d'Archéologie Préhistorique C.R. 6 (Brussels)* (1872): 432-435.

classify Mousterian tool assemblages.¹¹² Peyrony described two organizational classes of Mousterian assemblages: the Typical Mousterian and the Mousterian of Acheulean Tradition (MTA). The former was characterized by the high frequencies of scrapers (*racloirs*) and the latter by the presence of hand-axes reminiscent of the earlier industries known as the Acheulean. Peyrony was also the first to recognize the interassemblage variability (that is, observable differences in the nature and types of tools *between* associated collections of artifacts within a site) in the Mousterian, although he made no real attempt to explain it. Peyrony's simple two-category classification of Mousterian assemblages was widely accepted and persisted throughout the two decades just prior and subsequent to WWII.

Bordes was first exposed to the issue of Mousterian lithic variability at a very young age. He was a native of the Dordogne region, an area central to the study of the European Paleolithic, and as a boy he became acquainted with Peyrony. Influenced by Peyrony, Bordes became avidly interested in French prehistory, undertaking his first excavations in the region at age 15. Bordes' studies were interrupted by the outbreak of the war, but by 1946 he was again excavating Paleolithic sites under the direction of Raymond Vaufrey and Jean Piveteau.¹¹³ In collaboration with his friend Maurice Bourgon and his wife Denise de Sonneville-Bordes, Bordes began in the late 1940s to explore the question of Mousterian lithic variability. For Bordes, the typologies that had been developed for the Mousterian beginning with Lartet disadvantaged any attempt to study the issue of variability given their qualitative nature. Bordes' definitive contribution to the study of the French Paleolithic was not merely the refinement of a typology for Mousterian tools, but rather the development of a taxonomy for tool industries that was largely (though not entirely, as we shall see) independent of the subjective criteria that had bedeviled earlier researchers including his mentors Breuil and Peyrony.

¹¹² D. Peyrony, "Le Moustérien – ses faciès," *Comptes Rendus de l'Association Française pour l'Avancement des Sciences*, 44th Session: Strasbourg (1920): 496-497.

¹¹³ A. Jelinek "Obituary: François Bordes, 1919-1981," *American Antiquity* 47, no.4 (1982): 785-792.

Bordes' taxonomy of Mousterian tool industries rested on the premise that the old *fossiles directeur* approach to classifying and typing Mousterian tools was deficient and failed to account for the total range of tools that were actually present. Bordes sought first to standardize the notion of tool type in Mousterian assemblages. Where Peyrony, and earlier Breuil, recognized only a few characteristic Mousterian tool types, Bordes expanded the tool list initially to 48 and eventually to 63 distinct types. Bordes' *liste typologique* (see Table 4.1) explicitly identified morphological and technological characteristics of the tools in the determination of type. It was Bordes' application of his typology, however, that was the true scholarly advance. By calculating the cumulative percentages of different tool types within Mousterian tool assemblages, Bordes developed a series of technological indices that could be used to organize assemblages within distinct industrial groups. One of the most important of these technological indices was the *racloir* (side-scraper) index. In what was at the time a fairly complex statistical approach, Bordes charted the overall frequency distribution of *racloirs* in Mousterian tool assemblages and discovered a modal pattern indicating that Mousterian assemblages fell into three distinct groups.¹¹⁴ Bordes' subsequent refinement of this statistical technique, now known simply as *la méthode Bordes*, led to the eventual recognition of four primary Mousterian groups or *facies*. In Bordes' own words:

It is, therefore, evident that if we wished to stop using the "wet finger" approach in Prehistory we must consider the entire industry not merely some parts considered rightly or wrongly as "characteristic." But numbers are a big problem. When one considers two poor layers, each giving, say, 100 to 150 objects, it is still possible to spread the objects over two contiguous tables, to classify types and compare them visually. But when it comes to layers of several thousand pieces, something which is not uncommon in the French Southwest, the task becomes impossible, and one falls back on "impressions." While this may not be worthless for very experienced prehistorians, it may not mean much most of the time. Experience has taught us that in an industry well-carved, beautiful objects strike the sight and memory and appear to play a much more important role than they actually play. How many Mousterian assemblages have been classified as "traditional acheulean" because of the presence of one or two fine bifaces lost in hundreds of other pieces and that, perhaps, were objects collected by

¹¹⁴ F. Bordes "Essaie de classification des industries 'moustériennes.'" *Bulletin de la Société Préhistorique Française* 50 (1953): 457-466. Bordes initially identified the Denticulate, Typical, and Charentian groups, but later added the Mousterian of Acheulean (MTA) as a fourth *facies*.

Table 4.1. Bordes' Standard Type List, from *Leçons sur le Paléolithique* (1984)

LISTE TYPOLOGIQUE DES OUTILS SUR ECLAT ET SUR LAME
DU PALEOLITHIQUE INFERIEUR ET MOYEN

d'après F. Bordes

- | | |
|-------------------------------------|--|
| 1. Eclat levallois typique | 33. Burin atypique |
| 2. Eclat levallois atypique | 34. Perçoir typique |
| 3. Pointe levallois | 35. Perçoir atypique |
| 4. Pointe levallois retouchée | 36. Couteau à dos typique |
| 5. Pointe pseudo-levallois | 37. Couteau à dos atypique |
| 6. Pointe moustérienne | 38. Couteau à dos naturel |
| 7. Pointe moustérienne allongée | 39. Raclette |
| 8. Limace | 40. Eclat tronqué |
| 9. Racloir simple droit | 41. Tranchet moustérien |
| 10. Racloir simple convexe | 42. Encoche |
| 11. Racloir simple concave | 43. Denticulé |
| 12. Racloir double droit | 44. Bec burinant alterne |
| 13. Racloir double droit-convexe | 45. Retouche sur face plane |
| 14. Racloir double droit-concave | 46-47. Retouche abrupte
alterne épaisse |
| 15. Racloir double biconvexe | 48-49. Retouche abrupte
alterne mince |
| 16. Racloir double biconcave | 50. Retouche biface |
| 17. Racloir double convexe- concave | 51. Pointe de Tayac |
| 18. Racloir convergent droit | 52. Triangle à encoche |
| 19. Racloir convergent convexe | 53. Pseudo-microburin |
| 20. Racloir convergent concave | 54. Encoche en bout |
| 21. Racloir déjété | 55. Hachoir |
| 22. Racloir transversal droit | 56. Rabot |
| 23. Racloir transversal convexe | 57. Pointe pédonculée |
| 24. Racloir transversal concave | 58. Outil pédonculé |
| 25. Racloir sur face plane | 59. Chopper |
| 26. Racloir à retouche abrupte | 60. Chopper inverse |
| 27. Racloir à dos aminci | 61. Chopping-tool |
| 28. Racloir à retouche biface | 62. Divers |
| 29. Racloir à retouche alterne | 63. Pointe foliacée biface |
| 30. Grattoir typique | |
| 31. Grattoir atypique | |
| 32. Burin typique | |

Moustériens, or came from the top or bottom of the layer and did not really belong with the industry studied?

It occurred to me around 1946 the need to take into account the number and percentage of objects. I was not the first to calculate percentages and in the literature Bouysonnie, in particular, has often published the percentage of tools, but he never made systematic use of them for comparison. It was appropriate to systematize this use, and to make a precise critique of the concept of type.¹¹⁵

Bordes eventually finalized his taxonomy as follows:

- 1) Typical Mousterian (characterized by low percentage of transverse scrapers, absence or very low percentage of Quina-type scrapers, absence or rarity of true handaxes and backed knives, and variable percentage of Levallois flaking);
- 2) Charentian Mousterian (subdivided into a) Quina type with very high percentage of side scrapers and very low percentage of Levallois flaking; and (b) Ferrassie type with very high percentage of side scrapers, and very high percentage of Levallois flaking);
- 3) Denticulate Mousterian (characterized by low/very low percentage of side scrapers, often 'degenerate', and high/very high percentage of denticulates and notches); and
- 4) Mousterian of Acheulean or MTA (characterized by the presence of cordiform handaxes and can be subdivided into two additional groups (MTA sub-type A and sub-type B) based on the frequency of scrapers).¹¹⁶

Bordes' taxonomy was widely recognized for its utility and its emphasis on quantitative data (assumed to be objective in nature) as opposed to qualitative data (assumed to be subjective). In reality, however, Bordes' approach was not merely a simple quantitative scheme. He routinely used qualitative criteria to refine his taxonomy. The entire MTA *facies*, for example, was defined purely in terms of the presence of the distinctive hand-axe, which bore a resemblance to the earlier Acheulean tool industry of the Lower Paleolithic. The use of the hand-axe to define the MTA was made solely on the grounds of the tool as a purported

¹¹⁵ Bordes, *Leçons sur le Paléolithique*, 129.

¹¹⁶ F. Bordes and D. de Sonneville-Bordes, "The Significance of Variability in Paleolithic Assemblages," *World Archaeology* 2, no. 1 (1970): 62-64.

diagnostic type, not because of its relative frequency in the assemblage or its role in any of the technological indices created by Bordes. Indeed, some scholars have equated it with the classic *fossiles directeur* as employed by Mortillet, Breuil, and Peyrony.¹¹⁷ Also, Bordes routinely employed qualitative criteria in the formulation of his *liste typologique*, noting at one point in a rebuttal to Binford regarding the interpretation of a functional distinction between typical and atypical burins that he “had distinguished one from the other purely on qualitative criteria.”¹¹⁸

By the late 1950s, Bordes’ method was widely adopted and his taxonomy of Mousterian tool industries became the standard in Paleolithic archaeology. The taxonomy has demonstrated remarkable longevity. Even as statistical research in archaeology has progressed far beyond that of Bordes’ method, his taxonomy has remained the most accepted convention in describing the tool industries of the European Middle Paleolithic. But it was not Bordes’ method that proved to be the flashpoint in his debate with Binford; it was his early and virtually unchanging interpretation of the variability between his taxonomic units.

4.2 Bordes’ Interpretation of Mousterian Lithic Variability

Bordes first hinted at a cultural or ethnic interpretation of the variability between his taxonomic categories as early as 1953. In the article, “Levalloisien et Moustérien” in the *Bulletin de la Société Préhistorique Française*, Bordes concludes that the Mousterian tool assemblages are related to a distinct ‘way of life’, although he did not rule out seasonal variation.¹¹⁹ Later that same year in his seminal article, “Essai de Classification des Industries ‘Mousteriennes’” also in the *Bulletin de la Société Préhistorique Française*, Bordes began to use the term

¹¹⁷ P. Mellars *The Neanderthal Legacy: An Archaeological Perspective from Western Europe* (Princeton, N.J.: Princeton University Press, 1996), 172.

¹¹⁸ Bordes and Sonnevile-Bordes, “The Significance of Variability in Paleolithic Assemblages,” 67.

¹¹⁹ F. Bordes. “Levalloisien et Moustérien,” *Bulletin de la Société préhistorique française*, n° 4, (1953): 226-235.

'traditions' when referring to the different Mousterian assemblages.¹²⁰ By the early 1960s, Bordes routinely used the term 'tribe' to refer to the different cultural groups he posited as the makers of the different Mousterian tool industries. Moreover, by that time he had clearly and definitively concluded that the evidence best supported a cultural explanation, stating that "the existence of different cultures within the Mousterian complex appears to be an established fact."¹²¹

Implicit in Bordes' interpretation was a uniquely French mindset that routinely saw the prehistoric past as a direct extension of history and historical processes. Bordes, in other words, equated Mousterian *facies* with cohesive groups that shared some sense of cultural identity. To him, cultural groups in historical contexts were understood to be conservative, reacting to external change through the historical processes of migration or diffusion. Supporting this view was an assumption by Bordes that the Mousterian *facies* he identified in his taxonomy were reflective of some tangible prehistoric reality and that those *facies* had some inherent cultural meaning in the past. This assumption is detectable in Bordes' language when he routinely substitutes the term 'cultural groups' when referring to the different Mousterian taxa or uses the taxa names as a proper noun (such as Quina Mousterian woman or Charentian Mousterian man), seemingly unconcerned that those taxa were categories of his own making in the twentieth century. It was this casual and unquestioning acceptance of a historical explanation, or one might even say historical paradigm, that so strongly differentiated him from Binford.

4.3 A Frenchman in America

In the early 1950s, Bordes was unknown outside the insular world of Paleolithic researchers in Europe. Most U.S. archaeologists were completely unfamiliar with him and his work on the Mousterian. Old World archaeologists in the U.S. at that time were a select group

¹²⁰ Bordes, "Essaie de classification des industries 'moustériennes," 465.

¹²¹ Bordes, "Mousterian Cultures in France," 807.

who had the independent means to fund their work abroad, and knowledge of scholarship in Europe tended to be filtered through them to the larger U.S. archaeological community. Bordes' first entrée into U.S. scholarly circles came via an article published by Hallam Movius in 1953.¹²² In it, Movius summarized the current primary trends in Old World Paleolithic archaeology. Movius prominently featured Bordes' work as cutting edge and framed it as a new type of scientific research. Movius' article was very influential in the United States and inspired F. Clark Howell, a senior faculty member at University of Chicago, to invite Bordes to Chicago as a guest lecturer. Bordes first visited America in 1959 and returned on numerous occasions; he died in 1981 on a trip to Arizona. Along with other Old World archaeology luminaries such as J. Desmond Clark and Alberto Blanc, Bordes became a regular visitor to Chicago. It was during his first trip to Chicago in 1959 that Bordes first met Binford. Binford recalled later that his debate with Bordes began when he was hired at Chicago as a junior faculty member in 1960.¹²³ As a faculty member involved in a team-taught interactive course addressing the human career from its earliest beginnings, Binford routinely sparred with the more senior faculty as well as with visitors like Bordes over the best means to excavate and interpret the archaeological record. It was in the context of that course that Binford rekindled his interest in Old World archaeology.

Bordes thrived upon and was stimulated by his experiences in the United States. To address his broader audience, he began to publish regularly in English. As an extension of his guest lectures, he contributed an article to University of Chicago's publication in celebration of the Darwin Centennial, *The Evolution of Man*, in 1959. In 1961 he published a general article in *Science* on his work on the Mousterian problem in France. The article was widely read and cited. He followed up that article with another contributed article in *Studies in Prehistory* in 1966 and in 1971 contributed to Colin Renfrew's edited collection *The Explanation of Culture Change*.

¹²² Movius, "Old World Prehistory," 1953.

¹²³ C. Renfrew, "An Interview with Lewis Binford," *Current Anthropology* 28, no. 5 (Dec., 1987): 687.

Until his death in 1981, Bordes published over a dozen articles and two books in English. His two books, *The Old Stone Age* in 1968 and *The Tale of Two Caves* in 1972, are arguably some of the most widely read publications on the Paleolithic in English.

4.4 An American in France

When Lewis Binford was a young graduate student, he expressed an interest in Old World archaeology, but his advisors actively discouraged him from pursuing research outside the New World. As he described it later, his lack of personal wealth prohibited him from pursuing his interests in the Old World; thus, he became a New World archaeologist by default.¹²⁴ But his interest in Old World archaeology never waned. Upon his appointment to the faculty at University of Chicago, where the focus was almost exclusively Old World, Binford renewed his interest.

Binford, like many other U.S. archaeologists, first learned of Bordes from Movius' 1953 article and recognized in Bordes' work an opportunity for research that converged with his own emerging sense that archaeology suffered from a paucity of thought and interpretation and not data. By the late 1950s Binford was finishing up his studies at University of Michigan and had come to the realization that no amount of data or chronological or typological refinement would be enough to engage and address the larger questions in archaeology related to culture change and process. The problem for Binford was "not how to look but how to think."¹²⁵ What intrigued Binford about Bordes' work was that Bordes had systematically applied a new methodology that resulted in a complete revision of the conventional understanding of the Mousterian. Prior to Bordes, most Paleolithic researchers clearly assumed that culture always sprang from earlier cultural forms in a linear, evolutionary fashion.¹²⁶ Bordes' work debunked that assumption by showing that the different *facies* of the Mousterian as defined in his taxonomy demonstrated no

¹²⁴ See Sabloff, *Conversations with Lew Binford*, 25 and Renfrew, "An Interview with Lewis Binford," 688.

¹²⁵ Sabloff, *Conversations with Lew Binford*, 26.

¹²⁶ This was a standard interpretation in Lartet's, Mortillet's and Breuil's work.

such directional sequence. From Binford's perspective, Bordes had achieved something that no one else in archaeology had done strictly using archaeological material: he had given the archaeological record an independent voice, one free from speculation and tautological design, and it had answered in an unexpected way. Bordes had shown objectively and definitively that the archaeological record did not support an evolutionary, linear interpretation of the Middle Paleolithic. This was exactly the kind of work Binford hoped to do: pose a question and devise a means to test that question against the existing record.

Binford first met Bordes in 1959 during Bordes' first visit to America and, by his own account, "immediately started arguing with him."¹²⁷ Binford and Bordes struck up a lasting relationship that persisted even in the face of their public debate. From the beginning, Binford thought that Bordes' interpretation of the variability within the Mousterian was insufficient, although he acknowledged that Bordes' model of migrating cultures was logically derived. In 1964, Binford acquired funding and an invitation from Bordes to visit his lab at the University of Bordeaux; it was the first time that Binford had seen the Mousterian assemblages in their entirety. Binford commenced some advanced computer-aided statistics on Bordes' original tabulations and began work with his wife Sally on interpreting the Mousterian tools from the site of Jabrud in Syria. Binford shared some of that early computer work with Bordes, who subsequently invited him and Sally to excavate at Combe Grenal, one of the most important Mousterian sites in France. The Binfords undertook excavation at Combe Grenal in 1966 and then again in 1968. Binford collected reams of data and ran countless statistics on the Combe Grenal data, but the intellectual payoff never materialized; the data never spoke to him. It was time for a new direction.

4.5 Binford and the Rise of the New Archaeology

Although Bordes and Binford had engaged in a casual and friendly debate from their initial meeting in 1959, it was not until 1966 that Binford publicly challenged Bordes in print. To

¹²⁷ Sabloff, *Conversations with Lew Binford*, 28.

understand this first challenge, however, it is necessary to understand how Binford arrived at his intellectual positions. This requires looking into Binford's increasing dissatisfaction with the discipline from his days as a student at the University of North Carolina and the University of Michigan and later as a junior faculty member at the University of Chicago. Binford's challenge to Bordes, in other words, must be understood in the context of the rise of the New Archaeology, of which Binford was a primary architect. The story about the origins of the New Archaeology has achieved an almost mythological status. Several diverse publications published over the past ten years, some favorable to the New Archaeology and others not so, have attempted to trace the history of this movement.¹²⁸ And, while the purpose of this dissertation is not to provide another history of the New Archaeology, it is critical to address briefly Binford's role because it clarifies Binford's challenge to Bordes.

As noted in Chapter 3, U.S. archaeology was by the late 1950s in a period of intense self-appraisal. Publications critical of archaeology such as Walter Taylor's *A Study of Archeology* in 1948 and Joseph Caldwell's "New American Archaeology" in *Science* in 1959 had cracked the façade of culture-historical archaeology. Through these cracks came Binford and the rest of the New archaeologists. It is impossible to know whether the New Archaeology would have been formed with or without Binford's work, but Binford certainly led the parade and for that he is widely credited as the face of the New Archaeology. Binford has routinely published accounts of his intellectual journey.¹²⁹ A recurring element of those recollections is his pervasive sense that archaeology could be so much more than it was in late 1950s. Binford's primary concern with archaeology was that its practitioners had abandoned any semblance of interest in the larger questions of human behavior and the process of culture and cultural change. For Binford, these questions were the heart and soul of archaeology. In 1987

¹²⁸ See O'Brien et al., *Archaeology as a Process Archaeology as Process: Processualism and Its Progeny* (Salt Lake City, UT: University of Utah Press, 2005) and Kehoe, *Land of Prehistory: A Critical History of American Archaeology* (New York: Routledge, 1998) for two distinct treatments of the New Archaeology.

¹²⁹ See Binford 1968, 1972, 1981, 1983, and 1989.

Binford recalled his days at Michigan and the disconnection that he perceived between archaeologists and anthropologists.

