



ANTHROPOLOGY

WITHOUT INFORMANTS

Collected Works in Paleoanthropology
by L. G. Freeman

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In memoriam

F. Clark Howell (1927–2007), who knew everything about the past and its most important investigators almost without exception. He tried his best to teach me to be a professional prehistorian, and I owe whatever I have accomplished to him, though I never came close to being a professional of his caliber.

Sit tibi terra levis.



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Foreword

This volume encapsulates some of the most significant published work of Leslie G. Freeman, an important—and, I believe, underappreciated—figure in the history of American participation in the study of Paleolithic Europe.

Leslie Freeman entered this field in the 1960s, a time of intellectual turmoil and important developments in the history of archeology. First came the rise of the movement in American anthropological archeology that came to be known as the “New Archeology.” Led by the charismatic Lewis Binford, a network of relatively junior archeologists challenged prevailing orthodoxy in advancing new claims. They argued that archeology properly was—or should be—a science, and one that promised reliable knowledge of the prehistoric past through careful application of scientific method. Furthermore, since the various aspects of culture were part of an inter-related, systemic whole, information was potentially retrievable about all aspects of past sociocultural systems, including social organization and ideology, that had been conventionally regarded as more or less inaccessible to investigation. Suddenly, the scope of archeological investigation was seen as greatly broadened.

The second notable development was that of the concept—especially associated with Freeman’s mentor, F. Clark Howell—of paleoanthropology. Howell conceptualized the study of human evolution not as an exotic subfield of paleontology

but as the multifaceted *anthropological* study of human biological and cultural evolution. All subfields of anthropology had contributions to make to this endeavor (although that of linguistics was admittedly limited because of the paucity of direct evidence of ancient languages before the emergence of writing). In Howell's view, archeology and even sociocultural anthropology had vital contributions to make to understanding the behavior of the ancient hominins who left behind Paleolithic archeological sites.

Finally, Freeman's intellectual formation coincided with the first large-scale involvement of American archeologists with Paleolithic prehistory, especially in Europe. American archeologists had always worked largely in the New World, occupying themselves with the relatively narrow slice of the human past represented by occupation of the New World (the last 10,000–20,000 years or so). In the wake of a handful of pioneers like Hallam Movius (whose Old World fieldwork experience long antedated World War II), a new generation of archeologists chose to work with the deep archeological record of the Old World Paleolithic. Archeological deposits in Europe date back tens and hundreds of thousands of years, and the older parts of that record were left by hominins that were notably different skeletally from anatomically modern humans. For these early humans, one could not necessarily assume cultural capabilities and adaptations comparable to those of recent hunter-gatherers. This was an issue not faced by New World researchers. Enabled by postwar prosperity and a great expansion of U.S. higher education and research funding, this group began to put an American stamp on Paleolithic research. James Sackett, Harvey Bricker, Sally Binford, Alison Brooks, Leslie Freeman, and Richard Klein, among others, began to come to grips with the complexity and depth of the Paleolithic archeological record, as well as its interpretations by their European colleagues, who, as Freeman details in this volume, came from quite a different intellectual tradition from the American one. The most dramatic consequent confrontation of this period was between François Bordes and Lewis Binford over the interpretation of stone tool variability in the Mousterian industry (generally associated with the Neandertals). However, for the most part, the Euro-American encounter was quieter, thoughtful, and sustained, and resulted in many long-term and mutually beneficial research collaborations.

Leslie Freeman was a busy participant in these intellectual developments. His mentor, Clark Howell, who persuaded him to eschew socio-cultural anthropology for paleoanthropology, introduced him to Paleolithic fieldwork at Torralba and Ambrona in Spain. Freeman's period as a graduate student also coincided with Lewis Binford's tempestuous tenure at the faculty of the University of Chicago. Binford's sense of the exciting possibilities of a rigorously scientific archeology had a clear influence on Freeman. Freeman's choice of a doctoral dissertation topic—Mousterian lithic variability in Cantabrian Spain—resonated with Binford's enthusiasm for applying new analytical tools and scientific method to problems in traditional prehistory.

After Freeman's initial research experience with Howell on the Spanish Meseta, he moved to the archeologically rich region of Cantabria in north-central Spain for his dissertation on Mousterian lithic variability. This area has since remained the geo-

graphic focus of his research, although he worked in Catalunya at Abric Agut in the 1970s and returned to Ambrona with Howell in the 1980s. In the course of his career, Freeman has had sustained research collaborations with several colleagues (notably Howell, Richard Klein, and Karl Butzer), but none was as durable as his decades-long collaboration with the eminent Spanish prehistorian Joaquín González Echegaray, with whom he worked on two long-term cave excavation projects at Cueva Morín (with Mousterian and Upper Paleolithic deposits) and el Juyo (Upper Paleolithic) and numerous publications.

To a greater degree than many U.S.-based researchers, Freeman became a regularly contributing member of the Spanish Paleolithic research community. He and his wife, the distinguished socio-cultural anthropologist Susan Tax Freeman, have long maintained a home in Santander, where they have spent extended periods. Unlike most of his counterparts, Freeman was not an annual participant at the meetings of the Society for American Archaeology and the American Anthropological Association, but he frequently lectured and presented papers at meetings in Spain and other countries. His network of Spanish colleagues and collaborators is extensive. Although he has published in the most highly regarded U.S. journals (including *American Anthropologist* and *American Antiquity*), about one-third of his research publications are in Spanish outlets. This laudable involvement in the Spanish research community, I believe, had the effect of diminishing somewhat his visibility in Anglophone research circles. Perhaps most significant in this respect is that the monographic publications of his two long-term cave excavation projects (Cueva Morín and el Juyo) have been in Spanish, limiting access among English-language scholars. Furthermore, as of this writing, Howell's and Freeman's work at Torralba and Ambrona has not yet seen final monographic publication—a fact that has doubtless contributed to the controversy and misconceptions over interpretation of the sites about which Freeman writes in this volume.

Leslie Freeman's institutional base during nearly his entire career was the Department of Anthropology at the University of Chicago, where after receiving his Ph.D. in 1964, he returned in 1965 as a faculty member, during Clark Howell's effort to build a nucleus of paleoanthropological researchers. After Howell's departure for Berkeley in 1970, Freeman, along with Karl Butzer and Richard Klein, formed the "stones and bones" contingent with Paleolithic interests among the anthropology faculty. Freeman and his colleagues trained a number of students who went on to careers in Paleolithic research, including Geoffrey Clark, Margaret Conkey, Lawrence Straus, Thomas Volman, James Pokines, Heather Stettler, and myself. Butzer and Klein left Chicago in the 1980s, and unfortunately, were not replaced by faculty with Paleolithic interests. The Department of Anthropology had decided to reorient its archeological research interests toward early complex societies. When Freeman retired in 2000, the distinguished history of Paleolithic research at Chicago came to an end. As several chapters in this volume show, Freeman has remained an active scholar since his retirement. In addition to emeritus status in Chicago's Department of Anthropology, he has institutional affiliations with Montana State University and with the Instituto para Investigaciones Prehistóricas in Santander, which he cofounded.

The pieces collected in this volume represent a sampling of Freeman's thought and writing over more than forty years and touch on many subjects. They reveal several recurring and important issues that have occupied him over the years. One issue is that of human agency in the accumulation of excavated deposits from the deep past of the Lower Paleolithic, especially at sites like Torralba and Ambrona. In such cases, we cannot be sure that our ancient subjects behaved in ways that correspond to the behavior of any ethnographically known human groups. What role then can ethnographic analogy play? Interpretations of the hominin behavior that produced the arrangements of mammal bones and stone tools at Torralba and Ambrona have varied greatly—from depictions of human predators able to conduct well-planned elephant hunts to those of human scavengers quite incapable of hunting mammals of any size. Freeman has always been concerned with careful interpretation of patterning in all relevant prehistoric data that can be demonstrated to exist through replicable, appropriate statistical methods. He has not shied away from controversy, as his discussions of Lewis Binford's interpretations in Chapters 6 and 7 show. But his emphasis has always been not on personalities, but on the best ways of tackling the inherently thorny problems of interpreting the Lower Paleolithic record.