LB: The excitement was not in the museum, the excitement was in the department of anthropology, where there was Leslie White [deceased], Elman Service [retired], and Dick Beardsley [deceased]. In the United States, our degrees are in anthropology, not archaeology. No matter what you do with the stones and bones, your basic education and what you test it on is the field of anthropology, so the coursework and the kind of argument that we engaged in with the faculty were very, very stimulating. You went from Angell Hall--which was where the social anthropology and cultural anthropology courses were held--all excited, and you walked back over to the museum, and there were all those people in white coats counting their potsherds! Then you tried to figure out how to relate those two worlds. I mean, here's a world of exciting things and here's a world of mundane, little tasks. The material comes from a common set of conditions--human behavior--so how do we go from one to the other? There was a lot of discussion with colleagues and graduate students and a great deal of encouragement in this kind of thought. There was a kind of tension in the department between the archaeologists and non-archaeologists and the anthropologists, and that was a natural thing to fall into.¹³⁰

By the time that Binford arrived at Chicago in 1960, he had begun to flesh out his ideas about experimenting with different methodologies and analytic strategies in order to get at some of those larger questions in which he was interested. His first attempt to articulate his thoughts and criticism of archaeology came in 1962 when he published his first major article, "Archaeology as Anthropology." The article appeared in *American Antiquity* and was heralded as a clarion call for a new approach to archaeology. In it, Binford attempted to outline the inadequacies of archaeology as it was then conceptualized and practiced and to offer an alternative perspective on the kinds of contributions archaeology could make to the larger field of anthropology.

Archaeology must accept a greater responsibility in the furtherance of the aims of anthropology. Until the tremendous quantities of data which the archaeologist controls are used in the solution of problems dealing with cultural evolution or systemic change, we are not only failing to contribute to the furtherance of the aims of anthropology but retarding the accomplishment of these aims. We as archaeologists have available a wide range of variability and a large sample of

¹³⁰ Renfrew, "Interview with Lewis Binford," 685.

cultural systems. Ethnographers are restricted to the small and formally limited extant cultural systems.

Archaeologists should be among the best qualified to study and directly test hypotheses concerning the process of evolutionary change, particularly processes of change that are relatively slow, or hypotheses that postulate temporal-processual priorities as regards total cultural systems. The lack of theoretical concern and rather naive attempts at explanation which archaeologists currently advance must be modified.

I have suggested certain ways that could be a beginning in this necessary transition to a systemic view of culture, and have set forth a specific argument which hopefully demonstrates the utility of such an approach. The explanatory potential which even this limited and highly specific interpretative approach holds should be clear when problems such as "the spread of an Early Woodland burial cult in the Northeast" (Ritchie 1955), the appearance of the "Burrhead cult" (Waring and Holder 1945) in the Southeast, or the "Hopewell decline" (Griffin 1960) are recalled. It is my opinion that until we as archaeologists begin thinking of our data in terms of total cultural systems, many such prehistoric "enigmas" will remain unexplained. As archaeologists, with the entire span of culture history as our "laboratory," we cannot afford to keep our theoretical heads buried in the sand. We must shoulder our full share of responsibility within anthropology. Such a change could go far in advancing the field of archaeology specifically, and would certainly advance the general field of anthropology.¹³¹

At its core, Binford's argument with traditional archaeologists centered on the concept of culture and how culture was perceived as manifest in the archaeological record. Traditional culture-historical archaeologists from Binford's perspective assumed that culture was the manifestation of shared ideas identifiable across time and space. Binford derisively termed this 'aquatic' culture because of the way it was represented as flowing like water through time and space.¹³²

In his ideas on culture, Binford was greatly influenced by his former professor Leslie White at the University of Chicago. White, a cultural anthropologist, defined culture as a *process*, not a static set of shared traits. In his 1962 article, Binford paraphrased White's definition of culture as an "extrasomatic means of adaptation for the human organism."¹³³ But, it was Binford's systemic and processual notion of culture that was most distinctive and resonant

¹³¹ Binford, "Archaeology as Anthropology," 224.

¹³² Binford, "Archaeological systematics and the study of culture process," *American Antiquity* 31 (1965): 203-210.

¹³³ Binford, "Archaeology as Anthropology," 218.

in archaeology. Binford conceived of culture as a system of interrelated elements and cultural process as the interaction of those elements through time (another name for the New Archaeology was processualism for this reason). The archaeological record was the repository of the material remnants of those systems and, for Binford, only archaeologists were privy to the complete dataset that could explain how cultures changed through time and, more importantly, why they changed.

Binford encouraged his students and peers at Chicago also to take up the banner of processualism and to pursue the new agenda of anthropological archaeology. In this effort Binford was hugely successful. Out of the cohorts of graduate students and junior faculty at Chicago in the early 1960s came many of the best-known and influential names in archaeology, in the second half of the twentieth century, including Kent Flannery, James Hill, William Longacre, Leslie Freeman, Robert Whallon, Patty Jo Watson, and Frank Hole, all of whom shared Binford's frustration with traditional archaeology. This new cadre of like-minded colleagues led by Binford aggressively and loudly challenged the traditionalists on every front and in every regional specialty. By 1965, Binford and his peers were drawing increasing interest as evidenced by a standing-room-only crowd at the first formal symposium to feature their unconventional methods held at the 64th Annual Meeting of the American Anthropological Association in Denver. The all-day event was the first recognition of the synergy in this new approach to archaeology. Around this same time, Binford and other like-minded archaeologists began to embrace the moniker of New Archaeology as the name for their movement, co-opting the term from their critics who had used the term derisively.

As he began to formulate his ideas of culture and cultural process, Binford also began specifically and strategically to seek out instances where traditional approaches to interpretation of the archaeological record had not adequately explained detectable patterning or where traditional methodologies and concerns with chronology were not up to the task of providing the complex analyses necessary to shed additional light on archaeological anomalies. In other

words, he sought instances that seemed to contradict conventional wisdom within the field. One such instance was Bordes' work on interassemblage variability during the Mousterian.

4.6 Binford's Challenge of Bordes

Binford was originally introduced to the problem of Mousterian variability by Sally Schanfield, who later became his wife, during his time at University of Chicago. Schanfield had excavated Mousterian sites in Israel under the direction of F. Clark Howell and had worked in Bordes' laboratory in Bordeaux to analyze the lithic material. Binford was familiar with Bordes and his work, but had been hired at Chicago for his New World experience. Binford's interest was piqued by Schanfield's descriptions of the Jabrud-Shubbabiq material, and he began to research Bordes' work on interassemblage variability in the Mousterian. By his own admission, "the obvious problem was the alternation of industries documented by François Bordes for the Mousterian."¹³⁴ From Binford's experience with New World material, Bordes' explanation that migrating cultures or 'tribes' were responsible for the alternating industries within the Mousterian seemed to make little sense and distorted the idea of culture as a systemic process. Bordes' approach was historical, and Binford was committed to an anthropological approach to archaeology. If culture was an adaptive mechanism that allowed human groups to respond to dynamic and changing external conditions, as Binford assumed, then the better (from his perspective the more anthropological) explanation for alternating tool industries would be that of a single culture responding to variable conditions. Binford was also intrigued by the fact that the Mousterian was the product not of anatomically modern humans but of archaic humans or Neanderthals. As he questioned later, "[c]ould this possibly indicate a major difference between the conditions of organized adaptation of recent and ancient man?"¹³⁵ This was exactly the type of anomaly in the archaeological record that would allow Binford to test the limits of his proposed anthropological approach to archaeology. Binford immersed himself in the Mousterian. With Schanfield, he applied complex factor analyses to the Jabrud-Shubbabiq

¹³⁴ Binford, *An Archaeological Perspective*, 189.

¹³⁵ *Ibid.*, 188.

material. Binford resolved from the beginning that he would use his conceptualization of culture as a dynamic adaptive system, one in which tool types represent functional solutions to a set of ecological conditions, as his organizing principle. Binford refused to accept that observations about and categorizations of the past were in any way reflective of past reality; instead he regarded them strictly as modern constructs. Taxonomic categories of tools, he argued, were categories with meaning in the present and did not necessarily tell us anything about how people in the past categorized their tools. This approach was wholly different from Bordes who, like most French archaeologists, explicitly viewed tools as representative of cultural traditions or manifestations of shared cultural traits, an intrinsically static view of culture, and cultural change as being driven by the same processes discernible in historical contexts. For Bordes, if the artifacts changed and one could not determine an alternative causal factor such as seasonal variation or cultural evolution, then the only reasonable explanation was that of distinct cultures or ways of life.

Binford and Schanfield (later Binford) worked on the Jabrud-Shubbabiq material throughout 1964 and 1965. As a result of the many complex factor analyses, Binford became convinced that Bordes was wrong and that Mousterian variability was the result of different functional activities carried out at different times and for different purposes. By late 1965, Binford and Schanfield had developed a manuscript outlining their findings. Before the article went to print, however, Binford received a grant to visit Bordes' laboratory in Bordeaux during Christmas of 1965. For Binford that visit was transformational. Binford later recounted his recollections of that meeting in great detail.

I began to relax, to talk more forcefully, and pushed on into the critical areas of his interpretation of the variability noted as indicative of different "tribes." Both of our arms began to wave. "But François." "But Binford." We must have argued for more than an hour, our voices getting higher and higher, standing up, sitting down, pacing back and forth, leaning over the charts, big clouds of smoke issuing from his pipe, equally big clouds pouring from my cigarettes. The spectators had begun to move in closer; Mme. Bordes was sitting beside Sally, Pierre had stopped drawing, students no longer made any pretense of working. The sniffing and circling had stopped; we were engaged in a counterpoint duel with each thrust teaching one of us something about the other. It was exhilarating; with each *but*, my respect grew.

Bordes' logical strategies became more complicated, more intricate. With each "round" Bordes was finding out about me and I was finding out about him; we both liked what we found. All of a sudden Bordes jumped up and came around face to face. I stood up almost automatically. He put his hand on my shoulder, looked me directly in the eyes, and said, "Binford, you are a heavyweight; so am I." I put my hand on François' shoulder; he turned: "Let's go drink some good wine."¹³⁶

This meeting in 1965 at Bordes' lab forged an unlikely and frequently contentious friendship between Bordes and Binford that lasted until Bordes' death. It was also the first in a lengthy series of exchanges in which Bordes and Binford each made their case regarding the 'real' explanation for Mousterian lithic variability. In the end, each failed to win the agreement of the other.

In 1966, Binford's publication of "A Preliminary Analysis of Functional Variability in the Mousterian of Levallois Facies," co-written with Sally Schanfield Binford, officially launched his challenge to Bordes' interpretation of variability in the Mousterian. In that article, Binford was careful to frame his argument in terms of the testability of Bordes' hypothesis that Mousterian variability is the result of differing tribes or cultural groups. He very deliberately chose not to address the 'face' validity of Bordes' interpretation or to offer a qualitative rebuttal of Bordes' conclusions, likely fearing that Bordes' weightier reputation would overshadow any alternative Binford could offer in its stead. Binford's focus was to force a discussion of the 'fit' between his methods and those of Bordes. He presented his argument in such a way that, regardless of his conclusion, he would be seen as having advanced the state of knowledge on Mousterian variability, noting in his introduction that "[i]f a means of testing were developed and [Bordes'] hypothesis confirmed, a major contribution would be made since we would then be forced to conclude that the social behavior of Neanderthal populations was vastly different from that of *Homo sapiens*."¹³⁷ Despite the overall deferential tone of the article (at least compared with his

¹³⁶ Ibid., 192.

¹³⁷ Binford and Binford, "A Preliminary Analysis of Functional Variability in the Mousterian of the Levallois Facies," 1966:240.

later writings), Binford did take a swipe at the static view of culture held by Bordes and other traditional archaeologists, stating that stylistic or cultural explanation of Mousterian variability requires that “one must envision a perpetual movement of culturally distinct peoples, never reacting to or coping with their neighbors ... a situation [that] is totally foreign, in terms of our knowledge of *sapiens* behavior.”¹³⁸

Binford tackled the Mousterian problem with a new set of methods that was far more complex than any Bordes had previously used. Foremost among those methods was Binford’s application of factor analysis (aided by some of the first extensive use of computers in archaeology) in determining the co-variance between certain of Bordes’ standard types of Mousterian tools. Binford proposed that factor analysis allowed him to show that the clustering relationship of Mousterian tool types, which Bordes had interpreted as culturally distinct *facies*, was actually the result of differing sets of activities. He presented a summary of his factors and their suggested activities juxtaposed with Bordes’ original taxa (reproduced in Table 4.2 below).

In the summary of the 1966 article, Binford stated definitively that his and Sally Binford’s work conclusively demonstrated that Bordes’ cultural (historical) interpretation was incorrect and that his methods, or more specifically the application of his taxonomy of Mousterian tools types as an interpretive framework for understanding variability, were inadequate to address human behavior in a prehistoric context.

Our findings suggest that a great deal of the variability in Mousterian assemblages can be interpreted as *functional variability*. Further, the nature of this functional variability strongly suggests that the social systems represented were culturally based and that the principles of organization of these social systems were similar to those known from contemporary hunters and gatherers.

Our findings also suggest some possible solutions to the problem of interpreting the alternation of Mousterian assemblages demonstrated by Bordes. The following points are relevant:

- (1) The use of multivariate statistics allows us to partition Mousterian assemblages into subunits of artifacts which can reasonably be interpreted as representing tool-kits for the performance of different sets of tasks.

¹³⁸ Ibid., 240.

- (2) These subunits of artifacts vary independently of one another and may be combined in numerous ways.

The significance of these findings is that correlations must be sought not for total assemblages but for these independently varying factors. This can be implemented in the field by the following methods of data collection:

- (1) Excavation of sites so as to reveal their internal structure (e.g., digging wide, contiguous areas), thus allowing us to study the spatial clustering of activities at a given location.
- (2) Excavation of as wide a range as possible of different forms of sites (e.g., open-air stations, caves, and rockshelters) to obtain information on the relationship between settlement type and range of activities.
- (3) Excavation of sites from different environmental zones to test the relationship between extractive tasks and the differential distribution of resources in a region.
- (4) Observation of a number of attributes not generally studied in detail:
 - (a) the degree of correlation between kinds of raw materials and groups of artifacts to evaluate the differential use of local and distant flint sources for artifacts used in various activities,
 - (b) the degree of correlation between different sets of activities (as defined by factors) and the form and composition of faunal assemblages;
 - (c) degree of correlation between types of activities and the form and composition of floral assemblages (pollen and macroplant remains).
 - (d) degree of correlation between kinds of activities and the physical characteristics of sites (extent of living area, degree of protection, etc.).

The provocative results of our study suggest to us that the methods of analysis used here are potentially useful for formulating testable hypotheses about social organization and evolutionary culture change within prehistoric communities.¹³⁹

In the 1966 article, Binford laid the groundwork not only for an ongoing debate with Bordes, but for a strategic assault on the traditionalist notion in archaeology that nonconformities in the archaeological record (in this case, the nonconformity being that artifact assemblages in the Mousterian do not reflect the expected pattern of continuous change over time; rather, the assemblages vacillate through time and space) can be explained only via cultural replacement. Binford's functional explanation was offered as an explicit attempt to refute that most basic assumption of culture history: "since culture is localized in individuals and is derived from humans, then breaks in continuity of cultural patterning must derive from breaks in the continuity

¹³⁹ *Ibid.*, 292-293.

Table 4.2. Binford's summary of his factor analysis juxtaposing his functional interpretation with Bordes' taxonomy (1966)¹⁴⁰

Factor Number	Diagnostic Variables	Suggested Activity	Type of Activity	Analogy to Bordes' Types
I	Typical borer Atypical borer Bec Atypical burin Typical end-scrapers Truncated flake Notches Miscellaneous tools Simple concave scraper Ventrally retouched piece Naturally backed knife	Manufacture of tools from non-flint materials	Maintenance tasks	Typical Mousterian (concave graph)
II	Levallois point Retouched Levallois point Mousterian point Convergent scrapers Double scrapers Simple convex scrapers Simple straight scrapers Bifacially retouched piece Typical Levallois flake Unretouched blade	Killing and Butchering	Extractive tasks	Ferrassie (convex graph)
III	Typical backed knife Atypical backed knife Naturally backed knife End-notched piece Typical Levallois flake Atypical Levallois flake Unretouched flake	Cutting and incising (food processing)	Maintenance tasks	Mousterian of Acheulian Tradition (concave graph)
IV	Utilized flakes Scrapers with abrupt retouch Raclettes Denticulates	Shredding and cutting (of plant materials?)	Extractive tasks	Denticulate (concave graph)
V	Elongated Mousterian point Simple straight scrapers Unretouched blade Scraper with retouch on ventral surface Typical burin Disc	Killing and Butchering	Extractive tasks	Ferrassie (convex graph)

¹⁴⁰ Ibid., 259.

of human populations in the area.”¹⁴¹

4.7 Bordes' Response

Typical perhaps for his temperament and his reputation in the field, Bordes made no attempt to fire off a quick response to Binford. His first response in print was made in 1968 when he invoked Binford (without actually naming him) and then summarily dismissed his arguments, noting that “[c]ertain anthropologists are reluctant to accept the idea that man with different industries could have inhabited the same environments and that others with identical industries could have prospered in different environments ... [b]ut this is nevertheless the case.”¹⁴² Responding more directly (although again without naming him) to Binford’s observation that Bordes’ cultural explanation defied rational understanding of how humans cope with neighboring groups, Bordes offered a rebuttal to the notion that Mousterians lacked geographical isolation.

Contacts, it is said, must have been numerous, and must have led to a blending of cultures; the Mousterian physical type, moreover, seems homogeneous. But the Mousterian type is not really as homogeneous as has been maintained; and we are completely ignorant about man of the Acheulean-tradition Mousterian. Moreover, man is more ready to exchange his genes than his customs, as the whole history of Europe demonstrates. If a woman from the Quina-type Mousterian was carried off by an Achedulan-tradition Mousterian man, she may perhaps have continued to make her tribal type of thick scraper (scattered specimens of which are found in the Acheulean-tradition Mousterian), but after her death probably no one went on making them. And finally, it must always be remembered that the Palaeolithic world was an empty world. We must not unreservedly extend to the Mousterian period the observations recorded about primitive tribes in the world today. For one thing, the population was certainly very thin on the ground. Nor can we compare the Mousterians to the Australian aborigines or Bushmen, for the Mousterians no doubt wandered much less and so made even rarer contact with others. A man must often have lived and died without meeting anyone of another culture, although he knew ‘that there are men living beyond the river who make handaxes’.¹⁴³

For the next two years, Bordes and Binford maintained their debate informally. Lewis and Sally continued their excavations, with Bordes’ blessing and assistance, at Combe Grenal. Bordes’

¹⁴¹ Binford, *An Archaeological Perspective*, 252.

¹⁴² Bordes, *The Old Stone Age*, 144.

¹⁴³ *Ibid.*, 144-145.

and Binford's good natured but intractable ribbing of each other is evidenced in a cartoon presented in 1968 to the Binfords by Bordes and his illustrator, Pierre Laurent, in which they imagine a 'functionally' divided Mousterian cave with special areas designated for cooking, making tools, and, even, doing statistics (see figure 4.1).

It was not until 1970 that Bordes directly and personally rebutted Binford. Published with his wife Denise de Sonneville-Bordes, "The Significance of Variability in Paleolithic Assemblages" was written in English and appears to be addressed to Binford almost exclusively. Much of its brief text (only 10 pages) was devoted to criticism of Binford's functional interpretation of Mousterian variability. After summarizing Binford's hypothesis that the variability is attributable to different specialized or functional activities, Bordes offered six *a priori* objections to Binford as follows: (1) the lack of killing sites in the Mousterian record of France made it distinct from the American archaeological record which has abundant evidence of such sites; (2) the rarity of Mousterian specialized workshops indicated that fabrication and utilization of tools occurred in the same places, not in specialized locales; (3) the questionable validity of factor analysis as a sound methodology; (4) the lack of specificity of the term 'site' inhibits comparability; (5) the limitations and risks inherent in applying ethnographic analogy to the Paleolithic are not known; and (6) the evidence for specialized activities within sites could be seen as precluding specialized activities between sites.¹⁴⁴

Bordes specifically objected to Binford's reliance on factor analysis and dismissed any meaning derived from it as "*interpretation, always open to criticism*".¹⁴⁵ He further questioned the validity of factor analysis as a legitimate interpretive method until such time as it could be verified through means pioneered by Russian archaeologist S.A. Semenov, best known for using microscopic use-wear and experimentation to identify directly the function of prehistoric tools. But Bordes' primary objection to Binford's functional interpretation was its inability to explain

¹⁴⁴ Bordes and Sonneville-Bordes, "The Significance of Variability in Paleolithic Assemblages," 66-67.