The study of faunal remains for information about ancient subsistence and diet is another recurring theme in Freeman's research and is treated in Chapter 3. Freeman's concern is with reliably separating what we can and do know about these complex ancient systems from what we do not, and perhaps cannot, know. As he notes, these sources of "noise" in the archeological record are not always recognized and accounted for in the archeological literature.

Two further issues closely linked in Freeman's writings are the interpretation of Mousterian lithic variability (see Chapters 8–10) and the appropriate use of statistical methods, in archeology generally and in lithic analysis particularly. Freeman's doctoral research involved him closely with the stone implements of the Cantabrian region and showed him that Bordes's scheme of four Mousterian "facies" defined in southwestern France did not fit Cantabria well. Eventually, he was able to demonstrate that the kind of lithic variation Bordes measured was in fact not parsed into four discontinuous facies but varied continuously among assemblages. As he notes in his preface to this volume, Freeman learned while in the utilities industry, and again in the army, the importance of carefully measuring variables relevant to the problem at hand and manipulating the data with quantitative methods carefully selected for appropriateness given the nature of the data. He never forgot this lesson in his analyses of archeological data.

Finally, Paleolithic art, especially cave art, has been an important research concern of Freeman's since the 1980s. Initially reluctant to enter a field so characterized by highly speculative theories about ancient religion and systems of thought, Freeman came to find it amenable to careful, systematic investigation. As Chapters 11–15 indicate, painstaking observation and data collection can both disconfirm simplistic theories and reveal interesting patterning in the data that had not been recognized. His careful use of ethnographic and historical information, and of data

on the biology and behavior of the animals depicted in cave art, has opened new perspectives in this field.

This volume gives the reader a good appreciation for the range and depth of the scientific contributions of Leslie Freeman. It can only hint at the personal characteristics that have made knowing and working with Les such a rewarding experience for me and many others. The range of his intellectual curiosity is impressive—from ethnography to photogrammetry, from Romanesque art to big-game hunters' accounts of animal behavior. Les is always finding material in unlikely corners that can help illuminate the study of the Paleolithic. His enthusiasm is almost boyish for new statistical or field methods and new gadgets that might improve how archeology is done. And his sometimes outrageous sense of humor, heightened by a prodigious memory for limericks and song lyrics, has brightened many an afternoon of excavation. Leslie Freeman's contributions to the study of the Paleolithic have been considerable, and this volume is an excellent introduction to them.

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Preface

The chapters included in this book are a cross-section of the shorter and more general works I have written during more than forty years as a professional prehistorian, or behavioral paleoanthropologist (as I prefer to consider myself). I was trained as a socio-cultural anthropologist and got my first excavating experience in the New World. Later, my research has focused on the Old World, but the problems that have interested me most should be relevant to the concerns of all archeologists of whatever persuasion. I have selected papers that illustrate those concerns. They are all still relevant today, even though some of the papers selected appeared in print many years ago. Since the chapters I have chosen have been published before, they are reproduced here as they first appeared with one exception. It would have been unfair to revise them to make them seem more “up-to-date” and the major points they make are still as valid as ever.

The choice of chapters for inclusion reflects the extent of my career that has been devoted to Old World prehistory. (I have not included works on investigations in the New World or papers on my work in Medieval religious symbolism here.) The bulk of my research and publication has been in the field of Paleolithic studies in Europe, particularly Cantabrian Spain. Fascinating though I find that material, much of it was published in the form of site reports, detailed analyses of recovered

remains, or extensive surveys aimed at a specialist audience. Most of those publications were co-authored in cooperation with other collaborating scientists, and additionally many appeared in foreign languages or in Spanish, French, Czech, and German journals. Consequently, even when it was published in English, my work is better known to Europeanists than to the larger number of Americanist archeologists or those based in British institutions.

So part of the reason for this book is to familiarize others with my stance. I think it is important that all of us—whether we are anthropologists who learn about living societies, archeologists who excavate or read documents from the past, or members of the intelligent reading public at large—ought to know about the various ways those of us who study the past learn about the lifeways of our ancestors and relatives. My background and perspective are different enough from those of others so that it may seem novel (and, I hope, valuable) to professionals working in other areas with other approaches. I was trained as a socio-cultural anthropologist and only decided to become a behavioral paleoanthropologist late in my career. That helps explain some of the peculiarities of my approach.

If there is one thing that an archeologist should always do, it is to question. Affirmations, whether they are one's own or others', should always be examined critically no matter how sensible they seem at first glance. Even in the more speculative chapters in this book I have tried to arrive at conclusions that correspond better to what we know about the past (and present) than do previous conjectures. Of course, it is the duty of any scientist, not just an archeologist, to question all observations before they are accepted, and to challenge all of them that are contrary to what is already soundly established. New conclusions should only be accepted after they have been carefully tested, and that holds as well for the conclusions given in this book as for any other affirmations.

I would scarcely consider conducting an analysis of archeological data without employing one or another statistical or mathematical technique for the purpose. That is partly because of the ways I spent my time after a more or less wasted period in college. Drifting aimlessly after graduation, I spent three years working for a public utilities company, where some of my time was spent in boring repetitive tasks such as drawing the standard plans of gas metering stations. I looked forward to the months I was expected to spend each year helping to prepare their five-year prediction of natural gas requirements. That was fascinating. It taught the value of mathematical and statistical analysis. We did not then have access to the giant calculators used for multivariate statistics and so had to do our load forecasts by trial-and-error methods using Marchant™ and Monroe™ desk calculators. I learned how much easier generating the estimates would have been if we could have used the methods of multiple regression and factor analysis. The experience also taught the need for careful, painstaking cross-checking of data entry and results.

While thus employed, I helped one of my superiors conduct land surveys. I was also a member of the New Jersey National Guard, with the occupational specialty of Combat Demolition Specialist; any mistake in calculating explosive requirements might have had devastating results—as I saw when a lecturer almost blew himself

up placing a “ring main.” That reinforced the lessons I had learned about care in calculation. Then, during active duty with the U.S. Army, there were more than enough demolitions specialists to satisfy the demand, and so I was assigned to be a topographic survey section chief, a specialty that also called for careful calculation. All these experiences provided the background in the mathematical analysis of data that I use today. Although it was not a deliberate plan on my part, much of this early training seems as though it had been designed to help me along to my later career as a paleoanthropologist.

I finished my preparation at graduate school, where I owe my social anthropological training to my late professors Fred Eggan and Eric Wolf. I am especially indebted to the prehistorian A. J. Jelinek and to my recently departed teacher F. Clark Howell, to whom this volume is dedicated. Jelinek’s sensitivity to paleoecology is reflected in these pages, and so, particularly, is Clark Howell’s definition of paleoanthropology as a kind of anthropology, not simply the study of the skeletal remains of prehistoric hominids. It was Clark who persuaded me to take up the career of paleoanthropologist. As a graduate student at Chicago, I continued to employ statistical analysis, much of the time in collaboration with James Brown and under the guidance of L. R. Binford. I also learned much from other prehistorians who have since passed away (François Bordes and Francisco Jordá taught me how to think about the Mousterian). I have a still greater debt to my longtime colleague, mentor, and collaborator, Joaquín González Echegaray, for having encouraged me to develop my own approach. Whatever is good in what follows I owe to them.