¹⁴⁵ *Ibid.*, 67.

why, if the different *facies* of the Mousterian were functional in nature, examples of all *facies* were not found in each region. As he noted:

In Charente, the M.T.A. is not quite unknown, but rare, while the Charentian seems to be almost the absolute master. The Combe-Grenal region seems to have been occupied throughout Würm I by Typical Mousterian, with the exception of a very brief incursion of Denticulate Mousterian (layer 38). In Provence (de Lumley 1965: 135) territorial continuities seem very strong, and the M.T.A. is unknown. One cannot help wondering what kinds of activities were undertaken in Dordogne under this *facies* which were unnecessary in Provence. The same question can be asked for older times about the scarcity or absence of handaxes over wide regions of Eastern Europe or Asia. And if the answer is that the same activities were being performed in a different way, then may we ask the following question: since there are several ways of performing the same activities with different tool kits, why not admit that the different Mousterian types just represent these different ways, and the difference is cultural?¹⁴⁶

Bordes was effectively arguing to Binford that Mousterian *facies* were, in fact, reflections of group preference for certain types or styles of tools, tools that were capable of performing the same task(s) regardless of their morphology. He would not acknowledge or could not conceive that the Mousterian *facies* that he had identified in his taxonomy lacked meaning in their prehistoric contexts; and if the *facies* had meaning in the past then that meaning must be derived from an identity-conscious social group, i.e., a culture, tradition, tribe, etc. Further, Bordes as an intellectual steeped in a French culture historical approach to the past that viewed prehistory an extension of history assumed that, lacking direct evidence to the contrary, change or variability in these inherently meaningful *facies* must be attributed to processes that are documented in historical contexts: migration, replacement, or diffusion. Bordes could not recognize Binford's functional explanation because it violated the conceptual framework from which he derived his understanding of how the past was to be understood.

At this point the Bordes-Binford debate became a contest in which there would be no declared victor, no winner who would ultimately win the day among his peers for the superiority of his argument. The problem was not that no one had a better methodology or an advanced

¹⁴⁶ *Ibid.*, 73.



Figure 4.1 Cartoon presented to Lewis and Sally Binford by François Bordes and Pierre Laurent in 1968.¹⁴⁷

¹⁴⁷ Renfrew, *The Explanation of Culture Change: Models in prehistory*, xi.

understanding of the data; the problem was that each was operating from completely different views of the past, how it worked, and how it was expressed in the archaeological record. Each looked at the same data set, recognized that that the material was excavated and properly catalogued, even recognized that the categorization of the material into identifiable taxonomic divisions was meaningful; yet, they arrived at two completely different interpretive destinations. It is not unreasonable to regard the scholarly worldview that separated them as a clash of paradigms. Each protagonist became almost a caricature of his respective paradigmatic tradition. While he respected and admired Binford's intellect and clearly saw him as a worthy peer, Bordes could not or would not relinquish the notion that his taxonomic categories were inherently and internally meaningful, not just in the present, but in the past. Moreover, he assumed that those processes that drove cultural change in the prehistoric past were the same processes that were operating in the historic past. Thus, Bordes' explanations for Mousterian variability would always be rooted in a paradigm that invoked historical processes such as migration and diffusion. For Bordes, the primary goal of prehistoric archaeology was to outline a natural *history* of man. Conversely, Binford -- with his perspective that the prehistoric past is foreign and not necessarily subject to the same processes operating in historic or present contexts -- could not concede the casual assumption that our modern understanding of the archaeological record reflected anything remotely resembling a prehistoric reality. Thus, Binford's interpretation of the variability in Mousterian assemblages would always favor an objective distance and reliance on past processes that transcended the activity of specific cultural groups.

Bordes and Binford continued to spar with regularity in the literature right up until Bordes' death in the early 1980s, but neither was able to win the other to his side. Their arguments became increasingly detailed and each routinely invoked the latest research that supported his respective position. Bordes' extensive knowledge of the French Paleolithic and his large professional network of peers and friends kept him solidly abreast of every potential

find that even remotely cast doubt on Binford's functional interpretation. Bordes often vexed Binford with the overwhelming abundance of data that had been generated from various Mousterian sites, an argumentative strategy that could be seen as drowning one's opponents in a tidal wave of minutiae. Binford, on the other hand, was a determined strategist with a keen mind capable of quickly cutting to the core of a proffered counterpoint to tease out its weaknesses and inconsistencies. A good example of this was Bordes' attempt to offer as evidence contrary to a functional argument the results of a study published by J. Bouchud in 1966 which concluded that many Mousterian sites were occupied year-round.¹⁴⁸ If sites were occupied throughout the year, then that would have weakened Binford's premise that Mousterians, like most foraging people, were highly mobile and, thus, would have used sites for different purposes, necessitating different tool assemblages. Mobility and cyclical occupation of sites were core elements in Binford's functional interpretation and were those discredited it would have severely weakened his argument. Bordes was extremely confident in Bouchud's work, which was highly technical and based on an analysis of reindeer teeth recovered from a number of Mousterian sites. But Binford rose to the challenge and in excruciating detail dissected Bouchud's assumptions and questionable conclusions, an impressive feat considering that Binford had to research, interpret, and apply an expert level of knowledge about the birth cycles and tooth eruption patterns of extant Siberian reindeer populations.¹⁴⁹

4.8 Early Reaction to the Debate

In the early years of the debate between Bordes and Binford, roughly between 1965 and 1970, only a handful of scholars interested in the Mousterian problem, most of them American or British, addressed the controversy. Surprisingly little attention was paid to the

¹⁴⁸ J. Bouchud, *Essaie sur la Renne et la Climatologie et Paléolithique Moyen et Supérieur*, (Périgeux: Magne, 1966).

¹⁴⁹ Binford, "Interassemblage variability-the Mousterian and the 'functional' argument," in *The Explanation of Culture Change*, 238-240.

debate in France. Early reactions came from those who had previously worked with Bordes or Binford or who were actively involved in prehistoric research in France.

In the Anglophone world, there were several individuals who weighed in directly on the debate, including Paul Mellars, James Sackett, Leslie Freeman, and Desmond Collins. Mellars was an early commentator. He was researching the Mousterian in the early 1960s and by 1965 had published work suggesting that the observed variability in Mousterian sites could be explained from a chronological perspective, an interpretation that Bordes had considered and rejected likely due to its uncomfortable similarity to the unilineal evolutionary explanations that had been vilified by his mentors Breuil and Vaufrey.¹⁵⁰ Mellars, however, continued to press his chronological explanation into the early 1970s. In 1970, Mellars directly addressed the issues at the heart of the debate between Bordes and Binford, effectively cautioning against categorical acceptance of either hypothesis.¹⁵¹ Regarding Bordes' cultural explanation, Mellars summarized and reiterated his interpretation that Mousterian variability could be largely attributed to simple chronological reasons, noting that at many sites the same temporal sequence of the Bordes' Mousterian *facies* was consistently expressed in the archaeological record. Of Binford's functional explanation, Mellars was more explicitly critical, detailing the many weaknesses in Binford's arguments that required further clarification. Mellars objected most strongly to Binford's insistence that the functional explanation was the more 'testable' of the various hypotheses, noting that "the prospects of adequately testing *any* of the current hypotheses concerning functional variability in Paleolithic and Mesolithic assemblages will remain extremely limited."¹⁵² Mellars concluded that much more work was necessary before either explanation could be declared satisfactory.

¹⁵⁰ P. Mellars "Sequence and Development of Mousterian Traditions in South-Western France," *Nature* 205 (1965): 626-627.

¹⁵¹ Mellars, "Some Comments on the Notion of 'Functional Variability' in Stone-Tool Assemblages," 86.

¹⁵² *Ibid.*, 86.

Another early Anglophone commentator was Leslie Freeman, a student of Binford at Chicago. Encouraged by Binford, Freeman conducted a study of Mousterian lithic variability on sites found outside France in northwestern Spain. Freeman's conclusion was clearly influenced by Binford's stance on functional interpretation of Mousterian variability:

If the Mousterian facies are temporal stages in evolution of tool kits, one would expect this evolution to be shown by the replacement of one or more tool types popular in an earlier period (represented by one or more early levels) by another type or types which became popular later (while a later level was accumulating). This characteristic of the changing temporal popularity of styles has been used as the basis for Ford's graphic seriation method (Ford 1962). If the several facies represent artifact inventories linked with and indicative of membership in different identity-conscious social groups, the same phenomenon of apparent replacement must occur between levels of occupation accumulated by distinct groups. Tool types specific to one such group must be replaced by different types specific to another group in this case. Both these situations of apparent replacement will result in a pattern of significant negative correlations between the frequencies of the types involved.

Not one single case of a significant negative correlation between any two stone tool types was discovered. The evidence is overwhelming that Mousterian facies cannot be either evolutionary stages of industrial development or tool kits traditionally specific to distinct, identity-conscious socio-cultural groups. They must, then, be tool kits which differ in their degree of appropriateness to the performance of distinct tasks.¹⁵³

Freeman is representative of the significant influence that Binford had on students at Chicago in the mid-1960s. This influence was amplified when Freeman's cohort of students began to influence students at other universities across the United States. As we shall see in Chapter 5, numerous studies on stylistic and functional explanations for variability in the archaeological record were conducted by American archaeologists, particularly, in the 1970s and 1980s.

James Sackett took up the issue of style versus function in 1966 with his published study on the variability observable in Upper Paleolithic assemblages in France.¹⁵⁴ Although he did not directly address the impasse between Bordes and Binford, he was most likely aware of it since he was working extensively with Bordes and other French and American Paleolithic archaeologists. Sackett acknowledged the potential of Binford's functional approach in

¹⁵³ Freeman, "The nature of Mousterian facies in Cantabrian Spain," 233.

¹⁵⁴ J. Sackett "Quantitative Analysis of Upper Paleolithic Stone Tools," *American Anthropologist* 68, no. 2 (1966): 356-394.

understanding interassemblage variability, but questioned whether style and function could, in fact, be extricated from each other:

[I]t is difficult to believe that *stylistic* features alone, even if explicable in terms of unconscious decisions or motor habits, could maintain the consistency reflected in Tables XI-XIII for the thousands of years that radiocarbon dating now assigns to the Dordogne Aurignacian development. Therefore, despite our relative ignorance of Aurignacian cultural ecology and techno-economics, it remains quite possible that the seriation results could indicate shifting emphases in the *functions* of end-scrapers or adaptive processes within manuf-turing techniques themselves. Both style and function might in fact be involved simultaneously, since among stone tools these terms are more appropriately applied to the manner in which attributes behave in specific contexts rather than to attributes themselves. A morphological feature such as marginal retouch may have served functionally either as an auxiliary cutting edge or as a means for shaping the front portion of end-scrapers, but at the same time its frequency of occurrence and mode of execution may have been subject to stylistic dictates. At any rate, it is obvious that neither stylistic nor functional considerations are presently capable of explaining the ideational basis of end-scrapers patterning.¹⁵⁵

Sackett later took issue with Binford on many aspects of what he saw as Binford's rigid functional model for explaining assemblage variability. The pair maintained an ongoing debate of their own throughout the 1970s and 1980s.¹⁵⁶

Desmond Collins, a British archaeologist, first weighed in on the debate in 1969, disagreeing with Binford's functional explanation and lending support to a cultural interpretation similar to Bordes'.¹⁵⁷ His article, however, was not well-received; most of the published comments on the piece, including one from Bordes, were generally negative -- noting that Collins was operating from a general lack of knowledge of the Paleolithic. Collins continued to press his argument against functional interpretations of tool assemblages in general, but Binford effectively discredited his claims at a seminar in 1971 held at the University of Sheffield.¹⁵⁸

¹⁵⁵ *Ibid.*, 388-390.

¹⁵⁶ See Sackett 1977, 1982, and 1986 and Binford 1983 and 1989.

¹⁵⁷ D. Collins "Culture Traditions and Environment of Early Man," *Current Anthropology* 10, no. 4 (1969), 268.

¹⁵⁸ Binford "Interassemblage variability-the Mousterian and the 'functional' argument," 228-230.

The initial reaction to the Bordes-Binford debate among French archaeologists was remarkable in that it was almost completely absent. There were French archaeologists who early on were indirectly involved in the Bordes-Binford debate such as J. Bouchud, J-P Rigaud, and, most notably, Denise de Sonneville-Bordes. But all were directly in Bordes' sphere of influence and were largely peripheral in their direct contributions to the substance of the debate. As mentioned earlier, Bouchud's work on length and season of occupation at various Mousterian sites was used by Bordes as evidence against Binford's core premise of mobility.¹⁵⁹ Denise de Sonneville-Bordes' work with her husband on the Mousterian and in her own right on the Upper Paleolithic contributed greatly to the body of knowledge regarding typology and variability in lithic assemblages.¹⁶⁰ There were a few French archaeologists who were doing innovative and creative work in Paleolithic research in the 1960s, some of whom could have influenced or been influenced by the debate between Bordes and Binford, but they never gained any traction within the French archaeological community. George LaPlace, for example, offered a new typological approach to lithic analysis that rivaled that of Bordes'.¹⁶¹ His approach, which he termed a 'synthetotype', integrated both type and attribute lists in a systematic and comparative framework that would have fit well within Binford's functional approach. But La Place's 'synthetotypes' never caught on; it was cumbersome and lacked the ease of use afforded by Bordes' typology. Similarly, Michel Brézillon, an associate of André Leroi-Gourhan, published one of the first semiological analyses of lithic types that had intriguing ramifications for an understanding of the nature and origin of type definitions.¹⁶² While Brézillon's work was not directly related to the Bordes-Binford debate, it was an early attempt to attribute observed variability in lithic assemblages to modern constructs of what connotes 'type' rather than to any

¹⁵⁹ Bouchud, *Essaie sur la Renne et la Climatologie et Paléolithique Moyen et Supérieur*, 1966.

¹⁶⁰ See Sonneville-Bordes 1960 and Bordes and Sonneville Bordes 1970.

¹⁶¹ La Place "Essai de typologie systematique," *Annali dell'Universita di Ferrara* 15 (1964): 1-85.

¹⁶² M. Brézillon, "La dénomination des objets de pierre taillée." in *IVe supplement a Gallia Préhistoire* (Paris: CNRS, 1968).

inherent notion of type as conceptualized by ancient peoples. Like La Place, Brézillon, and for that matter Leroi-Gourhan, never achieved the recognition or level of authority and influence that Bordes had gained.

Chapter 5 will address the issue of the lack of French reaction to the Bordes-Binford debate more directly, but it is important to note that some French archaeologists have acknowledged the delay of new intellectual movements arriving in the discipline of archaeology in France. Cleuziou, Coudart, Demoule and Schnapp noted that “the paradox of archaeology in France is due to repeated time lags.”¹⁶³ In this case, this is an accurate observation, as it was not until the 1970s and 1980s that French archaeologists began to address the vexing questions, both methodological and theoretical, that arose because of the Bordes-Binford debate. On the other hand, reaction to the debate in the United States and other anglophone countries was more pervasive, likely due to the swift rise in popularity of the New Archaeology, which was coming into its own by the late 1960s.

¹⁶³ S. Cleuziou, et al., “The Use of Theory in French archaeology.” in *Archaeological Theory in Europe: The last three decades*, ed. Ian Hodder (New York: Routledge Press, 1991), 96.

CHAPTER 5

THE AFTERMATH: 1970-PRESENT

In the wake of the Bordes-Binford debate, three distinct and at times contradictory trends can be discerned in Paleolithic archaeology. First and most predictably, the debate spurred a wave of research into the causes of interassemblage variability, the core issue at the heart of the Bordes-Binford debate. A host of scholars on both sides of the Atlantic took up trowels and pens hoping to add to the body of knowledge that would bring resolution or at least closure to the debate. Second, the polemics of the debate fostered a general sense of intellectual polarization between French and American prehistorians, which was frequently reflected in the literature in discussions about whether French and American approaches to the study of the past were the result of incommensurable paradigms. Third, and on its face seemingly contradictory to the second, the debate stimulated interest in the Paleolithic among American archaeologists, sparking a wave of American involvement not just in French Paleolithic research but across the whole of continental Europe, southwest Asia, and northern Africa. The intense discussions of paradigms coupled with more frequent interaction made the four decades that have elapsed since the Bordes-Binford debate some of the most dynamic and diverse in the history of archaeology.

5.1 Debating the Merits of Bordes' and Binford's Positions on Mousterian Variability

In the years following the initial exchanges and challenges between Bordes and Binford, many scholars, French and American, initiated research agendas aimed at resolving the question of interassemblage variability in the Mousterian. Additionally, the debate was extended to variability in subsequent periods of the Upper Paleolithic in Europe as well. The

explanatory positions were, in the beginning, neatly categorized as either cultural/stylistic (vis-à-vis Bordes) or functional (vis-à-vis Binford). Only later in the 1970s did the trend begin to turn away from the perception that they were mutually exclusive.

In the early to mid-1970s, Bordes and Binford continued to dominate the scholarly arena and to argue their respective positions.¹⁶⁴ A few additional protagonists were active in the debate at that time, specifically James Sackett, who advocated for an expanded stylistic approach to artifact variability, and Paul Mellars, who pressed for an alternative chronological explanation for Mousterian variability.¹⁶⁵ But increasingly by the late 1970s and early 1980s Bordes' and Binford's students and associated peers began to eclipse the original protagonists in the literature. In the United States and Canada, Leslie Freeman, Arthur Jelinek, and Nicolas Rolland continued research agendas that included the study of Mousterian variability. In France in the 1970s, individuals such as Jean-Philippe Rigaud, Henri Laville, Henry de Lumley, Alain Tuffreau, and André Debénath were contributors to the debate. Other European scholars outside France such as Marcel Otte in Belgium and Januszcz Kozlowski in Poland, were also integral to advancing Paleolithic research and the question of assemblage variability in what could be viewed as the French tradition.

In the mid-to-late 1970s two individuals in particular, Arthur Jelinek and Nicolas Rolland, initiated a new approach to Mousterian lithic variability that was to prompt a radical reinterpretation. In 1976, Jelinek, an American scholar out of the University of Michigan group that included Binford, published groundbreaking research into the effects of intensive reuse and recycling on artifact assemblages.¹⁶⁶ In 1977 and 1981, Nicolas Rolland, a Canadian archaeologist from the University of Victoria, published research on a new quantitative method

¹⁶⁴ See Bordes 1972, 1973, and 1978 and Binford 1972, 1973, and 1977.

¹⁶⁵ See Sackett 1973, and Mellars 1970 and 1973.

¹⁶⁶ A. Jelinek, "Form, Function, and Style in Lithic Analysis," in *Cultural Change and Continuity: Essays in Honor of James Bennett Griffin*, eds. J. Griffin, and C. Cleland (New York: Academic Press, 1976).

for identifying variability that was the result of differential transformation of Mousterian tools from primary into secondary and tertiary tools.¹⁶⁷ Initiated in part by Jelinek and Rolland's work and in part by the continued reverberations of the larger Bordes-Binford debate, the period between the mid-1980s and the early 1990s saw a peak in terms of scholarly output on research into the questions of variability in the Paleolithic on both sides of the Atlantic. In France, a new generation of prehistorians including Jacques Tixier, Sylvie Beyries, Jacques Pélegrin, Jean-Michel Geneste, Eric Boëda, Liliane Meignen, Patricia Anderson-Gerfaud, and Alain Turq expanded on the work of earlier French researchers who also continued to research and publish. In the United States and Canada, Harold Dibble, Nicolas Rolland, Geoffrey Clark, C. Michael Barton, Anthony Marks, John J. Shea, and Steven Kuhn emerged and continued the American research tradition into Middle Paleolithic lithic variability. These American and European scholars began to see the Bordes-Binford debate as too dichotomous to be meaningful in the long run. It should be noted also that during this period research was no longer limited to the Paleolithic in Western Europe and included an expansion of work at sites in central and eastern Europe, southwest Asia, and northern Africa.

The culmination of this greatly expanded body of research began to pay dividends in the mid-1980s, most notably in the work of Harold Dibble. Dibble, a student of Jelinek, continued what is arguably some of the most important work on Mousterian lithic variability since the initiation of the Bordes-Binford debate.¹⁶⁸ By the early 1990s Dibble had synthesized the relevant work of numerous American, Canadian, and French prehistorians, particularly Jelinek's and Rolland's arguments, into a new explanatory model for Mousterian lithic variability. The model proposed by Dibble, in close conjunction with Jelinek and Rolland, recast the factors of style and function in the archaeological record of the Paleolithic -- factors central to the Bordes-Binford debate -- as intellectual straw men with questionable meaning. The model

¹⁶⁷See N. Rolland, "New Aspects of Middle Paleolithic Variability in Western Europe," *Nature* 255 (1977): 251-252 and "The Interpretation of Middle Palaeolithic Variability," *Man* 16, no. 1 (1981): 15-42.

¹⁶⁸ H. Dibble. "Raw-Material Variation in Levallois Flake Manufacture," *Current Anthropology* 26, no. 3 (1985): 391-393.

offered instead the idea that the tool types in Bordes' taxonomy were modern constructs which, while meaningful for descriptive purposes, represented stages in a continuum of tool utilization. In other words, the sixty-three different types of Mousterian tools identified by Bordes and the basis for his five *facies* had no inherent meaning and thus were of limited use in an interpretative framework. A toolmaker in the Mousterian, Dibble's model proposed, did not necessarily sit down and consciously decide to make a convergent *racloir* as opposed to a simple *racloirs*; s/he constructed a tool, modified it, used it, and discarded it or perhaps modified it again before it broke or was lost. Thus, the tool types observed in the archaeological record of the Mousterian and elevated in Bordes' taxonomy (which was accepted and used in total by Binford) were more a record of the *stages* at which tools were discarded and not intended end products. Dibble's synthesis known variously as the 'reduction model' or 'utilization model' effectively neutralized the style-versus-function dichotomy and the associated polemics that were part and parcel of the Bordes-Binford debate. As Dibble and Rolland wrote in 1992:

We are not arguing that function and style did not contribute to lithic artifact and assemblage variability. Undoubtedly they did, but it is a question of whether or not we can recognize their effects given the coarseness of our methodology and the very nature of our data. Our argument has been that, at least in the Middle and Lower Paleolithic, the relative contribution of these factors are greatly overridden by the other factors of raw material and intensity of utilization. This does not mean that the classification system is flawed, since it still operates effectively at the descriptive level for which it was intended. But if the effects of function and style are being masked, then a continued emphasis on these two factors in attempts to explain Middle Paleolithic variability cannot be fruitful. Nor has it been fruitful for the last two decades.¹⁶⁹

Dibble's and Rolland's observation on the state of the merits of the Bordes-Binford debate (i.e., style versus function) was reflective of the new norm in Paleolithic research that had emerged by the early 1990s. No longer was the question of Mousterian interassemblage variability

¹⁶⁹ H. Dibble and N. Rolland. "On Assemblage Variability in the Middle Paleolithic of Western Europe: History, perspectives, and a new synthesis," in *The Middle Paleolithic: Adaptation, Behavior, and Variability*, eds. H. Dibble and P. Mellars (Philadelphia: University Museum Press, 1992), 18.

framed in the terms that were originally established in the 1960s by Bordes and Binford. A new language had emerged.

Research conducted in the years just after the mid-1990s on Mousterian variability has tended to reflect the recognition of complexity that originated with Dibble's synthesis in the early 1990s. No longer in the literature was research into Mousterian lithic variability describable as one more rehash of the Bordes-Binford debate. That is not to say, however, that style and function were no longer considered meaningful factors in understanding artifact variability or that the influences of Bordes and Binford do not still hold sway to some extent in Paleolithic archaeology. Clearly they did, as most researchers continued to acknowledge the debate when discussing interpretations of variability in the archaeological record.¹⁷⁰ To some extent, the influence lingered on due to the presence of the original protagonists, directly in the case of Binford, who continued to publish and advise students well into the twenty-first century, and indirectly in the case of Bordes, who died in 1981. Nonetheless, his influence continued to strongly mark French Paleolithic archaeology through the work of his former students and the dominance of the Institut de Préhistoire et de Géologie du Quaternaire, the research center he helped found at the University of Bordeaux in the 1950s and that thrives still today. Curiously, the Bordes-Binford debate, which was effectively over – at least in terms of the original substance -- by the mid-to-late 1980s and was subsequently bumped out of the limelight by Dibble and others in the 1990s, was never fully resolved, and it has never been completely ushered off stage. Another reason that the debate continued to influence Paleolithic archaeology, even though subsequent researchers rejected its simple dichotomous terms for examining and explaining the past, is that it was still informative, not for the specific arguments of the protagonists but in terms of what they represent from an epistemological perspective.

¹⁷⁰ See Hays and Lucas 2000, Straus 2002, Clark 2002, Otte 2002, Harrold 2002, and Hiscock and Clarkson 2007.

5.2 The Debate as a Clash of Paradigms?

As discussed in previous chapters, the Bordes-Binford debate can be characterized in many ways: a difference of opinion, a case of differing interpretative traditions, or a conflict of methodologies. Marcel Otte even suggested that the debate had more to do with personal agendas than with the merits of the debate itself, noting that “fastidious descriptions of European lithic assemblages have led to different and often contradictory interpretations—lyric or pathetic—that seem ultimately to be reflections of irreconcilable personal obsessions.”¹⁷¹ But, fundamentally, the question becomes: was it a clash of paradigms? All of the descriptions noted above have been applied to the debate at one time or another and all may, in fact, have merit. It is worthwhile, however, to directly address the efficacy of casting the debate in terms of incommensurable paradigms as this has implications of a higher magnitude than a simple difference of opinion between scholars in a relatively esoteric discipline.

The concept of a paradigm, as understood in a modern context, was provided by Thomas Kuhn in the 1960s. Kuhn defined a paradigm as a particular type of scientific world view, one which acts to delimit what data can be brought to bear on a topic, the type and structure of questions that can be appropriately explored, and the range of potential interpretations.¹⁷² Moreover, Kuhn attempted to describe the concept of incommensurability as applied to paradigms:

In a sense I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds. One contains constrained bodies that fall slowly, the other pendulums that repeat their motions again and again. In one, solutions are compounds, in the other mixtures. One is embedded in a flat, the other in a curved, matrix of space. Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction.¹⁷³

¹⁷¹ M. Otte. “The Significance of Variability in the European Mousterian,” in *The Middle Paleolithic: Adaptation, Behavior, and Variability*, eds. H. Dibble and P. Mellars (Philadelphia: University Museum, University of Pennsylvania, 1992), 46.

¹⁷² Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962, 1970).

¹⁷³ *Ibid.*, 150.

It would be easy and convenient to say that Bordes and Binford looked at the same data set, used the same taxonomy, and arrived at two completely different positions and conclude summarily that the debate was a clash of paradims. The reality, however, was more complex. Yes, Bordes and Binford interpreted the data very differently, but were they really 'practicing in different worlds'? From one perspective, one could argue that Bordes and Binford adhered to a similar paradigm in that they recognized and accepted the same types and structures of the data (i.e., the Bordes Taxonomy) and, by and large, each agreed on the types of questions that were appropriate to ask of the archaeological record and a common methodology for how to acquire the data. They did indeed differ in terms of how the questions about the past should be structured, how the data should be analyzed, and certainly how the data should be interpreted, but these were not necessarily insurmountable and each certainly understood and was knowledgable about the approach and interpretation of the other.

In one very important and almost literal sense, however, Bordes and Binford were of two 'different worlds' -- Bordes of the Old World and Binford of the New. As Frank Harrold observed:

An indisputable paradigm clash did occur in the 1960s as French prehistory collided with American processual archaeology, most memorably in the Bordes-Binford dispute over the nature of Mousterian variability. Here indeed were such different conventions for asking and answering questions about the past that "settling" the argument was all but impossible. It was not, however, a paradigm crisis in Kuhn's classic sense. It was generated, not internally through increasing dissatisfaction with the old paradigm, but through **confrontation between two existing ones.**¹⁷⁴ [emphasis mine]

The idea of conflicting Old and New World paradigms may, in fact, be the best lens through which to interpret the Bordes-Binford debate.

The notion of Old World and New World paradigms, however, is not without controversy itself, and one must be careful not to overstate the polarization of the argument as French (or European) versus American beyond the specific Bordes-Binford debate as this was not an

¹⁷⁴ Harrold, "The Elephant and the Blind Men," 168.

absolute division. Several American archaeologists, notably James Sackett, whose approach to Paleolithic archaeology was decidedly more 'French' in its outlook than it was American, and certain French archaeologists, such as Anta Montet-White, who studied at Michigan in the 1960s, were significantly influenced by anthropological archaeology. Some scholars are and were selective in terms of what they see as strict transatlantic paradigmatic differences or simply nationally influenced interpretive traditions. Harrold, for example, who, as noted above, clearly saw the Bordes-Binford debate in paradigmatic terms in 1991, stated in 2002 that "[i]f it could ever have been said that European-American interchange involved the clash between two incommensurable paradigms, it is not so today."¹⁷⁵

Bordes and Binford were the first to invoke the idea that their debate could best be framed in terms of how their national research traditions (paradigms?) and associated archaeological traditions affected the way they approached their work. Bordes noted in the foreword to his 1968 book *The Old Stone Age* that:

There are a variety of schools in the field of prehistory. In France, the emphasis is especially on stratigraphy and typology; in the English-speaking countries, on the relationship between man and his environment; in the USSR, on palaeo-sociology. Of course, things are not quite so simple as this, and French researchers do not neglect the finding of ecology and sociology, any more than Americans or Russians neglect stratigraphy; it is more a question of stressing one point of view or another.

There are historical reasons for this tendency. In France, the first study of prehistory was the work of naturalists. The Americans, however, with primitive peoples still living among them, were able to study their relationship with the environment.¹⁷⁶

Binford clearly saw paradigmatic differences in archaeological approaches; he even wrote an entire article on the matter in 1982 with Jeremy Sabloff in which he noted that:

The ways archaeologists view the past--their paradigms--directly influence their interpretations of the archaeological record. Paradigm change need not be irrational or undirected; such change can best be accomplished by focusing

¹⁷⁵ Harrold, "Transatlantic Prehistory: Thoughts on the encounter between American and European Paleolithic prehistory," 27.

¹⁷⁶ Bordes, *The Old Stone Age*, 7.

attention on the various ways that dynamic cultural processes can be linked with the static archaeological record.¹⁷⁷

He applied that notion of conflicting paradigms to the differences he perceived between the Old and New World approach to archaeology:

The New World and Old World views of the world are paradigms. They summarize expectations as to what "culture" is like. Comparison of these two paradigms should illustrate just how insightful philosophers have been when they argue that our world view, or paradigm, conditions our observation and description of experience. But a paradigm also directly conditions the classificatory procedures which archaeologists have designed to measure culture.¹⁷⁸

In recent years, the most vocal advocates for recognition of distinct Old and New World paradigms have been Lawrence Straus and Geoffrey Clark, both products of the Chicago cohort that was heavily influenced by Binford and the emergence of the New Archaeology in the 1960s. In a series of articles beginning in the late 1980s, Straus and Clark expanded Binford's and Sabloff's concept of Old and New World Paradigms and applied it to a range of specific research questions in Paleolithic archaeology.¹⁷⁹ Even a sampling of the titles of their publications, such as "Paradigm Lost" (Straus 1987), "Paradigm Found (Straus 1991), "Paradigms in Science and Archaeology" (Clark 1993), and "A Paradigm is like an Onion" (Clark 1991), give evidence to their overriding concern with paradigmatic biases in Paleolithic archaeology.

But what exactly are these supposed paradigms, and are these divisions reflective of reality and accepted by American, French, and other European archaeologists? Or is this purely an exercise in reflexive self-questioning on the part of American archaeologists stimulated by the disciplinary critique of the New Archaeology and then projected onto their Old

¹⁷⁷ Binford and Sabloff, "Paradigms, Systematics, and Archaeology," *Journal of Anthropological Research* 38 (1982): 137.

¹⁷⁸ *Ibid.*, 144.

¹⁷⁹ See Straus 1987, 1991 and 2002, Clark 1991, 1993, 1999 and 2002 and Clark and Lindly 1991.

World peers? A brief review of how these traditions/paradigms are/were construed will serve to address these questions. For simplicity's sake, in this study these different approaches will be termed the Old World tradition and the New World tradition.

5.2.1 The New World Tradition

The New World tradition as defined first by Binford and then later by others such as Straus and Clark has typically been more consistently understood and described in the literature than the Old World tradition, even among those who are not entirely convinced that the differences between American and European archaeologists constitute different paradigms in the strictest sense. It is generally framed in terms of its adherence to an anthropological perspective that is rooted in the concept of culture as an adaptive system and a disavowal of the presumption that variability is attributable only to differences in cultural traditions. The emphasis in the New World tradition is on continuity and stability across time and space. Research pursuits focus on the search for patterning in the archaeological record and display a concern for developing and testing potential hypotheses for the recognition and interpretation of the observed patterning. A review of several different descriptions of the New World tradition from different perspectives is revealing in its consistency. Binford and Sabloff gave one of the first specific definitions:

The first, or New World, viewpoint is rooted in empirical generalizations regarding the nature of culture which early ethnographers generated from their comparative study of culture traits (largely material objects) across the named social groups of the American Indian. As we have seen, the perspective of the New World paradigm is one of outside observers looking at variability across socially organized groups of people.¹⁸⁰

Lawrence Straus, a scholar who has routinely cast the Old and New World traditions as definitive and distinct paradigms, said:

With the concern for science and testing of ideas, American archaeologists are interested in **explaining** variability by entertaining alternative hypotheses ... [b]ut this point does not imply that American archaeologists deny the existence of

¹⁸⁰ Binford and Sabloff, "Paradigms, systematics, and archaeology," 144.

more or less marked ethnicity in the past (depending on such key variables as human demographic density, geographic characteristics of territories, resource types, and distribution, etc.) or the potential ability of archaeology to elicit at least hints thereof. The problem is **how** to determine **which** attributes of the prehistoric record might be informative of ethnicity. This problem is subject to theory building and logical bridging arguments – but not to mere presumption.¹⁸¹

Another view was provided by Frank Harrold who, although accepting of the Bordes-Binford debate as a clash of paradigms, as noted above, has questioned the utility of seeing the broader distinctions between New World and Old World archaeological traditions in strict paradigmatic terms. Even still, he saw the same kinds of concerns among American archaeologists that were first highlighted by Binford and Sabloff and Clark and Straus:

American archaeologists brought with them some intellectual assets, such as a concern for explicit formulation and testing of hypotheses, and the linkage of prehistory to relevant data and theory in hunter-gatherer ethnology, human ecology, and evolutionary biology, that have enriched the study of the European Paleolithic.¹⁸²

From a European -- although not necessarily French -- perspective there is also recognition of what constitutes an Americanist approach and even some use of the term paradigm. Januszcz Kozlowski has noted:

The essential difference between European and American archaeology consists in the opposition of the European cultural-historical paradigm and the American model which equates archaeology with anthropology. I believe that the two approaches are complementary: on the horizontal: synchronous level an interpretation of facts and records from anthropological perspective is indispensable, whereas the diachronic approach requires a historical perspective.¹⁸³

¹⁸¹ Straus, "‘American’ Perspectives on the European Upper Paleolithic?" 2.

¹⁸² Harrold, "Transatlantic Prehistory: Thoughts on the encounter between American and European Paleolithic prehistory," 31.

¹⁸³ J. Kozlowski, "American and European Archaeology of the Palaeolithic: How to make use of divergent interpretations?" in *The Role of American Archeologists in the Study of the European Upper Paleolithic*, ed. Lawrence G. Straus (Oxford, England: Archaeopress, 2002), 35.

Similarly, Nuno Bicho, a Portuguese prehistorian, commenting on the impact of American approaches to archaeology in the study of the Portuguese Paleolithic, observed:

At the same time, American Archaeology trained that group of people to think using different scientific paradigms. This, of course, resulted in long-term projects, with large teams of different experts, with paleoecological problems in mind, as well as the study of site formation processes and taphonomic questions.¹⁸⁴

The New World tradition has also been consistently defined by Europeans in terms of what it lacks: a sense of and appreciation for the role of history and historical processes in explaining the past. Cleuziou and others have stated that “[i]t is well known that history is notably absent from the American New Archaeology.”¹⁸⁵ Otte is even more forceful noting that:

A third dimension often lacking in American researchers is related to the weight of history, the responsibility to cultural traditions in prehistoric times. Across the Atlantic, one should know and admit that history has no value other than itself, and it does not respond to any simple law of logic. This seems to provoke in America a kind of terror that stores the historical sense in an area that is inexplicable, therefore taboo.¹⁸⁶

What is remarkable in these examples is the consistency with which archaeologists on both sides of the Atlantic view the New World approach to archaeology. There is consensus that the New World tradition is characterized by its association with anthropology – and, conversely, its lack of association with history – and its adherence to a more explicitly theoretical and generalizable approach to the study of the deep past. Interestingly, this perception seems to persist even as theoretical debates among New World archaeologists have emerged over the perceived rigidity in the tenets of the New Archaeology as espoused by Binford and a renewed appreciation for a more historical perspective on the past.¹⁸⁷ This

¹⁸⁴ N. Bicho, “The Impact of American Archaeology in the Study of the Portuguese Upper Paleolithic,” in *The Role of American Archeologists in the Study of the European Upper Paleolithic*, ed. Lawrence G. Straus (Oxford, England: Archaeopress, 2002), 11.

¹⁸⁵ Cleuziou, et al., “The Use of Theory in French Archaeology,” 114.

¹⁸⁶ Otte, “Le rôle des américains dans la recherche du Paléolithique en Europe,” 40.

¹⁸⁷ See I. Hodder’s *Archaeological Theory Today* (Cambridge, England: Polity Press, 2001) and P. Ucko’s *Theory in Archaeology: A world perspective*, (London: Routledge, 1995).

consistency and persistence in itself is evidence that the New World approach to the study of the past may be best defined as a paradigm. Kuhn's classic definition of a paradigm is that of a worldview that transcends specific theories or theoretical approaches; a single paradigm, in the Kuhnian view, can encompass multiple theories.¹⁸⁸

But does this also hold true for the Old World tradition? To what extent does the French approach to archaeology reflect an Old World tradition and does it constitute a paradigm? Is there, in fact, a coherent and consistent intellectual world-view that one could say is typical of French – and other European -- prehistorians which rises to the level of what modern scholars would consider a paradigm? Often French Paleolithic archaeology is portrayed by American and British scholars simultaneously as normative, historical or culture-historical in its outlook on the one hand and, on the other, as 'atheoretical' or concerned more with methodology than theory. Which is it? Is it both? In attempting to answer these questions, it is worthwhile to explore various descriptions of the Old World archaeological tradition and to discuss briefly the role and use of theory in French archaeology.

5.2.2 *The Old World Tradition*

Bordes himself clearly thought that French and other European archaeologists approached the study of the past in a way that was unique to their personal, regional, and national experiences. He posited that these experiences could help influence how one 'read' the archaeological record, observing that "scientific facts are always more or less colored by interpretation, and each man's mind acts like a lens, which concentrates its rays according to its focus."¹⁸⁹ He stressed the French emphasis on methodology, particularly stratigraphy and typology, and noted the historical ties that French archaeology had with the natural sciences. Bordes' ideas about the Old World tradition were echoed in Binford and Sabloff's description over a decade later:

¹⁸⁸ Kuhn, *The Structure of Scientific Revolutions*, 1962.

¹⁸⁹ Bordes, *The Old Stone Age*, 7.