Now, to address the contents of the book. Many prehistorians seem to believe that if one has not made a “major contribution to theory,” regardless of whether it can be applied to any relevant data, then his or her life’s activity has been worthless. On the other hand, I have never found that any theory in the absence of applicable data is worth a plugged nickel. I have always tried to accompany each theoretical statement with the data to which it has relevance. So all the papers that follow blend theoretical statements with the archeological facts they are intended to help us understand.

Chapters in the first section of this book present some statements of my own theoretical perspective and some observations that ought to be taken into consideration in further interpretations of the data from the past. They do not fit elsewhere so I have brought them together here. The first chapter differentiates behavioral paleoanthropology from the other kinds of archeology and suggests a program to be followed in paleoanthropological research. “A Theoretical Framework for Interpreting Archeological Materials” addresses the use of analogy in the interpretation of early finds. In “The Fat of the Land” I have tried to indicate some dimensions of the promise and limits of research on prehistoric diet. (I cut out the final sections of this paper because they would appeal mainly to a very specialized audience; I also added a few remarks in an appendix to this paper.)

The next section summarizes some of the results of Paleolithic studies. In “By Their Works You Shall Know Them: Cultural Developments in the Paleolithic,” I have provided a general overview of cultural developments in the Old Stone Age as I

see them. Despite what we have learned since it was written (more than thirty years ago), it still has much of its original value. The next chapter focuses on the spatial relationships of Cantabrian sites. Spatial geographers have used Thiessen diagrams or Voronoi tesserae to study the distributions and relationships of modern cities: this chapter suggests that they may be useful for the study of Paleolithic sites as well.

The chapters on Torralba try to indicate what we know about that site and its sister, Ambrona, and to dispute the idea that early hominids could only have managed to survive in Europe as scavengers. In the Middle Paleolithic section, "Kaleidoscope or Tarnished Mirror? Thirty Years of Mousterian Investigations in Cantabria" presents the evidence that we should take a new look at the Mousterian, and the two following chapters outline several differences between the behavior of Neandertals and that of modern people, and describe some of the research errors committed by prehistoric archeologists in the past.

The first chapter about Paleolithic art is a more or less theoretical statement about where we should be looking for its meanings, and where they will not be found. In "The Many Faces of Altamira" I have tried to show how many ways present concerns are reflected in our handling of the past and discussed the relationship between the validation of religious shrines and the early debate about the painted cave of Altamira. The chapter on enhancement techniques discusses the ways in which some Paleolithic artists added impact to selected figures. The next chapters try to clarify what is meant by the term "sanctuary" when it is applied to Paleolithic caves and involve speculation about the prehistoric uses of the decorated site I know most intimately, the famous painted cave of Altamira.

Last, there is a chapter about the benefits of international research collaboration, showing that those benefits have flowed in both directions: from America to Spain, and (as importantly) from Spain to the Americas.

As I have indicated earlier, these papers are reprinted here essentially unchanged except for the bibliographies, rectification of misprints, omission of abstracts in languages other than English, and corrections to figures and legends that were incorrect in the originals. It is my hope that others, seeing what I have offered that is of worth and rejecting what they can show is wrong, will find something in these pages that stimulates them to further progress.

ANTHROPOLOGY WITHOUT INFORMANTS

P A R T

I

TOWARD A WORKING THEORY

Each of the three chapters in this section addresses a theoretical issue of considerable importance to archeologists of all persuasions. The first and second distinguish the field of behavioral paleoanthropology from other and very different kinds of archeology. When the pieces were written, archeologists in the United States pretty generally assumed that their kind of prehistoric archeology was the only one. But prehistory is defined as lasting until the peoples who are its subject have begun to produce their own written records. In much of the United States, preliterate people were observed by literate outsiders who left good written descriptions about what they had observed. In other cases, preliterate societies lasted until archeologists began to question living informants about the conditions under which they had previously lived. The anomalous nature of a prehistory with living informants, or recorded by contemporaries, should be obvious, and is the exception rather than the rule for archeologists who study the products of long-vanished societies and kinds of humanity that are often extinct. Some authorities claimed (erroneously) that groups of living hunter-gatherers had been “frozen in time” as living relics, so that all that was needed to fill in the gaps in the archeological record was to supply the missing data by analogy with some living group such as the Australian aborigines.

I go on to develop a model for understanding the past, drawn from Malinowski's concept of "institutions." I use a modification of that model of culture because it provides an inherent reason and a plausible mechanism for change, and it includes the physical materials upon which archeological reasoning must be based. I have replaced Malinowski's concept of the institutional charter with that of the "functional mode," which is one purposive aspect of institutional behavior that is more visible archeologically than are his "charters." (The charters of Malinowski's institutions cannot be directly observed by the archeologist, who only recovers traces of the activities the institution has produced.) Years ago, when I was a student, one of my professors discussed the custom of tipping one's hat to a lady. When I asked if the physical nature of the head covering was important, he said that it was not. But, I asked, what if it were a yarmulke? Malinowski would not have had the difficulty with my question that my professor did.

Malinowski was widely (and wrongly) rejected because of flaws in his reasoning about the "function" of institutions, when it would have been easy enough to revise that reasoning instead of throwing his theory out wholesale. I continue to use a re-statement of Malinowski's theory for the reasons mentioned, and especially because it consistently works when applied to real archeological remains. I'll persist in using it despite its relative antiquity and in spite of all criticism until someone shows me that there is a more practical solution.

It was fashionable when I was a young professor to define culture in a "more modern" way, as "shared ideas in people's heads." I offended some of my colleagues by observing that unless the ideas came out of the heads into some material embodiment—in the form of a social usage, or at least into language, which after all can be measured physically—it simply could not be observed at all.

These observations lead me to another important one. We are sometimes told that archeology should develop its own theoretical stance and its own research methods, and that it will never be a mature discipline until it has done so. I do not believe that for a moment, and I speak as one who has had to develop his own programs for the analysis of prehistoric data on a few occasions. In fact, modern theoretical physics has always relied on the techniques of mathematics, which should be a sufficient contrary argument. I advocate instead searching out and using any technique that works, no matter where or by whom they were invented. It is even my experience that several of the specially devised programs for archeological data analysis do not work as well as some of the more general and readily available commercial programs, such as SYSTAT™ or SPSS™; programs that are designed for exclusive archeological use should only be employed (or designed) where no alternative is available.

My second chapter discusses the prevalent idea that the archeologist can only work by making analogies between the behavior of some living or ethnographically known group. I agree that analogy can be useful when it produces hypotheses that are amenable to testing against the realities of archeological data, but the use of analogy to complete a picture of past human behavior where the humans involved are not modern, and may in fact be assumed to be much different from ourselves, is

simply wrong. Old as this chapter is, its attempt to indicate the fallacy of such reasoning remains valid despite all later claims to the contrary.

The late Christopher Hawkes claimed that it should be relatively easy to reconstruct prehistoric economic systems. “The Fat of the Land” attempts to show how difficult even the reconstruction of prehistoric diet can be when all one has to go on are archeological residues. There are many complications to the discussion of prehistoric diet from the archeological record that Hawkes was apparently unaware of, although some of them should have been obvious. This chapter is just the first part of the original paper, excised from the rest, which discussed the Spanish Paleolithic in terms that would not interest most readers. I have added some concluding observations, indicating that the interpretation of faunal remains from archeological sites is not as straightforward as Hawkes assumed.

Anthropology without Informants

ANTHROPOLOGY AND THE SEVERAL ARCHEOLOGIES

Anthropology is unique among the disciplines which study mankind in the breadth and diversity of its approaches. This multiplicity of perspectives is its major strength, lending it a flexibility and adaptability few fields can rival. Ideally, continued feedback among its subfields should ensure that each periodically may come to new insights about the nature of our species. For that ideal to be realized, communication between the subfields must be kept easy and open.

Just a few years ago, ease of communication could be guaranteed by exposing students in depth to all branches of anthropology. Then, anthropologists shared a basic vocabulary and a common set of referents. With the tremendous increase in quantity of anthropological data that has accumulated in the last twenty years, anthropological subfields have tended to multiply, specialize, and diversify, developing unique interests and multiplying esoteric jargon. As a result of this fission, some anthropological subdisciplines have begun to lose sight of one another. The increased complexity of our field makes it ever more difficult for the individual to become a competent anthropological generalist.

Although the changes that have taken place make it considerably harder for individuals to learn each other's specialties, they are by no means to be regretted,

as some seem to think. Such changes always accompany the development of any discipline; they are a sign of the increasing maturity of anthropology. If we devote more attention to the growing differences between subfields in the process of individualization and force ourselves to be more fully aware of the uniqueness of each specialty, we shall eventually see the way to a new and more realistic synthesis. Only when we appreciate what each field has to offer will we be able to draw from the strengths of each what it is best equipped to contribute to the study of man.

These remarks apply fully to the archeological subfields. Although nonspecialists still regard archeology as one kind of beast fit to carry one kind of burden, its branches have become intriguingly diverse. Their evolution has been so rapid that different kinds of archeologists have begun to misunderstand one another and sometimes to hold very narrowly circumscribed views of the nature of archeology as a whole.

This essay attempts to provide a clearer picture of one emerging anthropological subfield—paleoanthropology, a relatively recent development fusing aspects of physical anthropology and prehistoric archeology. In particular, it examines the part of paleoanthropology which studies the evolution of human behavior.

The field has always excited its share of public and professional interest, and rightly so. The immense majority of the history of humanity unfolds in the remote past and is known only from archeological remains. Paleoanthropology offers the only direct means of attaining any idea of the range of possible variation in the human condition, or of the prehistoric antecedents of its present state. To give a better idea of the nature and limits of the field, we may as well begin by explaining what paleoanthropology is not.

There are several kinds of archeology, not one. The only attribute all archeologists share is a reliance on the enduring material evidence of past human behavior. The largest distinction between archeological specialties, which will probably be familiar to most readers, sets the family of historical archeology off from the group of prehistoric archeologies. But that distinction is not the only one which must be made. Each family, in fact, encompasses a distinctive set of disciplines which are quite idiosyncratic, regardless of the general attributes they share.

Since all the historical archeologies deal with the very recent past, all may utilize documents written by contemporaries of the relics they study, whenever such documents are available. Nevertheless, the family is internally diverse. Its subfields may be very narrowly specialized by interest in a certain region (U.S. colonial archeology, Mesopotamian archeology), linguistic group (Slavic or Celtic archeology), or time period (medieval archeology) or focus on a specific aspect of economic life (nautical archeology, industrial archeology). Unlike the other subgroups, some of the specialized historical archeologies do not rely primarily on excavation as a data-gathering technique.

The various branches of historical archeology offer fascinating prospects when they can rely on eyewitness documents about their data. As a whole, they are finely focused "personal" kinds of archeology with the potential to capture remarkably specific details and to weave them into a surprisingly full and compelling fabric. If

that potential for bringing the past to life is seldom realized, it is because the written records are themselves often inadequate. The documents that survive mostly concern important personages: the few leading inventors, traders, statesmen, courtiers, soldiers, and churchmen of the day. Too often, historical archeology becomes the archeology of the historic, concerned with the pompous and monumental. Preserved documents tend to be incomplete, or biased, or simply unconcerned about the problems of greatest interest to us. But given a sufficient number of suitable texts to place a well-dated, closely spaced sequence of events in the context of their times, the historical archeologists have the greatest potential for the study of innovation, acculturation, and cultural process.

The research workers who have no contemporary written texts to draw on are usually called prehistoric archeologists. Paradoxically, however, some branches of the field have better documentation to rely on than the historical archeologists. In North America, Australia, parts of Asia, and the Pacific Islands, writing was unknown for millennia after other parts of the world had become literate. So, at the time they were first contacted by literate peoples, the inhabitants of those regions were "prehistoric" in a perfectly legitimate sense. But that contact took place only a few generations ago. A few of the peoples in question have been able to keep crucial portions of their ancestral beliefs and customs relatively intact, and these exceptionally conservative groups have now been well studied by ethnologists and social anthropologists, whose monographs are far better sources of anthropological data than historical documents or travelers' tales of any antiquity. In other cases, the prehistoric societies themselves have vanished, but living individuals learned about the traditional lifeways from their grandparents, who may even have lived in the very settlements now being excavated and analyzed by prehistoric archeologists. The paradox is obvious: this is a prehistory with the benefit of living informants.

As it happens, North American anthropologists pretty generally think of this very anomalous kind of archeology as prehistory *par excellence*, without recognizing just how unusual it is. That is to some extent understandable, since American ethnology and New World archeology grew up together, each contributing substantially to the development of the other. New World archeology eventually gave ethnology the chronological frame essential to rescue it from the tail-chasing of pseudohistorical reconstruction, but, in exchange, the theories and methods of American archeology have gained immeasurably because its conclusions have consistently had to be tested against hard ethnographic fact.

It is no accident that New World archeology has erected its sturdiest and most elegant structures in those areas where it has been able to rely on living informants or good ethnographic studies. Such sources provide it with much information about all aspects of culture, including those which leave the fewest durable material traces: the symbolic content of behavior or its material products, the social contexts in which those products were used, and the shape of the networks of social relations. Without informants or documentation, some of these aspects could not be inferred directly from archeological materials. With such evidence as a basis, reconstructions can, with caution, be pushed back in time on the order of several centuries without

losing their general validity. Since the total time depth of New World prehistory is extremely shallow, amounting to less than 1 percent of the hominid story, and since, as far as we know, all the prehistoric inhabitants of the New World are members of our own subspecies, *Homo sapiens sapiens*, there may even be justification for assuming broad behavioral continuities between any of them and living people.

In some well-studied regions of the New World, the density of excavated or decently tested sites occupied during the last millennium is impressively high: sometimes there are a score or more sites per century. Coupling the thickness of the archeological record with the density of the ethnographic detail available, late New World archeology and its analogues elsewhere in the world can provide more insight into relevant aspects of social and cultural change—long-range cultural process—and more specific evidence about the enduring corporate fabric of social relations among ordinary men than any of the historical archeologies. Nevertheless, the very factors which give this paradoxical “prehistory” its robustness for the testing of method and the development of theory often make it hard to apply its findings outside its home area.

In the Old World true prehistorians leave to others the study of the shadowy “protohistoric” zone where “prehistory” gives way to “history.” Normally they are concerned with nothing more recent than the local Neolithic. Ordinarily, those who study Paleolithic and Mesolithic remains are considered to have the only unblemished claim to the title “prehistorian.” Of course, New World archeologists who analyze Paleo-Indian or Archaic remains and those who work on the early archeology of preagricultural peoples anywhere in the world should have an equal right to the title, but the use of the single, unqualified term “prehistory” for what are really very different studies is awkward, at best. So, a few professionals have adopted the designation “paleoanthropology” specifically for the study of early man (especially fossil man) in the Old World, including the examination of skeletal remains as well as the study of behavioral residues. That usage seems to me to have much to recommend it: it designates a kind of prehistory with unusual characteristics, limits, and potentials.