The second, or Old World, viewpoint is rooted in a less systematically studied, but just as empirically based understanding of European history. In contrast, the Old World paradigm takes the viewpoint of inside observers looking at themselves, relative to the social world of their experiences. Advocates of this Old World paradigm compare other societies from this egocentric point of view.¹⁹⁰

Later descriptions of the Old World tradition by American archaeologists have hewed quite closely to that of Binford and Sabloff, emphasizing the historical approach.¹⁹¹ Clark and Lindly, for example, stated that French and other Old World prehistorians operated from the problematical “preconception that prehistory is history projected back into the preliterate past and that process in the remote past can be treated as an extension of process in history.”¹⁹² Straus has made the case also that in France:

[p]re-history is seen as literally writing the ethnohistory of the past by means of bones, sherds and/or stones. The key is the term “prehistory” – this is seen as a fundamentally **historical** enterprise (sometimes linked more closely to Quaternary geology, and at other times merely an appendage of history – as reflected in the academic departmental placements of prehistorians).

Europe, with its long, rich (and newly renewed) traditions of regionalism, linguistic differences and even separatism (in reaction to the trend toward central state domination begun during the Renaissance), naturally sees the archaeological record in terms of cultural divisions. This is, I believe, deeply engrained in the lifelong experience and education of European prehistorians.¹⁹³

This notion of the Old World tradition as a more historical approach is one that continues to gain traction especially among French archaeologists, although it is also not without controversy. Some American archaeologists, notably Binford, Straus and Clark, have categorically defined the Old World tradition as a historical (or sometimes culture-historical) approach purely on the grounds that French archaeologists frequently offer historical processes such as migration and diffusion as the catalysts for cultural change. Some scholars have even

¹⁹⁰ Binford and Sabloff, “Paradigms, Systematics, and Archaeology,” 144.

¹⁹¹ See Straus 1987, 1991 and 2002; Clark 1991, 1993, 1997; and Clark and Lindly 1991.

¹⁹² Clark and Lindly, “On Paradigmatic Biases and Paleolithic Research Traditions,” 577.

¹⁹³ Straus, “American Perspectives on the European Upper Paleolithic? An Introduction,” 1.

compared this approach to that of the concern for the *longue durée* as described by the *Annales* School and characterize this as an influence on Paleolithic archaeology. Janusz Kozłowski, for example, evoked this with his observation that “a lasting value of European prehistory has been its historical approach, similar to *histoire événementielle*, in F. Braudel’s sense expressing – under the appearances of superficial happenings – slower rhythms and long-term trends.”¹⁹⁴ Others, like Marcel Otte, who vigorously argues for the role of historical processes in explaining culture change, have rejected the notion that Old World archaeologists receive “dogmatic” training in a “grande école” designed to inculcate scholars into a historically inclined Old World or French intellectual tradition.¹⁹⁵

In reality, the alignment of French archaeology with the social sciences, including history, however, has not been systematic or even. An opportunity did arise after World War II when Lucien Febvre began to expand the traditional view of history in such a way as to be more welcoming of the human sciences. Moreover, Febvre’s concept of ‘total’ history seemed a natural fit for archaeology as it was inclusive of a long view of the past, a requisite for archaeologists interested in prehistory. He wrote about archaeology that:

[i]t is the scientific study of the various activities and creations of men in the past, spotlighted at the moment they happened, within the framework of societies, which, though very different, are yet equivalent to each other ... and with which they covered the earth’s surface and the succession of ages.¹⁹⁶

But an alliance between history and archaeology in France was not to be, at least not in any explicit way. This was particularly true in Paleolithic archaeology. Bordes and most other French prehistorians in the 1950s and 1960s, with the exception of André Leroi-Gourhan, made no reference, positive or negative, to the writings of the *Annales* School and generally seemed

¹⁹⁴ Kozłowski, “American and European Archaeology of the Palaeolithic: How to make use of divergent interpretations?” 36.

¹⁹⁵ M. Otte, “On Paleolithic Research Traditions: A response to Clark and Lindly,” *Current Anthropology* 33, no. 4 (1992): 408.

¹⁹⁶ Lucien Febvre, *Combats pour l'histoire* (Paris: A. Colin, 1953), 32.

to operate outside those intellectual circles. This situation extended well into the 1970s and 1980s. Despite efforts by Febvre and Bloch and Braudel's publicly expressed interest in material culture, archaeology played a very limited role in the *Annales*.¹⁹⁷ The same trend is noted by Cleuziou and others: "[a] glance at the bibliography in Braudel's last work [1986] shows the gap between the French historical school and archaeological research undertaken in the last twenty years concerning pre- and proto-historic societies."¹⁹⁸

That does not mean, however, that French Paleolithic archaeology or Bordes himself was unaffected by the intellectual trends that arose out of the *Annales* School, even if he never explicitly referenced them. There are clear parallels between Bordes' conceptualization of life in the deep past and the concept of the *longe durée* as espoused by Braudel and the *Annales* School. For example, Bordes stated in 1970:

[A]ll the history of Europe shows ... man exchanges his genes more readily than his customs. Moreover, in primitive societies, conservatism is usually very strong, and if one supposes that a Mousterian of Acheulian Tradition married a Quina woman, she might well have gone on using the thick scrapers to which she was accustomed, but we doubt that her daughters would have done the same.¹⁹⁹

Bordes also referred to the notion that "contact between different traditions may well have been rarer than is usually thought and not always peaceful" and he spoke of the "long intervals" in which one group would occupy a site only to be replaced or pushed out by another.²⁰⁰ The sentiments reflected in Bordes' statements include an appreciation for the longevity of cultural traditions and a sense of place, a type of inertia that resists change unless it is brought by an external entity or force. In these expressions are echoes of the cultural conservatism underlying the superficial happenings that are at the core of history

¹⁹⁷ Alain Schapp, "Les Annales et L'Archéologie: une rencontre difficile," *Mélanges de l'Ecole française de Rome, Antiquité* 93, no. 1 (1981): 469-478.

¹⁹⁸ Cleuziou, et al. "The use of theory in French archaeology," 114. Also, see F. Braudel, *L'identité de la France* (Paris: Arthaud, 1986).

¹⁹⁹ Bordes and Sonnevile-Bordes, "The Significance of Variability in Paleolithic Assemblages," 65.

²⁰⁰ *Ibid.*, 72.

as conceived and practiced by the *Annales* School. Olivier and Coudart have recognized this historical facet in French archaeology and proposed that:

What makes the French approach specific in the domain of human sciences is the central place accorded to history. It is the social dimension of history which plays a primordial role, and which aims to study, beyond the sequence of events, the interaction of humans with the specificity of nature and with the diversity of other cultures. In a continent with a long history (continental Europe, in this case) the transformation and reproduction of identities imply necessarily taking into account cultural confrontations and lasting geopolitical structures. Geography, cultural differences, politics and history lie at the heart of every event. This close meshing of space, time and human identity was bound to produce, in continental Europe, mentalities different from those which an island tradition has engendered (since here the construction of a shared history has always presupposed the rejection of other cultures) or from those in a country with a short history or colony (since here an interest in prehistory implied primarily an interest in the natives of that country).²⁰¹

By the close of the twentieth century, there was a fair amount of consensus that, at the very least implicitly, Paleolithic archaeology in France has been heavily influenced and continues to be reflective of a more historical approach to the archaeological record than archaeology as practiced in the U.S.

Another aspect of the Old World tradition has been the idea that it is atheoretical, or perhaps more properly, theoretically naïve. Certainly, Clark, Binford, Straus and other American archaeologists would characterize it as such; and, interestingly, some French archaeologists do as well.²⁰² But, is French archaeology atheoretical? Often, this attribution is made when French archaeological traditions of the past forty years are juxtaposed with the theoretical landscape of archaeology as practiced in the U.S. and other Anglophone countries -- a landscape in which theory and theoretical debate has been a predominant (and loud) concern. Moreover, the liberal borrowing of concepts and theoretical constructs by Anglophone archaeologists from mid-twentieth century French social theorists and philosophers has provoked curiosity and befuddlement about the failure of French archaeologists to do the same.

²⁰¹ Olivier and Coudart, "French Tradition and the Central Place of History in the Human Science," 364.

²⁰² See Audouze and Leroi-Gourhan 1981, Demoule 1999, and Cleuzio et al 1991.

Many American and British archaeologists equate this 'oversight' with a general lack of concern with and overall use of theory in French archaeology. The situation was best illustrated by several prominent French archaeologists who were asked by two British archaeologists to account for the lack of theory in French archaeology:

Our point of departure is the pressing questions put to us by Ian Hodder and Chris Tilley in a Cambridgeshire pub one evening in March 1986 over an excellent beer; why do French archaeologists, who are fortunate enough to speak the same language and breathe the same air as Althusser, Bourdieu, Foucault, Lévi-Strauss and some others – among them a psychoanalyst whom we have never heard of and who publishes only in very Parisian journals – why do these archaeologists use so little theory?²⁰³

Quite a few French (and some British and American) archaeologists have attempted to answer some version of that question over the past 25 years. Françoise Audouze and André Leroi-Gourhan proposed in the early 1980s that, from a theoretical perspective in archaeology, France was what they termed a "continental insularity."²⁰⁴ They observed that French archaeology, led by Leroi-Gourhan, B. Soudsky, and J.-C. Gardin, did have its own theoretical awakening in the late 1960s and early 1970s, much like that of the New Archaeology, but that it was extremely limited in scope – Bordes was not a part of it – and, unlike in the United States, the prime directive came to be that of methodology.

One does see parallels between the New Archaeology and sentiments proposed by French archaeologists such as Soudsky in 1970:

The road is long, but archaeology is young after all. Young people will break the barricades that surround the sciences and national languages, the impenetrable domains of separate fields, the 'parochial' terminologies and typologies... but we must also change the system of education and training of archaeologists. One must change the universities, academies, journals, monographs, museums, archives, and, above all, the excavations. There is much to be done.²⁰⁵

²⁰³ Cleuziou et al, "The Use of Theory in French Archaeology," 91.

²⁰⁴ Françoise Audouze and Andre Leroi-Gourhan. "France: A Continental Insularity," *World Archaeology* 13, no. 2 (1981): 170-189.

²⁰⁵ B. Soudsky, "Le problème des propriétés dans les ensembles archéologiques," in *Archeologie et calculateurs: problèmes mathématiques et semiologiques*, ed. J.-C. Gardin. (Paris: CNRS, 1970), 53.

But, as Cleuziou and others have documented, young French archaeologists, in response to Soudsky's and others' calls to arms, adopted a strategy that focused on the structures and institutions that supported the research to the detriment of the theory behind the research. Further, "the almost exclusive attention given to techniques of observation and documentation quickly became a negative element, indeed they became ends in themselves."²⁰⁶ In other words, theory was relegated to the sidelines.

In the past fifteen years or so, however, a new take on the role of theory in French archaeology has emerged, one that takes into account the long history and complexity of the Old World archaeological tradition with a nod to its affiliation with the natural sciences and history. In 1999, Laurent Olivier offered the following warning:

It is wrong to consider French archaeology atheoretical. On the contrary if French archaeology appears so poor in its theoretical interpretations, it is because the past was already theorized and the framework of its interpretation was already fixed even before the birth of the discipline. Conversely, in countries where no strong theory of Society exists – in particular, in Americanized countries the past needs to be imperiously theorized, and the question of its interpretation occupies a crucial place in the working of the discipline.²⁰⁷

In this sentiment, one sees allusions to the influence of the natural sciences and, more importantly, history, on the Old World tradition, influences at odds with the anthropological influence on the New World tradition.

Indeed, Sackett conveyed much the same thing from an Americanist perspective in 1991 when he proposed that:

What this boils down to ... is that American archaeologists have no paradigm apart from that furnished by anthropology itself. Indeed the anthropological paradigm is so ingrained in their outlook that they find it difficult to believe that a field of prehistoric research could logically have any other.

If one takes its references to cultures, tribes, styles, and functions literally in the American sense, French systematics does indeed seem anthropologically naïve. Americans often seem unwilling to cultivate the informed empathy needed to

²⁰⁶ Cleuziou, et al. "The use of theory in French archaeology," 99.

²⁰⁷ L. Olivier, "The Origins of French Archaeology," *Antiquity* 73 (1999): 177.

perceive that the French are neither third-rate anthropologists nor (seemingly the only alternative to American eyes) naïve empiricists.²⁰⁸

One could easily turn this observation around and frame it from a French perspective to say that:

What this boils down to ... is that [French] archaeologists have no paradigm apart from that furnished by [the natural sciences or history] itself. Indeed the [natural history/historical] paradigm is so ingrained in their outlook that they find it difficult to believe that a field of prehistoric research could logically have any other.

If one takes its references to cultures, tribes, styles, and functions literally in the [French] sense, [American] systematics does indeed seem [historically] naïve. [The French] often seem unwilling to cultivate the informed empathy needed to perceive that the [Americans] are neither third-rate [(pre)historians] nor (seemingly the only alternative to [French] eyes) [reckless theorists]. [my adaptation of Sackett's quote above]

Whether one thinks that French Paleolithic archaeology is or was theoretical or not, a definitive observation can be made: French prehistorians over the past forty years have largely ignored the tumultuous theory 'wars' that have plagued archaeology in the U.S. One could offer several reasons to explain this, from language and cultural barriers to disciplinary inertia to a dearth of practitioners and resources in comparison with their Anglophone peers. The former (language and cultural barriers) may have been the most profound. The New Archaeology was in ascendancy in the United States for more than a decade before anyone in France translated the primary tenets into French and discussed their implications for the practice of archaeology.²⁰⁹ Coupled with the resistance of many American archaeologists to publishing in French, the language barrier was and still is a major impediment.

Another reason is that Bordes, who dominated much of French Paleolithic archaeology, never displayed an interest in addressing theoretical topics that he believed were so much speculation and, therefore, not worthy of his time -- time better spent focusing on fleshing out

²⁰⁸ Sackett, "Straight Archaeology, French Style," 138.

²⁰⁹ S. Cleuziou et al, "Renouveau des méthodes et théorie de l'archéologie," *Annales. Economie, Société, Civilisation* 1(1973): 35-51.

the dataset that could be used to solve some of the larger, more vexing questions. Leroi-Gourhan, on the other hand, Bordes' only peer in France in terms of stature in the field of Paleolithic archaeology, was keenly interested in theory and did some groundbreaking theoretical work in the 1960s, but his influence within France was limited. Leroi-Gourhan was virtually unknown to Anglophone archaeologists until the 1980s and his influence in France, while significant, failed to ignite the younger generation of scholars the way that Binford did in the United States.

The failure of Leroi-Gourhan to ignite the younger generation of French archaeologists was not an isolated incident. The work of other French archaeologists in the late 1960s and early to mid-1970s such as J.-C. Gardin, who advocated a transition from –etic descriptive units to –emic interpretative units when classifying and interpreting artifacts, and G. Laplace, who proposed an alternative to Bordes' lithic taxonomy, never caught on with prehistorians in France. The reason for this sputtering of theory in French archaeological circles was twofold: the unique social and educational conditions in French universities and the lack of resources.

Several scholars have argued that in French universities, unlike those in the United States, the ability to promulgate an appreciation for theory among students was stymied by the tightly controlled access to teaching and research positions, which effectively promoted a status quo.²¹⁰ In reference to the New Archaeology, Cleuziou and others commented that “if this phenomenon was more pronounced in America, so that developments there received more attention, it was in part due to the amplifying effect of the particular sociological milieu of American universities”²¹¹ Individuals such as Leroi-Gourhan, who came from the social sciences and was a bit of a self-taught renegade, had little opportunity to cultivate a cadre of students of his own and to place them in influential positions. In the late twentieth century, most

²¹⁰ See Scarre 1999, Schnapp 1982, and Cleuziou et al 1991.

²¹¹ Cleuziou, et al. “The use of theory in French archaeology,” 104.

archaeological research in France continued to be dominated by just a handful of academic research centers.

The issue of resources in French archaeology also contributed to the delay in theory-building. Institutional constraints such as the lack of support services for research and too few academic positions as well as the diversion of state funding from traditional to rescue archaeology in the 1970s all acted to subvert research in archaeological theory. The resource situation was so bad by the early 1980s that Audouze and Leroi-Gourhan observed that:

French archaeology is in a virtual state of crisis. Important departments of archaeology with research laboratories of an international level and with specialized libraries are too few ...this may explain why French archaeology is so impervious to European currents of thought, especially theoretical ones.²¹²

Theory was, and to some extent still is, a luxury that French archaeologists have been less able to afford than their American peers. It would be a mistake, however, to equate this with a lack of appreciation or interest in theory. Many French Paleolithic archaeologists have over the past two decades begun to make inroads into theoretical research -- from Jean-Paul Demoule's work on the role of ethnicity and the conceptualization of archaeological cultures (1999), to the concept of the *chaîne opératoire* recently furthered by Boëda (1997) and Pélegrin (1993), to the general studies on the social dimensions of technology by Schlanger (1990) and Pigeot (1990) - - to name just a few. On the whole, although the situation is complex, it would distort any reasonable understanding of the practice of archaeology in France to label it atheoretical. But, it would be fair to say that French archaeologists generally, and French prehistorians specifically, have certainly concerned themselves less with theory and engaged infrequently at best in research activity explicitly devoted to theory than their American peers.

This brings one back to the questions of whether French Paleolithic archaeologists operate within an Old World archaeological tradition and whether that Old World tradition constitutes an intellectual paradigm that is distinctive from its New World counterpart. The

²¹² Audouze and Leroi-Gourhan, "France: A continental insularity," 182-183.

answer is less definitive or conclusive than it is for the New World tradition. There is a more homogeneous self-awareness among most New World archaeologists of their place and role in the advancement of their discipline that is not readily apparent among most French and other Old World archaeologists. French archaeologists lack the overarching and explicit organizing principles that New World archaeologists draw from anthropology, and it is arguable whether history holds an equivalent position in an Old World tradition. The historical approach and an appreciation for the *longue durée*, however, have clearly had a formative impact on the practice of archaeology in France and certainly that is discernible in the work of most Paleolithic archaeologists trained and practicing in an Old World milieu. It is reasonable, in that regard, to conclude that French archaeology is representative of an Old World archaeological tradition, but it is less clear whether the Old World tradition, in fact, has the internal coherence and consistency in logic typical of the classic concept of paradigm.

Categorization of the Old World tradition as a paradigm is attractive from the perspective of using it as a foil against which to probe the limits of a strict anthropological approach to understanding the deep past. It is also the case that casting the Old World tradition in stark paradigmatic terms may have been useful forty years ago, but in the early twenty-first century it may be less useful. In the 1960s, French archaeology was much more cohesive in terms of practice and approach. A reasonable conclusion would be that the Old World tradition then was, in actuality, a classic paradigm. It is less certain that that is the case today. The approach to the past taken by French and other Old World archaeologists is more balanced and inclusive than it had been in the past – this also true of American and other New World archaeologists. As Harrold framed it in 2002:

[d]ifferences between Americans and Europeans persist in the disciplinary and institutional contexts in which Paleolithic archaeology is practiced, the professional formation and career trajectories of archaeologists and the vocabulary they use ... [s]uch real differences encourage the perception that there are deep, even paradigmatic differences in the view points of American and European Paleolithic. My sense is that, two generations after the Bordes-Binford

confrontation, the relationship has become more complex and nuanced, and, for the most part, intellectually productive.²¹³

From the perspective of understanding the deep intellectual divide that separated Bordes from Binford in the 1960s, it also is useful and meaningful to frame the debate as a clash of paradigms. The paradigmatic perspective becomes in that context a convenient heuristic device. This allows the historian of the debate to understand better why Bordes and Binford could not come to a satisfactory resolution. Moreover, it allows the historian to understand how and why subsequent generations could not resolve the debate – stemming from two incommensurable paradigms, a resolution was not possible -- and why advancement of the research on the causes of Mousterian variability inevitably had to come from a new line of inquiries, ones which were influenced by the debate but were separate and distinct. Perhaps most importantly, however, understanding the debate from a paradigmatic perspective allows practitioners and historians alike to decipher the narrative strategies that have been used to frame the debate and its aftermath for subsequent generations.

²¹³ Harrold, "Transatlantic Prehistory: Thoughts on the Encounter between American and European Paleolithic Prehistory," 29.