■ THE QUALITY OF PALEOANTHROPOLOGICAL DATA

Paleoanthropology is a unique kind of prehistory because the things it studies are so old and odd, scarce and scattered. The paleoanthropologist’s world, as we now see it, begins four million years ago or somewhat more and lasts through the appearance of the earliest true modern human beings. There is some haziness at both boundaries, but most of what we study is at least thirty thousand years old and we almost never treat anything less than ten thousand years old. For more than 90 percent of that remote time, we are dealing with the products of fossil men whose skeletons were so different from ours that it would be foolish to assume extensive behavioral continuities between them and us. (In fact, there is some reason to think that early *Homo sapiens sapiens* was probably quite unlike us behaviorally.)

It is no accident that archeologists working with more recent material can sometimes make very penetrating guesses about the behavior of their human subjects,

based on a shrewd appreciation of human nature. There is much empirical evidence suggesting that, in some general ways, all living human beings are pretty much alike, even though the specifics of their behavior differ tremendously. Such observations are the basis for the doctrine of “the psychic unity of mankind,” which is especially fundamental to structuralist anthropology today. But man attained his modern physical structure gradually, and all evidence indicates that his present psychic unity is a recent phenomenon. Thus paleoanthropologists cannot assume that extinct populations thought like living men, or that long-vanished cultural systems are simply stochastic transformations of modern ones. Other archeologists, even some prehistorians, may fill gaps in the archeological record with guesswork or direct ethnographic analogy, with some chance of success. Paleoanthropologists cannot make use of these tools except to formulate hypotheses susceptible to evaluation, verification, or rejection on the basis of the hard evidence they find in the ground.

The oddness of paleoanthropological data is manifest in another fundamental way. Over the millennia, the present world landscapes, vegetation patterns, and animal communities to which cultural systems are adapted have gradually evolved from earlier states. Those states were so different that it requires the collaboration of a great number of specialized natural scientists to reconstruct them. Without specialist cooperation to re-create past natural settings, meaningful paleoanthropological research is impossible.

Because it must wring the maximum information from rare material archeological remains, paleoanthropology has turned increasingly to quantification to make analysis more rigorous. Most professionals were not adequately prepared for this development, and as a result there has been much trial-and-error learning, involving many mistakes. Still, despite the fumbling, we can now define problems more concisely and approach their solution with an order and precision impossible before quantification.

The scarce and scattered nature of paleoanthropological data has other important implications for research. Since immense periods of time are involved, we usually find far less perishable material than our colleagues in the other archeological specialties. More important, ages of action of normal geological processes have swept away most sites and disturbed most of those that remain. For the first three million years of the hominid story, we have only a few score undisturbed sites in all. The later Paleolithic record has fewer gaps, but it is still incomplete. As a result, we are usually faced with the task of reconstructing an extinct socio-cultural system from the materials produced by only part of its members operating in only one or a very few of the many modes the system could assume. For example, in Spain during the whole of the mid-Pleistocene we have only Acheulean hunting and butchering camps: not one contemporary “base camp” has ever been recovered. So far, we cannot generate one verifiable reconstruction of the total subsistence and settlement system of a single Paleolithic society, let alone discuss sensibly any cultural system which left less tangible evidence.

The natural forces which destroy sites do not operate uniformly over the whole land surface. For millennia, there may be sporadic sites in Africa only. Then, suddenly,

the African record gives out, while a clump of five or six later sites will be found in Asia or Europe. There are vast temporal gaps where we have not yet found any sites at all. Where we do have a record it is always skewed. Sometimes all the undisturbed sites are in river valleys; at other times all may be on seacoasts or lakeshores. Since there are so few sites in any case, these erratic geographic shifts of the archeological record through time make it impossible to follow the continuous development of any prehistoric cultural system in any of its functional modes for more than a very brief period. If prehistorians are supposed to produce a kind of history of cultures—to delineate connected sequences of events in the past—then there is a sense in which one can reasonably maintain that paleoanthropologists are not prehistorians at all, for the history of any past sociocultural system eludes them.

PALEOANTHROPOLOGY AND PROCESS

One popular school of thought has it that archeology's major potential for anthropological theory is its unique perspective on the long-term operation of "cultural process." According to this view, social anthropologists see only relatively static, instantaneous slices through the constantly changing spectrum of behavior. On the other hand, the much greater time depth afforded by the archeological record shows the striking results of long-continued action of forces of cultural change and thus permits a special facility for understanding those forces.

One kind of "cultural process" is certainly accessible to the prehistorian. Process is sometimes defined as the set of dynamic relationships which characterize the operation of one of the system's functional modes, or which integrate those modes, without causing noticeable permanent change in the structure or functioning of the system as a whole. For example, the sequence of events and behavior characteristic of a religious ceremony, the context and meaning of that particular ceremony and the purpose it is meant to achieve, the organization of the participants and the effect of the ceremony on their status, all are processual in this sense. I grant that paleoanthropologists may study aspects of process so defined. However, the cultural anthropologist who observes the dynamics of the living system can do a better job. I am less confident of the paleoanthropologist's ability to study process defined as those dynamic operations which bring about a permanent alteration of one or more parts of the system and, consequently, change the functioning of the system as a whole, despite the vast time depth accessible to us. After all, if we do not produce a kind of history, how can we study cultural change?

Perhaps nothing seems more logical than that great differences between prehistoric assemblages of distinct ages are "caused by" age difference—that they result from cultural change over the interim. But even the greatest differences need not indicate this kind of change. Difference between archeological assemblages can also be due to sampling error, the influence of raw materials, variations in performance by individuals, stylistic boundaries between societies or their segments, or the suitability of distinct toolkits for the performance of specific tasks. Unless we can evaluate the contribution of each of these factors, something which has not to my knowl-

edge been done in the past, our conclusions about “cultural change” are bound to be unwarranted and misleading. The revisions made in the supposedly well-established sequences of European Paleolithic industrial evolution during the past twenty-five years clearly illustrate the insecurity of our reconstructions of “cultural change.” In fact, it is the paleoanthropologist, not the ethnographer, who observes frozen, instantaneous slices of behavior. Our great time depth will not restore fossilized data to life so that we may watch the system change. There is no guarantee that the few available, widely spaced windows on the remote past illuminate episodes from the same unfolding drama. Regardless of assertions to the contrary, our contribution to the study of cultural process consists mostly of a series of untestable speculations and unanswered (and perhaps unanswerable) questions.

To those who believe that paleoanthropologists must write history, because that is *all* they can hope to do, this view will seem pessimistic. I think that judgment is wrong. No doubt, some branches of anthropology do attempt historical reconstruction above all, but that is not the overriding aim of most of the field. Many social and cultural anthropologists, physical anthropologists, and linguists are not mostly or even peripherally concerned with historical reconstruction. I think archeologists sometimes let the looming presence of time blind them to more important aspects of their data. Certainly some archeologists (especially those who deal with abundantly documented recent products of fully modern man) can make and have made important additions to our knowledge of culture history, but not all archeologists should necessarily try to. Paleoanthropology is one of the fields whose primary potential lies in other directions.

REASONING FROM GARBAGE TO CULTURE

Having presented these negative observations, I must now indicate where the productive dimensions of paleoanthropological research may, in fact, be found. For this exposition, certain general assumptions about the relationship between functioning socio-cultural systems and the archeological record must be stipulated. First, cultures are systemic: their elements are inextricably interrelated, so that change in any element must bring about a concomitant change in at least some of the others. (There is abundant proof of this assertion in the ethnographic literature on technological change and its effects on other aspects of culture.) Second, socio-cultural systems are adaptive. It is not necessary to stipulate that all elements have a direct and immediate relationship to the survival of the society, just that some elements do function to adapt the personnel to each other, to the natural setting, and to other human groups nearby.