CHAPTER 6

FRAMES OF REFERENCE FOR A NEW GENERATION

Irrespective of whether one characterizes the Bordes-Binford debate as a clash of incommensurable paradigms, a case of incompatible interpretative traditions, or simply a scholarly disagreement, it is, fundamentally, representative of a particular time in the history of archaeology. This was a period that threw into sharp relief the transition of the discipline from a descriptive to an explanatory science. Like most intellectual transitions, there was no bright line marking where one period ended and a new one began. But, making sense of any pivotal event requires the construction of a system of references and language by which one can situate what is observed, heard, or understood, into a framework that then itself serves as kind of intellectual shorthand. One of the more interesting aspects of the Bordes-Binford debate is how the details and substance have been packaged and communicated. In the United States, the particulars of the debate are known only superficially outside the small professional community of Paleolithic archaeology, yet virtually all casual students of the discipline are introduced to the debate. Also, there is a reference to the debate in most introductory archaeology textbooks written in English and virtually all historical treatments of the rise of the New Archaeology at least mention the Bordes-Binford debate in that context as one of the core developmental events in the history of archaeology over the past forty years. In France, on the other hand, one is hard pressed to find any mention of the debate outside the very circumscribed world of Paleolithic archaeology and even then there are very few attempts to situate the debate in a historical or transatlantic context. This chapter will explore the differences in how French (and other European) and American scholars describe the debate and how it has been represented to a new generation of scholars.

6.1 The Bordes-Binford Debate from an American Perspective

American archaeologists, on the whole, have tended to focus more on the debate than their French colleagues. There are several reasons for this. First, Binford's role as the *de facto* father of the New Archaeology -- as well as his personal charisma -- put him in a highly influential position among archaeologists in the United States. Second, the larger implications of the debate (that is, the difficult question of how to explain artifact variability in the archaeological record) resonated among many American archaeologists who saw a more universal applicability outside the regional and temporal boundaries of the Middle Paleolithic. Finally, the 'story' of the debate captured the imagination and attention of Americans more so than Europeans because of its allegorical nature, reflecting in some ways a uniquely American narrative. Each of these reasons will be addressed more specifically below.

For many American scholars, Binford was the face of the New Archaeology and, in the excitement and enthusiasm of the 1960s, his research and publications were widely distributed and read. He was heralded as a breath of fresh air by those in his circle and/or sympathetic to his message and vilified by those who represented the status quo -- which, one should add, likely increased his stature among the younger set. Consequently, his debate with Bordes generated more interest than might have been accorded to someone less visible in the field. Moreover, his academic appointments at Michigan, Chicago, Santa Barbara, UCLA, and New Mexico (and later at Southern Methodist University) put him in contact with many students, some of whom went on to highly influential positions themselves. Subsequent generations, many of whom were directly or indirectly linked to Binford, were exposed to the debate as part of the fabric of their academic training.

Binford's charisma served him well and he capitablized on it frequently. Several of his peers even went so far as to label Binford the "consummate pitchman".²¹⁴ He routinely wrote -- often with relish -- about his intellectual dust-ups with senior scholars like Bordes. (See Chapter

²¹⁴ O'Brien et al., *Archaeology as Process: Processualism and Its Progeny*, 264.

4 for the story about his first 'formal' debate with Bordes at Bordes' laboratory in Bordeaux in 1965.) Binford was very calculated in his approach to solidifying his position and influence. A telling observation was made by O'Brien, Lyman and Schiffer:

Thus at age thirty-four Lew Binford was declared a heavyweight contender by the preeminent French Paleolithic scholar [Bordes]. And at age forty-one, he told the world about it [in his book *An Archaeological Perspective*]. In his mind he hadn't knocked out any of the champions, but he had stood up to their hardest shots and delivered a few shots of his own.²¹⁵

Whether one admired or hated Lewis Binford, he certainly knew how to get attention. And that attention drew younger scholars into his circle, which in turn influenced them in profound ways. The 'halo' effect from this influence elevated his debate with Bordes among American archaeologists. As Bruce Trigger has noted, this is not an unusual situation. Indeed,

[c]losely argued, passionate, and dramatic debates are a vital part of the adversarial approach that characterizes discourse in the social sciences. Young scholars often initiate controversies in hopes of promoting their careers, while protracted quarrels between established academics fascinate their colleagues. The debate between Lewis and Sally Binford and François Bordes as to whether different Mousterian industries represented tribal groups or specialized tool-kits epitomized the rival claims of the culture-historical and processual approaches of the 1960s and 1970s.²¹⁶

Susan Kus echoed this sentiment when she observed that "too often the way to attention in our discipline... is to wrest agenda setting by producing the next 'heresy-aspiring-to-dogma.'"²¹⁷

Robert Kelly, even more incisively, noted that too often archaeologists "reward polemic, bombast, and showmanship."²¹⁸

²¹⁵ Ibid., 96-97.

²¹⁶ B. Trigger, "Archaeological Theory: The big picture," *The Grace Elizabeth Shallit Lecture at Brigham Young University*, (Salt Lake City, UT: The University of Utah Press, 2003), 10-11.

²¹⁷ S. Kus, "Ideas are like burgeoning grains on a young rice stalk: Some ideas on theory in anthropological archaeology," in *Social theory in archaeology*, ed. M.Schiffer, (Salt Lake City: University of Utah Press, 2000), 169.

²¹⁸ R. Kelly, "Elements of a behavioral ecological paradigm for the study of prehistoric hunter-gatherers," in *Social theory in archaeology*, ed. M.Schiffer, (Salt Lake City: University of Utah Press, 2000), 78.

Polemics and bombast aside, the Bordes-Binford debate did pique the interest of American archaeologists on the merits of its substance as well. The issue of lithic variability was one that crossed regional or temporal specializations. Whether one was interested in the Middle Paleolithic of southwestern France, the Upper Paleolithic sequences of Europe or Australia, or the Paleoindians of the southwestern United States, lithic variability was a phenomenon in need of explanation. Rolland and Dibble proposed that:

it also is true that this debate – the more familiar of which is known as the “Bordes-Binford debate” – is of significance for prehistoric archaeology in all parts of the world and in all time periods where there is a focus on lithic variability. Although the exact nature of the variability may differ, the fundamental underlying causes are universal. This is why the “Bordes-Binford debate” became so famous, even among American archaeologists.²¹⁹

The intense interest in the Bordes-Binford debate was reflective of the need in the discipline to develop an adequate repertoire of explanatory models to address the fundamental question of lithic variability.

That intense interest has yet to subside completely. While it is true that most archaeologists have given up on ‘resolving’ the Bordes-Binford debate, the question still stimulates a tremendous amount of research. Even a cursory review of common archaeology journals turns up dozens of articles related to lithic variability; a good many, it should be noted, also continue to reference the works of both Bordes and Binford. Bisson observed as recently as 2000 that:

Far from receding into the background, Binford’s hypothesis remains sufficiently important that it was subject to a lengthy critique in the recent comprehensive synthesis of the Mousterian by Mellars.²²⁰

²¹⁹ Rolland and Dibble, “A New Synthesis of Middle Paleolithic Variability,” 480.

²²⁰ M. Bisson, “Nineteenth Century Tools for Twenty-First Century Archaeology? Why the Middle Paleolithic Typology of François Bordes Must Be Replaced,” *Journal of Archaeological Method and Theory* 7, no. 1 (2000), 18-19.

The Bordes-Binford debate has seemingly become the routine touchstone for many younger archaeologists pursuing lithic variability as a line of research.

The most enduring aspect of the Bordes-Binford debate from an American perspective may be the one that has least to do with the substance of the debate but much to do with the nature of the debate as an allegory or symbolic narrative. The Bordes-Binford debate was -- and to some extent remains -- a reflection of the unique social and historical trends that swept over America in the 1960s and 1970s. The period was typified by a strong sense of enthusiasm and faith in science.²²¹ American ingenuity and technical prowess was on the rise. NASA was sending men to the moon. American physicians were making great advances against illness and disease. From a social and political perspective, the Baby Boom generation was beginning to make itself felt. Youthfulness and liberalism were rising in value; conservatism and the experience of age were in eclipse.

All of these undercurrents made their way into archaeology as well. Binford epitomized those trends. He preached science and used computers to conduct complex statistical analyses. He was young, and imbued archaeology with newness both in spirit and in name. Bordes, on the other hand, was the antithesis of this. He represented the conservative status quo; he was respected for the deep well of his knowledge of Paleolithic sites and their artifacts and for his status and position, but his approach was not seen as a means to advance the research into new areas of explanation. The debate in the eyes of many American archaeologists was an extension of that larger narrative.

American archaeologists have tended to portray the debate in pugilistic terms. Binford was 'knocking down champions' and 'taking shots' at the older establishment. Brian Fagan characterized the debate as 'hard-fought'.²²² Clive Gamble praised Binford for 'digging up

²²¹ Patterson, *Toward a Social History of Archaeology in the United States*, 1995.

²²² Brian Fagan. "Review of *In Pursuit of the Past* by Lewis Binford." *Antiquity*, (2002): 62-63.

[archaeology's] nineteenth-century roots and burning them."²²³ This is how many American archaeologists continue to represent the debate to the younger generation: scholarly discourse is adversarial, confrontational. Binford himself advocated that approach when he explicitly argued that archaeology must be "disputatious" in order to achieve legitimate standing as a serious science.²²⁴ All that said, however, there has been a curious dearth of detailed scholarly analyses of the debate in its historical and transatlantic context beyond a simple reflection of the larger trends of the New Archaeology. This research turned up no explicit attempts in the literature to address the Bordes-Binford debate as a complete and separate topic of study.

6.2 The Bordes-Binford Debate from a French (European) Perspective

In contrast with the American perspective, French and other European archaeologists have generally been more measured and less interested in expressing their perceptions of the Bordes-Binford debate. There are two distinct threads that run through the literature. One is a conciliatory tone that emphasizes the contributions that Americans such as Binford (and those he inspired to work in the Old World) have made to the progress of Paleolithic research in France. The other is more ambivalent. On the one hand, there is recognition that Bordes' conservative stance may have represented a less progressive, intellectually limited position in terms of advancing our understanding of the deep past; on the other, there is uneasiness with what the debate represents in terms of American involvement on their 'turf.'

In the conciliatory camp, Kozłowski expressed a common sentiment of those who sought to find common ground between the extremes represented by the debate, noting that "it cannot be claimed that European archaeologists do not use anthropological models or that none of the American researchers have ever used the [culture-] historical perspective."²²⁵ Otte

²²³ Clive Gamble, "Review of *In Pursuit of the Past* by Lewis Binford." *Nature*, (2002): 49-50.

²²⁴ Binford, *Debating Archaeology*, 10.

²²⁵ Kozłowski, "American and European Archaeology of the Palaeolithic: How to make use of divergent interpretations?" 35-36.

went further when he stated that the American approach has “complemented the more strictly evolutionary approach normally taken in Europe.”²²⁶ Otte, however, did not always contain his ambivalence:

The outsider's point of view available from the Americas provides a more comprehensive and, inevitably, more 'professional' view, but one totally devoid of imagination. This activity is seen as 'scientific', as if a fear of a humanist level pushed U.S. researchers to seek refuge under the umbrella of science simply and conveniently because it was seen as 'exact' ... American schools should continue to lead European researchers, through their ideas and their constant questioning of historical inertia and willing seduction by nationalist sirens, which seems to spontaneously escape Americans. Finally, we also hope that the European Schools will resist simplistic mechanistic explanations, for example, environmental determinism. Like European history, prehistory evolved in a vague and subtle way, made of thought, art and tradition. No law encloses our quest for identity at every moment of our evolution. Apparently, these two trends complement and enhance, but they should never be confused in order to preserve the respective wealth of the other.²²⁷

Bordes himself even struggled to come to terms with the nationalistic roots of the debate, yet offered a conciliatory explanation that American archaeologists have a unique challenge in reconciling a past record of human behavior with living examples of native peoples. This professional proximity repays, and even demands, an anthropological perspective in ways not required of Europeans.²²⁸

The more current narrative in French archaeology with regard to the Bordes-Binford debate, and more generally the New Archaeology, has reflected a recognition of both the utility and limitations of the more anthropological approach offered by American archaeology and a renewed sense that the historical perspective embodied in the Old World tradition still has value in terms of understanding the past. Also embedded in this current narrative has been suspicion that the New Archaeology has devalued the worth of a uniquely European perspective on the past. A long but revealing series of passages by Otte and Keeley in 1990 offers the best

²²⁶ Otte, “Le rôle des américains dans la recherche du Paléolithique en Europe,” 39.

²²⁷ *Ibid.*, 42.

²²⁸ Bordes, *The Old Stone Age*, 7-8.

example of this ambivalence towards a rigid application of an anthropological approach to the?

Paleolithic of Europe:

This tendency [to focus on local phenomena or events and reinforce ideas of local continuity and evolution] has been exaggerated by the rejection (e.g., by the New Archaeology) of “historical” explanations of prehistoric change such as migration and diffusion in favor of “processual” ones which emphasize internal systemic change. The intellectual focus is no longer on prehistoric sequences of events but on the processes and stages of social evolution.

Underlying such processual studies is the idea of “progress,” which has given a moral color to archaeological studies. Reconstructions proposing that social evolutionary changes in a region are the result of invasion or significant diffusion imply that the inhabitants were “unprogressive” and their cultures “derivative” and uncreative. Thus, implicitly, the “reputation” of nations and regions, as well of the archaeologists identified with them, have become entwined with prehistoric cultural histories and their interpretation. The association of some migrationist and hyperdiffusionist hypotheses with racist and imperialist doctrines has tinged intellectual disesteem with moral opprobrium. The celebrated quarrel (Bordes vs. Binford) over the interpretation of the technical facies of the French Mousterian has never been enlightened by a pan-European view. Such a widening of the field of vision, however, provides a key to the interpretation of this phenomenon.

In the longest view, the most common motor of successive changes in technology, economy, and social life in a given region may be exchange and replacement through contact, conquest, and migration. It seems to us neither intelligent nor accurate to maintain a sharp distinction between change brought about in this way and evolutionary change (i.e., the increase in complexity and/or scale of an economic, political, or social unit).

Migration and diffusion must be added to processual interpretations of change along with “internal” factors. They are *processes* with recurrent features, not merely historical accidents. If prehistorians are truly to account for human prehistory they cannot allow theoretical myopia, regional chauvinism, and a local focus to dismiss or obscure the large-scale processes of human mobility and facility for intellectual borrowing”.²²⁹

The subtext of resistance, and even a little anger, in passages such as these represent, to some extent, what Audouze and Leroi-Gourhan have labeled a typically superficial French reaction to “the penetration of foreign ideas.”²³⁰ Some American archaeologists, however, have

²²⁹ Otte and Keeley, “The Impact of Regionalism on Paleolithic Studies,” 577-82.

²³⁰ Audouze and Leroi-Gourhan, “France: A continental insularity,” 181.

stoked this resistance with their less than tactful characterizations of their Old World colleagues.

Clark, for example, had this to say about European archaeologists' use of the Bordes taxonomy:

The conventional culture-stratigraphic analytical units used by European prehistorians arose as an accident of history in late-19th and early 20th century France ... and, although they [Bordes' standard tool types] were created in the mind of a mid-20th-century Frenchman, were widely believed to replicate tool forms present in the minds of his long-dead ancestors.²³¹

This type of statement reflects the casually patronizing tone that many American archaeologists too often have taken when engaged in dialogue with European scholars.

This has reinforced a negative view of the New Archaeology in France and elsewhere in Europe that began early on and persists even now. Harrold summed this up best in 2002 when he observed that:

The New Archaeology in its early years was characterized by a high degree of confidence – many would say hubris – that many aspects of past cultural systems could be reconstructed, despite the incomplete nature of the archaeological record, if correct scientific methodology were used. European prehistorians often saw such confidence as naïve, based on a lack of familiarity with the complexities and limitations of the deep Paleolithic record. American archaeologists were seen as too trusting in the possibilities of ethnographic analogy, in idealized notions of scientific method, and in computers and statistics.²³²

That hubris and naïvete is most evident when it comes to the recognition by Americans of the scholarly contribution of Old World archaeologists to the study of the Paleolithic. This most likely is attributable to a persistent trend among many American archaeologists to overlook work that is not published in English. That hopeless monolingualism has prompted charges from

²³¹ Clark and Lindly, "On Paradigmatic Biases and Paleolithic Research Traditions," 578.

²³² Harrold, "Transatlantic Prehistory: Thoughts on the encounter between American and European Paleolithic Prehistory," 29.

European scholars running the gamut from the relative mild excoriation of “profound ignorance” to the more nefarious claim of “linguistic imperialism.”²³³

On the whole, however, the one aspect of French literature that is most significantly different from that in the United States is the lack of attention given, overall, to general theoretical and epistemological debate or discussion within archaeology. Indeed, it is almost a cliché how difficult it is to find examples of such research among French archaeologists. Searches in large worldwide databases for general works in French on archaeological method, theory or philosophy result in very few examples – generally in the neighborhood of one to two hundred – most of which were written in the past fifteen to twenty years. In contrast, a similar search for such general works in English number in the many hundreds, a significant number dating back forty to fifty years or more. French archaeologists have also noted the scarcity. Cleuziou observed in the early 1990s that “[g]eneral books on archaeology are rare in France and ignore such debates [like the Bordes-Binford debate] entirely. The first book to deal with them was Moberg’s *Introduction à l’archéologie* [1976], a translation and revision of the original Swedish text by Moberg himself.”²³⁴

This paucity of concern with theory and philosophy within the field in France is most pronounced among the cadre of French archaeologists who were the intellectual descendants of Bordes and the more conservative ‘Bordeaux School’. A few French archaeologists have attributed this to the “general preference by [French] archaeologists for empirical and pragmatic procedures” over that of theory.²³⁵ That sentiment echoes Sackett’s characterization of “straight archaeology, French style.” The general preference for pragmatism and the nuts and bolts of ‘doing’ archaeology has been reinforced by a host of factors ranging from simple inertia

²³³ See Otte and Keely, “On Paleolithic Research Traditions: A response to Clark and Lindly,” 408 and Cleuziou and Demoule, “Situation de l’archéologie théorique,” *Les Nouvelles de l’Archéologie* 3 (1980): 7-15.

²³⁴ Cleuziou et al., “The Use of Theory in French Archaeology,” 105.

²³⁵ *Ibid.*, 181.

to complex academic and political structures which act to perpetuate a belief that archaeologists must concentrate on the collection of data and put off the grander discussions to a future time.

In 1999, Olivier offered, for example, the following observation on the state of theory in French archaeology: “[a]bove all, archaeology is little debated in France, since it does not have strong presence in university teaching outside Paris. French archaeology remains essentially a state enterprise, run by bureaucrats and not by researchers.”²³⁶ Similarly, Cleuziou and others have alluded to the fact that the structural nature, both social and political, of French universities themselves had a dampening effect on the ability of theoretical research concerns to gain traction and support. In reference to why the New Archaeology never took hold in France, they noted that “if this phenomenon was more pronounced in America, so that developments there received more attention, it was in part due to the amplifying effect of the particular sociological milieu of American universities.”²³⁷ In most French universities, then, archaeology was and largely remains a practical, not a theoretical, pursuit, a status that has deterred much intellectual discourse on the larger questions stemming from the Bordes-Binford debate.

Americans with long-time ties to and experience working in France and other parts of Europe have also observed and commented on this tendency to shy away from the more theoretical or philosophical concerns. Harrold has noted that American challenges to French archaeological theory or method failed to “generate many lengthy rejoinders or discussions in the French literature,” generally due to the fact that “French paleolithic archaeologists were and are less given than their New World counterparts to epistemological debate.”²³⁸ Clark, more trenchantly, asserts that the tendency to wrestle with the larger question of theory and process that so marked the New Archaeology “had relatively little impact on Paleolithic archaeology,

²³⁶ Olivier, “The Origins of French Archaeology,” 176.

²³⁷ Cleuziou, et al. “The use of theory in French archaeology,” 104.