Next, culture is manifest in shared and observable behavior patterns. Since we are forced to deal with material residues of behavior, the currently popular definition of culture as models in people’s heads is inappropriate. In fact, it is naive. Even the cultural anthropologists who subscribe to this view cannot observe ideas in their informants’ heads until they come out of those heads and into concrete words and behavior. For paleoanthropologists, ideas which are never manifest in behavior are

irrelevant. Most ideas are, in fact, frequently expressed in some aspect of behavior, and most have multiple behavioral manifestations. Last, by studying patterned occurrences of material residues in relatively undisturbed sites we must assume that paleoanthropologists can identify significant aspects of the behavior which produced those residues. There are certainly limits beyond which their reconstructions cannot be pushed. While we do not yet know exactly where these limits lie, we do know that these limits permit them far more interpretive scope than we suspected ten years ago.

As we are all aware, human beings live today in organized groups (societies), and each modern society has a distinctive set of shared behavior patterns, beliefs, and values which it communicates to new members by the socialization process. These shared behavior patterns and attitudes enable group members to deal effectively with their natural and social environments: they provide sets of routine and predictable responses to recurrent situations, even for situations which recur only rarely and seldom to the same individuals. Living societies have relatively large and complex behavioral inventories. Some of these are more appropriate to some members than others (that is, sex roles and roles that require especial strength, wisdom, or maturity), and all societies simplify the learning task by apportioning different sets of specialized behavior patterns (roles) to those defined as especially suited to those patterns. This provides for adequate performance of essential tasks with a minimum of duplicated effort and without requiring every individual to learn the whole cultural repertoire.

The inventory of learned beliefs and behavior may be broken down into convenient analytical units in more than one way. When one is interested in the patterns assigned to the several positions in a society that an individual may occupy, roles are the most appropriate behavioral sets. If on the other hand, one focuses on the purposes of the behavior, individual performers and their positions are less pertinent than the patterns themselves, and the behavioral categories of greatest relevance are sets of responses culturally defined as appropriate to identifiable and recurrent situations. These sets of responses may be called the "functional modes" of a social group. Curing, dancing, mourning, hunting, toolmaking, fighting, trading, feasting, burying, butchering, housekeeping, and gossiping are examples of functional modes of behavior. The concept of the functional mode is deliberately flexible; no attempt is made to stipulate its minimal or maximal scope. Gossip as a functional mode is a subset of the more inclusive functional mode of "social control." Any attempt to refine the concept further runs counter to the fact that neither living human behavior nor patterned archeological residues are ever packaged in minimal, nonoverlapping sets.

In any society some functional modes are manifest in the behavioral usages of lone individuals; others require cooperation by several persons; and some may involve participation by all members of society. The personnel who participate in some functional modes (such as hunting) may form loosely constituted, temporary groups which dissolve as the purpose of action is accomplished or as they fail. Other functional modes require participation by more rigidly structured, long-enduring

corporate bodies (such as lineages). Several functional modes may simultaneously be manifest in the behavior of a single individual or group.

Each functional mode has a cultural apparatus, consisting in the total range of permissible behavioral alternatives open to the performers, the attitudes and values which guide performance, and (only sometimes) a set of physical equipment used by the performers, which we may call the *matériel*. A single type of artifact may be part of the *matériel* of several functional modes. The behavior actually produced by the performers from the larger culturally defined inventory of appropriate alternatives may be called the set of activities generated (on that occasion) by the social unit operating in the specific functional mode. Even in cases where the functional mode of behavior requires no durable *matériel*, its activities often alter the natural surroundings in lasting and recognizable ways.

The paleoanthropologist, excavating undisturbed occupation layers, recovers durable artifacts in association with particular contextual material, such as fungal spores, chemical traces, isotopes, phytoliths, animal and plant remains, sediments, and information about the location and the relative position and abundance of each category of recovered evidence. A quantitative search for significant, patterned relationships between artifactual and contextual data can optimally define related constellations of *matériel* that vary together, independent of other sets. These represent the *matériel* and by-products of activities associated with distinct functional modes of behavior: some are toolkits and products of extractive processes or technological activities; others mostly reflect organizational or ideological elements.

Because of idiosyncrasies in individual behavior, the artifacts and by-products produced by different performers may be expected to exhibit recognizable differences, and the *matériel* used by one team may vary stylistically from that used by others engaged in the same activities. A careful analysis of the durable residues of behavior may therefore give information about the composition of teams and about overlap in team membership. When sufficient overlap in characteristics can be discerned in the residues of activities specific to several different functional modes, we may be able to demonstrate the presence of enduring, multipurpose social units. Once we have recognized specific and recurrent functional modes we can proceed to make reliable comparisons between the *matériel* appropriate to a particular functional mode through time. Where a sufficient number of contemporary occupations exists in a small region, stylistic similarities in the *matériel* of distinct functional modes may permit the recognition that all those modes are aspects of a single cultural system, and the spatial and temporal extent of the system may be delineated.

I have no desire to give the reader the impression that this sort of analysis is easy in practice, but neither is it an unattainable dream. A few prehistoric occupations have begun to be studied in this way, and with improvements in technique suggested by our struggles with these cases such analyses will become increasingly feasible and their results more reliable in the future. By the diligent application of such techniques we may hope to squeeze the maximum information about past lifeways out of archeological materials.

SHIFTS IN PERSPECTIVE

Due to its new interests, paleoanthropology needs to supersede some analytic practices that are customary among other kinds of prehistorians. In the last few decades, Old World prehistory abandoned an earlier concern with the geographic and temporal spread of a few supposedly diagnostic “guide fossils”; it has turned to the comparison of whole artifact assemblages to delineate chronological and “cultural” relationships. To recognize basic similarities between tools used at different times and places, certain peculiarities of the tools are ignored so that assemblages from all over the Paleolithic world may be discussed in the same terms. The key to maximizing the points of comparison between assemblages has been the development of a generally applicable scheme for assemblage classification consisting of a clearly defined set of nonoverlapping formal categories into which any Paleolithic artifacts may be sorted and a set of rules for the objective and systematic comparison of the relative abundance of each tool type in different assemblages. Prehistorians interested in describing past lifeways commonly speak of the whole occupation level or the whole site as the smallest spatial unit of practical relevance for analysis. Productive as these developments have been, they must themselves now yield to more refined approaches.

Paleoanthropologists, too, are concerned with artifacts, and, to communicate with other prehistorians, they will undoubtedly have to continue to use the current classificatory schemes up to a point. However, they are more interested in determining just what types of artifacts were significant in the cultural systems of the prehistoric occupants of a single horizon and in defining the characteristic attributes of functionally equivalent artifacts made by different individuals, groups, and societies. Typologies which were designed to be universally applicable and to maximize the recognition of similarities between assemblages must necessarily be insensitive to the sorts of distinctions paleoanthropologists wish to make. As a result, for paleoanthropologists’ own particular purposes they must first develop a separate classification for each occupation based solely on artifacts from that level. As it becomes pertinent to compare different occupations, the statistical descriptions of the individual assemblages are pooled, building out from the specific case to greater generalizations. This is the inverse of the practice most Old World prehistorians accept: they begin with a set of preestablished general categories and add specific detail to describe the peculiarities of real tools which do not conform exactly to the “ideal” types. (The results of the two processes are distinct and should prove complementary.)