²³⁸ Harrold, “The Elephant and the Blind Men,” 169.

especially as practiced by Old World workers, and that epistemological angst is mostly confined to anglophone research traditions."²³⁹

On the other hand, there were and are pockets of interest in the less pragmatic aspects of archaeology among French archaeologists. For example, many past and current scholars who were influenced by Leroi-Gourhan and his 'Paris School' displayed much greater interest in theory and epistemology. Laurent Olivier, Alain Schnapp, Jean-Paul Demoule, and François Audouze have all written extensively on theory and the implications of epistemology. There is still a notable gap, however, in the French literature on discussion of theory, epistemology, method, and philosophy. Some of this is merely a matter of lag time, as Cleuziou and others have noted:

it was not until the arrival of *Nouvelles de l'archéologie*, a journal dealing with the administrative information, research policy and the scientific aspects of archaeology, established in 1979 by Braudel's Maison des sciences de l'homme, that contacts with the Anglo-American world and discussions on theory and concepts became a consistent feature of French archaeology.²⁴⁰

One presumes that this gap will eventually close. By and large, however, the ambivalent perception of the Bordes-Binford debate and the broader involvement of Americans in French Paleolithic archaeology has been and continues to be the dominant narrative of French and other European archaeologists.

6.3 Conclusion

Most histories of archaeology that have addressed the Bordes-Binford debate emphasize the juxtaposition of the aims and practices of the more descriptive culture-historical archaeology with that of the more explanatory New Archaeology. Bordes and Binford are portrayed as archetypes representing the tensions inherent in the transition to a newer, better, more scientific (more American?) mode of thought. But the Bordes-Binford debate did not occur in an intellectual vacuum, and archaeology as practiced by French and French-trained

²³⁹ Ibid., 20.

²⁴⁰ Cleuziou, et al. "The use of theory in French archaeology," 106.

archaeologists did not meet a sudden methodological demise in its wake. To be sure, the Bordes-Binford debate reflected the differences between the Old World and the New relative to the practice of archaeology as a means to explain human behavior and cultural change, and the reverberations of the debate can still be felt today on both sides of the Atlantic. The debate was nuanced and complex and both protagonists dramatized the narrative to construct and advance their respective arguments. Even more interesting are the ways in which subsequent generations of scholars have selected particular narrative strategies to describe and make meaning of the debate.

The primary intent of this research has been to situate the Bordes-Binford debate within the larger context of the history of archaeology with specific consideration of how the debate reflected the different interpretative traditions of French and American archaeologists. The research has demonstrated that the Bordes-Binford debate was, in fact, reflective of latent tensions in the discipline and of the larger socio-cultural landscape in which Bordes and Binford operated. Moreover, the research has shown that the divergent intellectual trends discernible in the Bordes-Binford debate reflected fundamental differences regarding the study and interpretation of the deep past, differences that were profoundly influenced by the conceptualization of the discipline in French and American intellectual contexts. These differences could certainly, in some contexts, be construed as paradigmatic in nature. Moreover, the research showed that the Bordes-Binford debate continues to serve as a useful backdrop against which to examine the larger intellectual trends associated with the New Archaeology and the allegorical narrative that has grown up around them.

And yet despite the amount of spilled ink furthering the Bordes-Binford debate and the commonly held recognition on the part of most experts in the field of the fundamental theoretical and methodological distinctions between French and American archaeological traditions, the practice of Paleolithic archaeology by French and American archaeologists has changed very little. Binford himself noted in the preface of his 2002 edition of his book *In Pursuit of the Past*,

published initially in 1983, that he had seen “an enormous growth in the literature with essentially very little progress!”²⁴¹

For the most part, the initial and lingering effects of the Bordes-Binford debate have over the past almost fifty years become compartmentalized in a state of relative and mutual tolerance. A common sentiment expressed by Lawrence Straus is that “neither point of view is necessarily all right or all wrong.”²⁴² And, while some practitioners in the field continue to call for an approach of “critically self-conscious science” -- presumably equated with an American version of anthropological archaeology -- in the context of Paleolithic archaeological research these questions are often divorced from practical field work.²⁴³ More Americans are now active in collaborative research projects in all areas of the Old World than at any point in the past, often resulting in many positive outcomes. The larger theoretical questions such as those embodied in the Bordes-Binford debate do not generally inform basic field research, irrespective of the nationality of the principal investigator, although they continue to receive significant attention in American journals of archaeological research. Overt tensions between the two intellectual traditions are very infrequent, becoming more evident in the literature only when scholars tackle larger, more abstract theoretical or epistemological questions.

This research is but one step toward a more robust understanding of the Bordes-Binford debate and its impact. Further research will continue to refine our understanding of its historical importance. Nonetheless, one can conclude that an understanding of the disciplinary narrative embedded in the Bordes-Binford debate is essential in tracing the transatlantic influences that continue to shape the field of Paleolithic archaeology. An observation by Frank Harrold most eloquently sums up the current state of Paleolithic archaeology in the aftermath of the Bordes-Binford debate:

²⁴¹ Binford, *In Pursuit of the Past*, 233-234.

²⁴² Straus, “American Perspectives on the European Upper Paleolithic? An introduction,” 3.

²⁴³ Clark, “Observations on paradigmatic bias in French and American Paleolithic Archaeology,” 22-23.

[T]here are surely some important transatlantic differences in how prehistory is performed and written ... [but] forty years of interaction, argument, cooperative research, and accommodation have led to a complex intellectual landscape of methodological and theoretical approaches and scholarly conclusions influenced, but not determined, by nationality.²⁴⁴

It is anticipated that the Bordes-Binford debate will continue to serve as a dramatic foil for future scholars navigating the cultural complexities of that intellectual landscape. But, in the spirit of Bordes' and Binford's passionate -- yet never rancorous -- disagreement, perhaps that is the most fitting outcome.

²⁴⁴ Harrold, "Transatlantic Prehistory: Thoughts on the Encounter Between American and European Paleolithic Prehistory," 30.

REFERENCES

- Abbie, A., Francois Bordes, Dorothy A. E. Garrod, P. V. Tobias, Henry V. Vallois, A. Wiercinski, and C. L. Brace. "More on the Fate of the "Classic" Neanderthals," *Current Anthropology* 7, no. 2 (1966): 204-214
- Anderson-Gerfaud, Patricia. "Aspects of Behavior in the Middle Paleolithic: Functional analysis of stone tools from southwestern France," in *The Emergence of Modern Humans: An archaeological perspective*, ed. P. Mellars, Ithaca, NY: Cornell University Press, 1990.
- Anthony, David. "Migration in Archeology: The baby and the bathwater," *American Anthropologist* 92, no. 4 (1990): 895-914.
- Audouze, Françoise. "New Advances in French Prehistory," *Antiquity* 73 (1999): 167-169.
- Audouze, Françoise and Andre Leroi-Gourhan. "France: A continental insularity," *World Archaeology* 13, no. 2 (1981): 170-189.
- Barton, C. Michael. "Beyond Style and Function: A view from the Middle Paleolithic," *American Anthropologist* 92 (1990): 57-2.
- Bennett, John. "Recent Developments in the Functional Interpretation of Archaeological Data," *American Antiquity* 9, no. 2 (1943): 208-219.
- Beyries, Sylvie. *Variabilité de l'Industrie Lithique au Moustérien: Approche Fonctionnelle sur Quelques Gisements Français*. Oxford: BAR International Series S238, 1987.
- _____. "Functional Variability of Lithic Sets in the Middle Palaeolithic," in *Upper Pleistocene Prehistory of Western Eurasia*, eds. H. Dibble and A. Montet-White, Philadelphia: University Museum Press, 1988.
- Bietti, A. "The Mousterian Complexes of Italy: A search for cultural understanding of the industrial assemblages," *Anthropos* 21 (1982): 237-251.
- Binford, Lewis R. "Archaeology as anthropology," *American Antiquity*. 28 (1962): 217-225.
- _____. "Archaeological systematics and the study of culture process," *American Antiquity* 31 (1965): 203-210.
- _____. "Smudge pits and hide smoking: the use of analogy in archaeological reasoning," *American Antiquity* 32 (1967): 1-12.

- _____. "Some comments on historical versus processual archaeology," *Southwestern Journal of Anthropology* 24 (1968): 267-275.
- _____. "Comment," *Current Anthropology* 8 (1967): 234-235.
- _____. "Archaeological perspectives." in *New Perspectives in Archeology*, eds. S.R. Binford and L.R. Binford. Chicago: Aldine, 1968.
- _____. *An Archaeological Perspective*. New York: Academic Press, 1972.
- _____. "Contemporary model building; paradigms and the current state of Paleolithic research," in *Models in Archaeology*, ed. D.L. Clarke. London: Methuen, 1972.
- _____. "Interassemblage variability-the Mousterian and the 'functional' argument," in *The Explanation of Culture Change*, ed. Colin Renfrew. London: Duckworth, 1973.
- _____. *For Theory Building in Archaeology*. New York: Academic Press, 1977.
- _____. *Bones: Ancient Men and Modern Myths*, New York: Academic Press, 1981.
- _____. *Working at Archaeology*. New York: Academic Press, 1983.
- _____. *Debating Archaeology. Studies in archaeology*. San Diego: Academic Press, 1989.
- _____. *In Pursuit of the Past*. London: Thames and Hudson, 2002.
- _____. "In Pursuit of the Future." In *American Archaeology Past and Future: A celebration of the Society for American Archaeology 1935-1985*. eds. D.J. Meltzer, D.D. Fowler, and J.A. Sabloff. Washington, D.C.: Smithsonian Institution Press, 1986.
- _____. "Data, relativism and archaeological science," *Man* 22 (1987): 391-404.
- Binford, Lewis R., and Sally R. Binford. "A preliminary analysis of functional variability in the Mousterian of the Levallois Facies," *American Anthropologist* 68 (1966): 238-295.
- Binford, Lewis R. and Jeremy Sabloff. "Paradigms, systematics, and archaeology," *Journal of Anthropological Research* 38 (1982): 137-152.
- Binford, Sally R. "Early Upper Pleistocene Adaptations in the Levant," *American Anthropologist* 70, no. 4 (1968): 707-717.
- Binford, Sally R. and Lewis R. Binford. *New Perspectives in Archeology*. Chicago: Aldine, 1968.
- _____. "Stone Tools and Human Behavior," *Scientific American* 220 (1969): 70-84.
- Bisson, Michael. "Nineteenth Century Tools for Twenty-First Century Archaeology? Why the Middle Paleolithic Typology of François Bordes Must Be Replaced," *Journal of Archaeological Method and Theory* 7, no. 1 (2000): 1-48.

- Boëda, E. "Le concept Levallois et évaluation de son champ d'application," in *L'homme de Néandertal, vol. 4: La technique*, eds. L. Binford and J-Ph. Rigaud, Liège: Université de Liège, 1988.
- _____. "Approche de la Variabilité des Systèmes de Production Lithique des Industries du Paléolithique Inférieur et Moyen: Chronique d'une variabilité attendue," *Technique et Culture* 17-18 (1991): 37-79.
- Bordes, François. "Principes d'une méthode d'étude des techniques de débitage et de la typologie du Paléolithique ancien et moyen," *L'Anthropologie* 54 (1950): 19-34.
- _____. "Levalloisien et Moustérien," *Bulletin de la Société Préhistorique Française*, n° 4, (1953): 226-235.
- _____. "Essai de classification des industries 'moustériennes.'" *Bulletin de la Société Préhistorique Française* 50 (1953): 457-466.
- _____. *Typologie du Paléolithique ancien et moyen*, Delmas: Publications de l'Institut de Préhistoire de l'Université de Bordeaux, Mémoire n° 1, 1961.
- _____. "Mousterian Cultures in France," *Science* 134 (1961): 803-810.
- _____. *The Old Stone Age*. New York: McGraw-Hill, 1968.
- _____. "In Memory of Raymond Vaufrey," *Current Anthropology* 9, no. 4 (1968): 337-338.
- _____. "Reflections on Typology and Techniques in the Palaeolithic," *Arctic Anthropology* (1969): 1-29.
- _____. *A Tale of Two Caves*, New York: Harper and Row, 1972.
- _____. "On the chronology and contemporaneity of different palaeolithic cultures in France," in *The Explanation of Culture Change*, ed. Colin Renfrew. London: Duckworth, 1973.
- _____. "Typological Variability in the Mousterian Layers at Pech de l'Aze I, II, and IV," *Journal of Anthropological Research* 34, no. 2 (1978):181-193.
- _____. *Leçons sur le Paléolithique*, Paris: CNRS, 3 vol, 1984.
- Bordes, François and Denise De Sonneville-Bordes. "The Significance of Variability in Paleolithic Assemblages," *World Archaeology* Vol. 2, No. 1 (1970): 61-73.
- Bordes, F., J.-P. Rigaud, and D. De Sonneville-Bordes. "Des buts, problèmes et limites de l'archéologie Paléolithique," *Quaternaria* 16 (1972): 15-34.

- Bouchud, J. *Essai sur la Renne et la Climatologie et Paléolithique Moyen et Supérieur*. Périgueux: Magne, 1966.
- Braudel, Fernand. *On History*. Trans. by Sarah Matthews, Chicago: University of Chicago Press, 1980.
- _____. *L'identité de la France*. Paris: Arthaud, 1986.
- Breuil, Henri. "Essai de stratigraphie des dépôts de l'Age de Renne," *C.R. Congrès Préhistorique Francaise*, Perigueux (1905): 74-80.
- _____. "Les subdivisions du Paléolithique supérieur et leur signification," *C.R. 14^{eme} Congrès Préhistorique Francaise*. Geneva (1913): 165-238.
- Brézillon, Michel. "La dénomination des objets de pierre taillée." In *Ive supplement a Gallia Préhistoire*. Paris: CNRS, 1968.
- Broderick, Alan H. *Father of Prehistory*. New York: William Morrow & Company, 1963
- Caldwell, Joseph. "The New American Archaeology." *Science* 129 (1959): 303-307.
- Chase, Phillip, "Relationships between Mousterian Lithic and Faunal Assemblages at Combe Grenal," *Current Anthropology* 27 (1986): 69-71.
- Chazan, Michael. "Conceptions of Time and the Development of Paleolithic Chronology," *American Anthropologist* Vol. 97, No. 3 (1995): 457-467.
- Childe, V. Gordon. "Changing Methods and Aims in Prehistory," *Proceedings of the Prehistoric Society* 1 (1935): 1-15.
- _____. *The Danube in Prehistory*. Oxford: Oxford University Press, 1929.
- Clark, Geoffrey A. "Observations on paradigmatic bias in French and American Paleolithic Archaeology." In *The Role of American Archaeologists in the Study of the European Upper Paleolithic*, ed. Lawrence G. Straus. BAR International Series 1048. Oxford, England: Archaeopress, 2002.
- _____. "Neandertal Archaeology: Implications for Our Origins," *American Anthropologist*, 104 (2002): 50-67.
- _____. "Highly visible, curiously intangible," *Science* 283 (1999): 2029-2032.
- _____. "The Middle-Upper Paleolithic Transition in Europe: an American perspective," *Norwegian Archaeological Journal* 30 (1997): 25-53.
- _____. "Paradigms in science and archaeology," *Journal of Archaeological Research* 1 (1993): 203-234.

- _____. "A paradigm is like an onion: reflections on my biases." in *Perspectives on the Past*, ed. G.A. Clark. Philadelphia, PA: University of Pennsylvania Press, 1991.
- Clark, Geoffrey A. and John M. Lindly. "On Paradigmatic Biases and Paleolithic Research Traditions," *Current Anthropology* 32 (1991): 577-587.
- Clarke, David. *Analytical Archaeology*. London: Methuen, 1968.
- _____. "Models and paradigms in contemporary archaeology." In *Models in Archaeology*, ed. D. Clarke. London: Methuen, 1972.
- Cleuziou, Serge, Anick Coudart, Jean-Paul Demoule and Alain Schnapp. "The use of theory in French archaeology." in *Archaeological Theory in Europe: The last three decades*, ed. Ian Hodder. New York: Routledge Press, 1991.
- Cleuziou, Serge and Jean-Paul Demoule. "Situation de l'archéologie théorique," *Nouvelles de l'Archeologie*. 3 (1980): 7-15.
- Cleuziou, Serge, Jean-Paul Demoule, Alain Schnapp, and Annie Schnapp. "Renouveau des méthodes et théorie de l'archéologie," *Annales. Economie, Societe, Civilisation* 1(1973): 35-51.
- Close, Angela, "The Identification of Style in Lithic Artefacts." *World Archaeology* 10 (1978): 223-36.
- Colin-Simard, Jean-Claude. *Découverte archéologique de la France*. Paris: Amiot-Dumont, 1955.
- Collins, Desmond. "Culture Traditions and Environment of Early Man," *Current Anthropology* 10, no. 4 (1969): 267-316.
- Courbin, Paul. *Qu'est-ce que l'archéologie? Essai sur la nature de la recherche archéologique*. Paris: Payot, 1982.
- Daniel, Glynn. *A Hundred Years of Archaeology*. London: Duckworth, 1950.
- _____. *The Origins and Growth of Archaeology*. New York: Crowell, 1968.
- _____. *A Hundred and Fifty Years of Archaeology*. Cambridge, Mass: Harvard University Press, 1976.
- _____. *Towards a History of Archaeology: Being the Papers Read at the First Conference on the History of Archaeology in Aarhus, 29 August-2 September 1978*. London: Thames and Hudson, 1981.
- Demoule, Jean-Paul. "Ethnicity, Culture and Identity: French archaeologists and historians," *Antiquity* 73 (1999): 191-199.

- Dibble, Harold L. "Raw-Material Variation in Levallois Flake Manufacture," *Current Anthropology* 26, no. 3 (1985): 391-393.
- _____. "Mousterian Assemblage Variability on an Interregional Scale," *Journal of Anthropological Research* 47, no. 2 (1991): 239-257.
- Dibble, Harold, and André Debenath. "Paradigmatic differences in a collaborative research project." in *Perspectives on the Past*, ed. by G.A. Clark. Philadelphia, PA: University of Pennsylvania Press, 1991.
- Dibble, Harold and Nicolas Rolland. "On Assemblage Variability in the Middle Paleolithic of Western Europe: History, perspectives, and a new synthesis," in *The Middle Paleolithic: Adaptation, Behavior, and Variability*, eds. H. Dibble and P. Mellars, Philadelphia: University Museum Press, 1992.
- Dunnell, Robert C., "Five Decades of American Archaeology," in *American Archaeology Past and Future: A celebration of the Society for American Archaeology 1935-1985*, eds. D. Meltzer, D. Fowler, and J. Sabloff, Washington: Smithsonian Institution Press, 1986.
- Fagan, Brian M. Review of *In Pursuit of the Past* by Lewis Binford," *Antiquity* 58 (1984): 62-63.
- Febvre, Lucien Paul Victor. "Vers une autre histoire," *Revue de Métaphysique et de Morale* 58 (1949): 225-247.
- _____. *Combats pour l'histoire*. Paris: A. Colin, 1953.
- Freeman, Leslie G. "The nature of Mousterian facies in Cantabrian Spain," *American Anthropologist* 68, no.2 (1966): 230-237.
- Frison, George C. "A Functional Analysis of Certain Chipped Stone Tools," *American Antiquity* 33, no. 2 (1968): 149-155.
- Gallay, A. "Logicism: a French view of archaeological theory founded in computational perspective," *Antiquity* 63 (1989): 27-39.
- Gamble, Clive. "Review-*In Pursuit of the Past* by Lewis Binford," *Nature* 303 (1983): 49-50.
- Gardin, Jean-Claude. "Problèmes d'analyse descriptive en archéologie." in *Etudes archéologiques*, ed. P. Courbin, 1963.
- _____. "Methods for the Descriptive Analysis of Archaeological Material," *American Antiquity* 32, no. 1 (1967):13-30.
- _____. *Archéologie et calculateurs: problèmes mathématiques et sémiologiques*. Paris: CNRS, 1970.
- _____. *Archaeological Constructs: an Aspect of Theoretical Archaeology*. Cambridge: Cambridge University Press, 1979.