The minimal spatial unit of interest to paleoanthropologists must logically become the smallest space in which distinct functional modes were manifest: activity-specific areas within a single occupation level rather than the undivided level as a whole. So far, new techniques for artifact classification and the analysis of spatial distributions are still in the developmental stages, but there have been encouraging preliminary results.

■ THE PRESENT AND FUTURE OF THE STUDY OF THE PAST

Studies of the behavior of early humans have already produced data which other anthropologists find relevant and interesting, but paleoanthropology is such a young field that most of present knowledge is based on the findings of more traditional prehistorians. While specific details are always being added so that the picture of past adaptations changes, some general conclusions seem firmly established.

It is often said that tools made our species, and while that is broadly true, tools did not make us what we are today all at once. The ability to manufacture rudimentary stone tools does not indicate that the toolmakers had attained a fully efficient cultural means of adaptation. The first stone tools are not much more consistently patterned than the termiting sticks and sponges used by living chimps, but they are more durable and thus they strike our attention in the archeological record.

The “cultural” gulf between the first toolmaking hominids and some living apes was apparently not great. Had stone tools immediately conveyed an overwhelming competitive advantage on their makers, the first stone-chippers should have radiated with extreme rapidity over much of the temperate and tropical world, and they apparently did not. Had tools been the most crucial means of adaptation, one would also expect that the record would show a rapid increase in consistent patterning of stone artifacts, and an immediate selective advantage for control, perfection, and diversification of the artifact forms produced. That did not happen either. If stone tools were so efficient, the first species of hominid to make them should have displaced the rest virtually overnight. Yet for a million years after the first stone tools were chipped, several different kinds of hominids survived in Africa—and no one of them got the upper adaptive hand. Taken all together, this evidence suggests that the advantage stone tools conveyed was not what one would expect if they signaled the appearance of fully effective cultural systems as we know them today. Several hominid groups may have experimented with stone toolmaking, and only eventually did other factors, probably involving increased efficiency of communication and more effective social organization, begin the kind of feedback between tools, the brain, society, and culture that started one species down the long track toward the modern human condition.

For a long time, the processes of socialization and communication must have been much different from their present counterparts. For millions of years, the variability tolerated in the manufacture of any particular kind of tool to a pattern was very great, and there was little evident stylistic difference in the products of distinct societies. Mostly the study of the earliest tools shows the latitude permitted in performance.

Lithic artifacts give little indication by themselves of the kinds of complex, controlled behavior that would require articulate speech. That is probably so because flaked stone is inherently limited as an indicator of behavioral complexity. When total systems of artifact and context are examined, however, the earliest European Acheulean sites provide evidence of intricate kinds of organization, planning, and

programming of activities which seem highly unlikely without well-developed systems of articulate speech.

The behavioral complexity and functional specialization manifest in modern cultural systems—the number of recognizably different functional modes—have increased through time and continue to increase at present. Many still maintain that the behavioral gulf between nonhuman primates and modern industrial humanity was bridged by a series of quantum jumps; the invention of fire, the “blade-and-burin revolution,” and the agricultural revolution are examples. As we learn more about the past, these revolutions seem more likely to have been long, gradual sequences of almost imperceptible adaptive readjustments rather than cataclysmic changes.

It was formerly suggested that revolutionary advances accompanied the appearance of new forms of hominids and that the advent of the *Homo erectus* grade or the spread of *Homo sapiens sapiens* was correlated with marked progress in behavior. Now it seems that was not the case. Mid-Pleistocene *Homo erectus* is found associated with both chopper-chopping tool complexes and Acheulean industries. The authors of Mousterian assemblages were sometimes Neandertals, sometimes anatomically modern people. The significant behavioral innovations we can define do not coincide with the appearance of new hominid forms, and, as a corollary, we may affirm that there was no necessary connection between body form and cultural type or behavioral sophistication in the remote past, any more than there is a necessary connection between race and culture today. Interestingly, there is no convincing evidence that Pleistocene hominids of either the same or different species were ever particularly hostile toward their neighbors. The comparative lack of evidence for interpersonal violence contrasts rather markedly with some later situations and contradicts popular misconceptions about man's inborn aggressiveness.

In this brief outline, I have presented conclusions about past behavior of direct relevance to social anthropologists, physical anthropologists, and linguists. Many other similarly interesting observations could have been discussed. For example, future investigations of the constitution and functions of temporary, goal-oriented social groups will be pertinent to social anthropologists studying the characteristics of hunting parties, trapping teams, boating crews, and similar groups based on flexible bonds of partnership. Certainly our intensive analyses of the specifics of cultural adaptations to a variety of natural settings will be relevant to all other anthropologists.

Paleoanthropology's goal, which it is showing it can attain, is the reconstruction of vanished lifeways from durable archeological residues. The universe of behavior of fossil hominids has many aspects which are unrepresented among living societies. Paleoanthropologists can study variations in behavioral complexes that today are invariant. That is their major strength. Paleoanthropology need not justify its research by claiming to contribute to the definition of universal laws governing cultural behavior. Whether we eventually learn that such universal laws do or do not exist, the description of the vast spectrum of cultural variation is a worthwhile end in and of itself. As Clifford Geertz so aptly put it: “If we want to discover what man amounts to, we can only find it in what men are: and what men are, above all other things,

is various. It is in understanding that variousness—its range, its nature, its basis and its implications—that we shall come to reconstruct a concept of human nature that, more than a statistical shadow and less than a primitivist dream, has both substance and truth.”

It is in contributing to that understanding that paleoanthropology achieves full partnership with the other sciences of mankind.

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A Theoretical Framework for Interpreting Archeological Materials

This essay discusses the proposition that the most serious failings in present models for interpreting archeological evidence are directly related to the fact that they incorporate numerous analogies with modern groups. This has prevented the development of frameworks of theory which might lead to an understanding of the sociocultural significance of archeological residues based directly on the comparison of those residues. The use of analogy has demanded that prehistorians adopt the frames of reference of anthropologists who study modern populations and attempt to force their data into those frames, a process which will eventually cause serious errors in prehistoric analysis, if it has not done so already. It is unnecessary, because it is possible to develop models for the interpretation of archeological evidence which minimize analogy. It is unscientific, because if we utilize models which are only sensitive to the elucidation of parallels with modern groups, the discovery of parameters of sociocultural structure unique to prehistoric time periods is impossible. Unless we can discover those parameters where they exist, evidence from prehistory will contribute very little to the understanding of ranges of variation in cultural systems, the nature of the interrelationships between elements of culture, or processes of cultural development.

RECENT DEVELOPMENTS

In the last decade, prehistoric research has attained a new level of sophistication in the gathering and interpretation of archeological materials. The revolution that has taken place is a twofold one, involving the development of new methodological approaches to the gathering and simple description of data (most of which owe a great deal to other disciplines such as physics, statistics, paleontology, paleobotany, geomorphology, geography, climatology, and pedology) and the construction of new theoretical approaches to the interpretation of those data.