- Garrod, Dorothy. "Henri Breuil: 1877-1961," *Man* 61 (1961): 205-207.
- Geneste, Jean-Michel. *Analyse Lithique d'Industries Moustériennes du Périgord: une approche technologique du comportement des groupes humains au Paléolithique moyen*. Doctoral Dissertation, Université de Bordeaux, 1985.
- Gordon, David. "Mousterian Tool Selection, Reduction, and Discard at Ghar, Israel," *Journal of Field Archaeology* 20, no. 2 (1993): 205-218.
- Harris, Marvin. *The Rise of Anthropological Theory*. New York: Thomas Y. Crowell Co., 1968.
- Harrold, Francis B. "Transatlantic Prehistory: Thoughts on the Encounter Between American and European Paleolithic Prehistory." in *The Role of American Archeologists in the Study of the European Upper Paleolithic*, ed. Lawrence G. Straus. BAR International Series 1048. Oxford, England: Archaeopress, 2002.
- _____. "The Elephant and the Blind Men," in *Perspectives on the Past*, ed. G.A. Clark, Philadelphia: University of Pennsylvania Press, 1991.
- Hinsley, Curtis M. *Savages and Scientists: The Smithsonian Institution and the Development of American Anthropology, 1846-1910*. Washington, D.C.: Smithsonian Institution Press, 1981.
- Hiscock, Peter and Chris Clarkson. "Retouched Notches at Combe Grenal (France) and the Reduction Hypothesis," *American Antiquity* 72 (1) 2007: 176-190.
- Jelinek, Arthur, "Obituary: François Bordes, 1919-1981", *American Antiquity* 47, no.4 (1982): 785-792.
- _____. "Form, Function, and Style in Lithic Analysis," in *Cultural Change and Continuity: Essays in Honor of James Bennett Griffin*, eds. J. Griffin, and C. Cleland, New York: Academic Press, 1976.
- _____. "Lithic Technology Conference, Les Eyzies, France," *American Antiquity* 31, no. 2 (1965): 277-279.
- Kehoe, Alice B. *The Land of Prehistory: A Critical History of American Archaeology*. New York: Routledge, 1998.
- Kelly, Robert L. "Elements of a behavioral ecological paradigm for the study of prehistoric hunter-gatherers," in *Social theory in archaeology*, ed. M.Schiffer, Salt Lake City: University of Utah Press, 2000.
- Kidder, Alfred V. *An Introduction to the Study of Southwestern Archaeology With a Preliminary Account of the Excavations at Pecos*. New Haven: Pub. for the Department of Archaeology, Phillips Academy, Andover, Mass, 1924.

- Kluckhohn, Clyde. "The Conceptual Structure in Middle American Studies," in *The Maya and Their Neighbors: Essays on Middle American Anthropology and Archaeology*, ed. C.L. Hay, New York: Dover Publications, 1977.
- Kozłowski, Janusz K. "American and European Archaeology of the Palaeolithic: How to make use of divergent interpretations?" in *The Role of American Archeologists in the Study of the European Upper Paleolithic*, ed. Lawrence G. Straus. BAR International Series 1048. Oxford, England: Archaeopress, 2002.
- Kuhn, Steven. *Mousterian Lithic Technology: An ecological perspective*. Princeton, NJ: Princeton University Press, 1995.
- Kuhn, Thomas S. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press, 1962, 1970.
- Kus, Susan. "Ideas are like burgeoning grains on a young rice stalk: Some ideas on theory in anthropological archaeology," in *Social theory in archaeology*, ed. M. Schiffer, Salt Lake City: University of Utah Press, 2000.
- Laming-Empeire, Annette. *Origines de l'archéologie préhistorique en France, des superstitions médiévales à la découverte de l'homme fossile*. Paris: A. et J. Picard, 1964.
- Laplace, Georges "Essai de typologie systematique," *Annali dell'Universita di Ferrara* 15 (1964): 1-85.
- _____. *Recherches sur l'origine et l'évolution des complexes leptolithiques*. Rome: Ecole française de Rome, 1966.
- Lartet, Edouard. "Sur l'ancienneté géologique de l'espèce humaine dans l'Europe occidentale," *Annales des Sciences Naturelles (Zoologie)* 14 (1860):117-122.
- _____. "Nouvelles recherches sur la coexistence de l'homme et des grands mammifères fossiles réputés caractéristiques de la dernière période géologique," *Annales des Sciences Naturelles (Zoologie)* 15 (1861):177-253.
- Lartet, Edouard and Henry Christy. "Cavernes du Périgord: Objets gravés et sculptés des temps préhistoriques dans l'Europe Occidentale," *Revue Archeologique* 1 (1864):233-270.
- _____. *Reliquiae Aquitanicae: Being Contributions to the Archaeology and Paleontology of Périgord and the Adjoining Provinces*. Rupert Jones, ed. London: Williams and Norgate, 1875.
- Laville, Henri, "The Relative Position of Mousterian Industries in the Climatic Chronology of the Early Wurm in the Périgord," *World Archaeology* 4 (1973): 323-29.
- _____. *Climatologie et chronologie du Paléolithique en Périgord*. Editions du Laboratoire de Paléontologie Humaine et de Préhistoire, Memoire 4. Provence: Université de Provence, 1975.

- Leroi-Gourhan, A. and Brézillon, M. "L'habitation magdalénienne no. 1 de Pincevent, près Montereau (Seine-et-Marne)," *Gallia Préhistoire* 9 (1966): 263-85.
- _____. "Fouilles de Pincevent: Essai d'analyse ethnographique d'un habitat magdalénien (la section 36)." In *Ville Supplement à Gallia Préhistoire*. Paris: CNRS, 1972.
- Lubbock, John. *Prehistoric Times As Illustrated by Ancient Remains and the Manners and Customs of Modern Savages*. Oosterhout, Netherlands: Anthropological Publications, 1865, reprint 1969.
- _____. *The Origin of Civilisation and the Primitive Condition of Man*. Chicago: University of Chicago Press, 1870, reprint, 1978.
- Malasse, Anne Dambricourt, Andre Debénath, and Jacques Pelegrin. "On New Models for the Neanderthal Debate," *Current Anthropology* 33, no. 1 (1992): 49-54.
- Meignen, L., "Un exemple de comportement technologique différentiel selon les matières premières: Marillac, couches 9 et 10." in *L'homme de Néandertal, vol. 4: La technique*, eds. L. Binford and J-Ph. Rigaud, Liège, Belgium: Université de Liège, 1988.
- Mellars, Paul. *The Neanderthal Legacy: An Archaeological Perspective from Western Europe*. Princeton, N.J.: Princeton University Press, 1996.
- _____. "The Character of the Middle-Upper Palaeolithic Transition in Southwest France." in *The Explanation of Culture Change*, ed. Colin Renfrew. London: Duckworth, 1973.
- _____. "Some Comments on the Notion of 'Functional Variability' in Stone-Tool Assemblages," *World Archaeology* 2, no. 1 (1970): 74-89.
- _____. "Sequence and Development of Mousterian Traditions in South-Western France," *Nature* 205 (1965): 626-627.
- Michelson, Annette. "In Praise of Horizontality: André Leroi-Gourhan 1911-1986," *October* 37 (1986): 3-5
- Montet-White, Anta. "Denise de Sonneville-Bordes: 1919-2008," *Paleoanthropology* (2009): 1-2.
- Morgan, Lewis Henry. *Ancient Society*. The John Harvard library. Cambridge: Belknap Press of Harvard University Press, 1877, reprint, 1964.
- Mortillet, Gabriel de. "Classification des ages de la pierre. Classification des diverses périodes de l'age de la pierre." *Congres International d'Anthropologie et d'Archéologie Préhistorique C.R. 6 (Brussels)* (1872): 432-447.
- _____. "Classification des ages de la pierre," *Revue d'Anthropologie* 1 (1872): 432-435.
- _____. "Les Etudes préhistoriques devant les orthodoxies," *Revue d'Anthropologie* 1 (1875): 120.

- _____. *Le Préhistorique: Antiquité de l'Homme*. Paris: Reinwald, 1883.
- Mortillet, Gabriel de and Adrien de Mortillet. *Le Musée Préhistorique*. Paris: Reinwald, 1881.
- Movius, Hallam, ed. *Excavations of the Abri Pataud, Les Eyzies (Dordogne)*, (Cambridge, MA: Peabody Museum of Archaeology and Ethnology, 1975.
- _____. "Old World Prehistory," in *Anthropology Today; an Encyclopedic Inventory*, ed. Alfred Louis Kroeber, Chicago: Univ. of Chicago Press, 1953.
- Movius, Hallam, and Judson, S. *The Rock Shelter of La Columbière: Archaeological and geological investigations of an Upper Perigordian site near Ponci (Ain)*. Cambridge, MA: Peabody Museum of Archaeology and Ethnology, 1956.
- Nelson, Nels C. "Chronology of the Tano Ruins, New Mexico," *American Anthropologist* 18, no. 2 (1916):159-180.
- O'Brien, Michael J., R. Lee Lyman, and Michael B. Schiffer. *Archaeology as Process: Processualism and Its Progeny*. Salt Lake City, UT: University of Utah Press, 2005.
- O'Brien, Michael J., and R. Lee Lyman. *James A. Ford and the Growth of Americanist Archaeology*. Columbia, MO: University of Missouri Press, 1998.
- Olivier, Laurent. "The Origins of French Archaeology," *Antiquity* 73 (1999): 176-178.
- Olivier, Laurent and Anick Coudart. "French Tradition and the Central Place of History in the Human Sciences: Preamble to a dialogue between Robinson Crusoe and his Man Friday." in *Theory in Archaeology: A world perspective*, ed. Peter Ucko, London: Routledge, 1995.
- Otte, Marcel. "Le rôle des américains dans la recherche du Paléolithique en Europe," in *The Role of American Archeologists in the Study of the European Upper Paleolithic*, ed. Lawrence G. Straus. BAR International Series 1048. Oxford, England: Archaeopress, 2002.
- _____. "The Significance of Variability in the European Mousterian," in *The Middle Paleolithic: Adaptation, Behavior, and Variability*, eds. H. Dibble and P. Mellars, Philadelphia: University Museum, University of Pennsylvania, 1992.
- _____. "On Paleolithic Research Traditions: A response to Clark and Lindly," *Current Anthropology* 33, no. 4 (1992): 407-408.
- Otte, Marcel and Lawrence Keeley. "The Impact of Regionalism on Palaeolithic Studies," *Current Anthropology* 31, no. 5 (1990): 577-582.
- Patterson, Thomas Carl. *Toward a Social History of Archaeology in the United States*. Case studies in archaeology series. Fort Worth: Harcourt Brace College Publishers, 1995.

- Peyrony, Denis. "Le Moustérien – ses faciès," *Comptes Rendus de l'Association Française pour l'Avancement des Sciences*, 44th Session: Strasbourg (1920): 496-497.
- _____. "Le Moustier: ses gisements, ses industries, ses couches géologiques," *Revue Anthropologique* 40 (1930): 48-76.
- _____. "Les Industries 'aurignaciennes' dans le bassin de la Vézère," *Bulletin de la Société Préhistorique Française* 30, no. 10 (1933): 543 – 559.
- _____. "Une mise au point au sujet de l'Aurignacien et du Périgordien," *Bulletin de la Société Préhistorique Française* 43, Numéro 7 (1946): 232-237.
- Pigeot, Nicole. "Technical and Social Actors. Flint knapping Specialists and Apprentices at Magdalenian Etiolles," *Archaeological Review from Cambridge* 9 (1990): 126-141.
- Piggott, Stuart. *The Progress of Early Man*. London: A. & C. Black Ltd, 1935.
- _____. *William Stukeley, An Eighteenth-Century Antiquary*. Oxford: Clarendon Press, 1950.
- _____. *Scotland Before History, An Essay*. London, New York: T. Nelson, 1958.
- Prestwich, Joseph. "On the Section at Moulin Quignon, Abbeville, and on the peculiar Character of some of the Flint Implements recently discovered there," *Quarterly Journal of the Geological Society* 19 (1863): 497-505.
- Renfrew, Colin. "An Interview with Lewis Binford," *Current Anthropology* 28, no. 5 (Dec., 1987): 683-694.
- Rolland, Nicolas. "New Aspects of Middle Paleolithic Variability in Western Europe," *Nature* 255 (1977): 251-252.
- _____. "The Interpretation of Middle Palaeolithic Variability," *Man* 16, no. 1 (1981): 15-42.
- Rolland, Nicolas and Harold Dibble, "A New Synthesis of Middle Paleolithic Assemblage Variability," *American Antiquity* 55 (1990):480-99.
- Rouse, Irving. *Migrations in Prehistory: Inferring Population Movement from Cultural Remains*. New Haven: Yale University Press, 1986.
- Sabloff, Paula. *Conversations with Lew Binford: Drafting the New Archaeology*. Norman, OK: University of Oklahoma Press, 1998.
- Sackett, James. "Quantitative Analysis of Upper Paleolithic Stone Tools," *American Anthropologist* 68, no. 2 (1966): 356-394.
- _____. "Style, function and artifact variability in palaeolithic assemblages." in *The Explanation of Culture Change*, ed. Colin Renfrew. London: Duckworth, 1973.

- _____. "From de Mortillet to Bordes: a century of French Paleolithic research," in *Towards a History of Archaeology*, ed. G. Daniel, London: Thames and Hudson, 1981.
- _____. "Straight Archaeology, French Style: The phylogenetic paradigm in historic perspective." in *Perspectives on the Past*, ed. G.A. Clark, Philadelphia, PA: University of Pennsylvania Press, 1991.
- Salmon, Merrilee. *Philosophy and Archaeology*. New York: Academic Press, 1982.
- Scarre, Chris. "Archaeological Theory in France and Britain," *Antiquity* 73 (1999): 155-157.
- Schlanger, Nathan. "Techniques as human action," *Archaeological Review from Cambridge* 9 (1990): 18-26.
- Schnapp, Alain. "Archéologie et Tradition Académique en Europe aux XVIIIe et XIXe Siècles," *Annales. Économies, Sociétés, Civilisations* 37, no. 5 (1982): 760-777.
- _____. "Les Annales et L'Archéologie: une rencontre difficile," *Mélanges de l'Ecole française de Rome, Antiquité* 93, no. 1 (1981): 469-478.
- Schoolcraft, Henry R. *Historical and Statistical Information Respecting the History, Condition, and Prospects of the Indian Tribes of the United States. Collected and Prepared Under the Direction of the Bureau of Indian Affairs Per Act of Congress of March 3rd, 1847*. New York: Paladin Press, reprinted, 1969.
- Smith, Philip E. L. *Le Solutréen en France*. Bordeaux: impr. Delmas, 1966.
- Sonneville-Bordes, Denise de. *Le Paleolithique Superieur en Périgord*. Bordeaux: Delmas, 1960.
- Soudsky, B. "Higher level archaeological entities: models and reality." In *The Explanation of Culture Change*, ed. Colin Renfrew. London: Duckworth, 1973.
- _____. "Le problème des propriétés dans les ensembles archéologiques," in *Archeologie et calculateurs: problèmes mathématiques et sémiologiques*, ed. J.-C. Gardin. Paris: CNRS, 1970.
- Spencer, Herbert. *The Principles of Biology*. New York: D. Appleton and Company, 1866.
- Squier, Ephraim G., and Edwin H. Davis. *Ancient Monuments of the Mississippi Valley*. New York: Bartlett & Welford, 1848.
- Steward, Julian H. and Frank M. Setzler, "Function and Configuration in Archaeology," *American Antiquity* 4, no. 1 (1938): 4-10.
- Stiner, Mary. *Honor Among Thieves: A zooarchaeological Study of Neandertal Ecology*. Princeton, NJ: Princeton University Press, 1994.
- Stiner, Mary and Steven Kuhn. "Subsistence, Technology, and Adaptive Variation in Middle Paleolithic, Italy," *American Anthropologist* 94 (1992): 12-46.

- Stocking, George W. *Anthropology at Chicago: Tradition, Discipline, Department*. Chicago: Joseph Regenstein Library, University of Chicago, 1980.
- Straus, Lawrence G. "Paradigm lost: a personal view of the current state of Upper Paleolithic Research," *Helinium* 27 (1987): 157-171.
- _____. "Paradigm found: a research agenda for study of the Upper Paleolithic and post-Paleolithic in SW Europe." in *Perspectives on the Past*, ed. G.A. Clark, Philadelphia, PA: University of Pennsylvania Press, 1991.
- _____. "L'Abbé Henri Breuil: Archaeologist", *Bulletin of the History of Archaeology* 2, no. 2 (1992): 5–9.
- _____. "American Perspectives on the European Upper Paleolithic? An introduction." in *The Role of American Archeologists in the Study of the European Upper Paleolithic*, ed. Lawrence G. Straus. BAR International Series 1048. Oxford, England: Archaeopress, 2002.
- _____. "Yanks (and Canucks) in the Abbé Breuil's Court: notes on the role of American archeologists in the study of the European Upper Paleolithic." in *The Role of American Archeologists in the Study of the European Upper Paleolithic*, ed. Lawrence G. Straus. BAR International Series 1048. Oxford, England: Archaeopress, 2002.
- Strong, William D. "Anthropological Theory and Archaeological Fact," in *Essays in Anthropology: Presented to A.L. Kroeber in Celebration of His Sixtieth Birthday, June 11, 1936*. ed. R. Lowie, Berkeley, Calif: University of California Press, 1936.
- Taylor, Walt W. *A Study of Archeology*. Menasha, WI, American Anthropological Association Memoir 69, 1948. (cited from the 1967 reprint)
- Tixier, Jacques., Inizan, Marie-Louise and Roche, Hélène. *Préhistoire de la Pierre Taillée: Vol. 1: Terminologie et Technologie*, Valbonne: Cercle de Recherches et d'Etudes Préhistoriques, 1980.
- Trigger, Bruce. *A History of Archaeological Thought*, 2nd ed. New York: Cambridge University Press, 2006.
- _____. "Archaeological Theory: The big picture," *The Grace Elizabeth Shallit Lecture at Brigham Young University*, Salt Lake City, UT: The University of Utah Press, 2003.
- Tuffreau, Alain. "Middle Palaeolithic Settlement in Northern France," in *The Middle Paleolithic: Adaptations, Variability, and Behavior*, eds. H. Dibble and P. Mellars, Philadelphia: University Museum Press. 1992.
- Turq, Alain. "Le Moustérien de type Quina du Roc de Marsal (Dordogne)," *Bulléin de la Société Préhistorique Française* 82 (1985): 47-51.
- _____. "Approche technologique et économique du facies moustérien de type Quina." *Bulléin de la Société Préhistorique Française* 86 (1989): 244-56.

- Tylor, E. B. *Primitive Culture: Researches into the Development of Mythology, Philosophy, Religion, Language, Art and Custom*. London: Murray, 1871.
- Van Peer, Philip. "Interassemblage Variability and Levallois Styles: The case of the northern African Middle Paleolithic," *Journal of Anthropological Archaeology* 10 (1991): 107-151.
- Vincent, A. "L'os Comme Artifact au Paléolithique Moyen: Principes d'étude et premiers Résultats," in *L'homme de Néandertal: centenaire de la découverte de l'homme de Spy: colloque international, 4-7 décembre 1986, Domaine Provincial de Wegimont*, ed. M. Otte, Liège, Belgium: Université de Liège, 1986.
- Watson, Patty Jo, Steven LeBlanc, and Charles Redman. *Explanation in Archeology; An Explicitly Scientific Approach*. New York: Columbia University Press, 1971.
- Wiley, Gordon and Jeremy Sabloff. *A History of American Archaeology*, 3rd ed. San Francisco, CA: Freeman, 1993.
- Wiley, Gordon, and Philip Phillips, *Method and Theory in Archaeology*. Chicago: University of Chicago Press, 1958.
- Wilmsen, Edwin. "Functional Analysis of Flaked Stone Artifacts," *American Antiquity* 33, no. 2 (1968): 156-161.
- Wilson, Daniel. *Prehistoric Man; Researches into the Origin of Civilization in the Old and the New World*. Cambridge: Macmillan & Co, 1862.
- _____. *Prehistoric Man; Researches into the Origin of Civilisation in the Old and the New World*. 2nd ed., London: Macmillan and Co, 1865.
- Woodbury, Richard B. "Nelson's Stratigraphy," *American Antiquity* 26, no. 1 (1960): 98-99.
- Wylie, Alison. *Thinking from Things: Essays in the Philosophy of Archaeology*. Berkeley, CA: University of California Press, 2002.

BIOGRAPHICAL INFORMATION

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