While all prehistorians agree that the materials with which they deal represent only a small proportion of the materials used in and altered by human behavior, many would now reject the view Hawkes expressed only thirteen years ago, that without the aid of written records little information except that dealing with past economies can be extracted from archeological evidence (Hawkes 1954). A brief survey of only some of the modern research that illustrates this trend shows studies exploring the ramifications of White's (1959) view of culture as man's extrasomatic means of adaptation and Steward's (1955) concept of cultural ecology by Binford (1962) and Struever (1966); studies involving consideration of the nature of the socialization process (Deetz 1960; Whallon 1965); examination of the process of cultural drift (Binford 1963); studies concerned with the nature of stylistic differentiation of socio-cultural groups and subgroups (Binford and Binford 1966; Cronin 1962; Deetz 1960; Longacre 1964; Whallon 1965); and attempts at definition of the number and nature of tasks undertaken by prehistoric groups (Binford and Binford 1966; Freeman and Brown 1964; Freeman 1966). The best of these studies have been directed to the isolation and examination of the functional and processual dimensions of cultural systems. Much less effort has been spent in the construction of frameworks for viewing the structure of such systems. Even where attention has been given to this aspect of cultural studies, research has involved attempts to determine the existence, in the prehistoric record, of structural principles observable in (especially "related") modern societies. The method takes for granted that it is possible to derive, from the study of a sample of modern societies, elements of sociocultural structure (including whole institutions and corporate groups) which are homologous with those of the prehistoric period. Although this approach may be an especially fruitful one when applied to recently extinct cultural systems, it is likely to yield misleading results when applied to the study of cultural materials produced by more ancient societies, especially societies more than 40,000 years extinct.

THE USE OF ANALOGY

In part, the use of analogy in archeological interpretation has been due to a desire to construct categories of cultural development—"levels" of economic organization or social complexity—under the assumption that such constructs are the goal of evolutionary studies, and that the principles of the classification are derivable from our knowledge of the evolutionary process. However, the construction of such cat-

egories, which has been called “general evolution” by Sahlins and Service (1960), is really not “evolution” at all, but taxonomy. Multitudes of classifications of the same items, be they objects, organisms, or sociocultural systems, are possible (Simpson 1961). Some of those are “evolutionary” in the sense of being derived from the developmental history of the items classified, and some are not. To establish the relevance of such a classification to the evolution of the items concerned, one must base it on the historical record of development of the items. As yet, studies of the “fossil record” of cultural evolution are inadequate to serve as the basis for any evolutionary classification that is detailed enough to be useful. It is impossible to classify as yet ungathered data.

But are the data really ungathered? It is often assumed that this is not the case. Admittedly, it is said, the “fossil record” is incomplete, but we can substitute for missing elements in the record studies of the behavior of “modern representatives” of those elements. As Service says, “Certainly aboriginal Arunta culture is not younger than western civilization; it is obviously a great deal older, and precisely therein lies one of the virtues of studying that kind of culture” (1962: 8). The assumption that modern representatives of past stages of cultural development exist is a major justification for the use of analogy. Curiously, that justification is a derivative of the view that culture is an adaptive system. As Service goes on to say: “the aboriginal culture of the Arunta . . . is . . . a form of adaptation to a particular kind of (total) environment made long, long, ago and preserved into modern times because of its isolation” (1962: 8; the parentheses are his). This kind of reasoning is misleading.

It is based, of course, on the hypothesis that like environmental stimuli produce like cultural responses. In a very general way, this is true. (There are a limited number of methods of working stone by percussion. Elements not present in an environment cannot be utilized.) Nevertheless, if the statement is examined in detail, it is false. Each society exercises some degree of control over the influence of its environment by exploiting some aspects of environment at the expense of others. No society utilizes all it could of the offerings of its surroundings. In addition, the differences in the manipulation of the same resource by two distinct cultures are often great. Two “distinct” cultures from exactly similar environments, both of which are affected by exactly the same aspects of those environments, and both of which utilize identical resources in identical ways, would be part of exactly identical ecological systems. This is really the same as saying they would be one and the same culture. The validity of making inferences based upon general principles of adaptation discernible among modern populations is not denied; on the contrary, such inferences are necessary. But that is not the same as the inferential process I am attacking. In fact, it leads to contrary results.

It is known that modern populations of higher animals and their distributions are the result of a complex historical process involving long sequences of changes in adaptation to changing environments, including other animal populations. The present diversity of such animal forms is the end product of a series of developments involving numerous transitions from old to new environmental situations, either by population spread or environmental change, and numerous consequent

readaptations. In addition to this process, the complementary development of a variety of new “ways of making a living . . . exemplified in the phenomenon of adaptive radiation” (Simpson 1961: 14–15) also played a large part. Competition for resources resulted either in differentiation of forms, often involving increasing specialization in the utilization of specific resources, or in the disappearance of all but one form from the environmental locus of competition (Simpson 1961: 16–17). Sociocultural systems, like animal populations, have tended to regional-and-resource specialization during the course of human history. New ways of making a living have occurred at the same time: one can certainly speak of the dispersal of food production as an example of an adaptive radiation. Any such radiation alters the interaction between members of the invaded natural community in some way (Simpson 1961: 10). In the case of the spread of food production, the process of clearing land for planting, among other factors, altered the size and nature of animal communities, and thus altered the possibility for hunter-gatherers in competition with agriculturalists to survive. At the present time, hunting and gathering adaptations tend to exist in situations which are undesirable to food-producing peoples. Where hunter-gatherers survive in environments utilized by food producers, they have usually had to specialize in the extraction of kinds of resources least affected by food production. They must, in fact, be totally unrepresentative of the sorts of hunting-gathering adaptations that existed before the advent of food production.

Another line of reasoning that militates against Service’s hypothesis is based simply on the logical limits to prediction from a limited sample. Hunting-gathering adaptations of the present are extremely diverse. From a detailed analysis of Bushman cultural systems, it would be possible to predict very little about the social structure of the Kwakiutl. The cultures do have elements in common, of course, but those elements are of such a general nature that information gleaned from one group is not particularly useful in interpreting the behavior of the other in any detail. (It is true that in another sense a great deal can be learned from the comparison. It illustrates the diversity of forms of structural elements among hunting-gathering peoples of the present, and the dangers inherent in reasoning from one or a few such systems to all.) Now, useful and detailed analyses of socio-cultural systems have really only been made among peoples who lived during the last hundred years. The total length of time during which hunting-gathering adaptations have existed, on the other hand, is on the order of two million years or more. It would seem logical that Bushmen are many thousands of times more likely to be representative of all modern hunting-gathering groups than all such groups of the present are to be representative of the total range of hunting-gathering adaptations past and present. This is especially so because most past groups were composed of beings biologically so different from present humanity that we simply cannot assume continuities (other than such broad ones that they are relatively useless in interpretation) between their behavior and our own.

I have not meant to imply that the comparison of past and present socio-cultural adaptations can reveal no important similarities or identities. However, such parallels must not be assumed to exist before it has been demonstrated that they do. The

use of assumed similarities with modern behavior in the explanation of the behavior of extinct groups is not only fallacious, it is also deleterious to research since it prevents the discovery that the postulated similarities do not exist.

■ A MODEL MINIMIZING ANALOGY

I have attempted to establish the fact that analogical reasoning from modern behavior must be kept to a minimum in the construction of models of past cultural systems. I intend to show in the remainder of this chapter that the construction of a workable model of the structure of culture, for use in interpreting archeological materials in which a minimum of analogical reasoning is involved, is feasible, and that its application avoids the pitfalls I have outlined.

■ THE NATURE OF CULTURE

Any model of cultural structure which is to be of utility to the prehistorian must consider the material aspects of culture, since those include the observational data upon which he must base inferences about human behavior. It must be assumed, for the purposes of such a definition, that patterned occurrences of the elements the prehistorian studies can be discovered, and that when they are derived from undisturbed contexts they indicate that patterned human behavior was responsible for their existence. It must also be assumed that patterned behavior due to biological factors can be isolated from culturally conditioned behavior, at least potentially. Last, although ideas and values are important to the prehistorian as they influence behavior, values which do not become observable through some effect on behavior need not be considered part of culture. A definition of culture which satisfies these restrictions is the following: culture consists of both the total configuration of patterned activities (which are not simply referable to the biology of the actors) performed by a society, including the materials used in or produced by those activities, and the social units responsible for activity performance. This definition resembles that of Malinowski (1960) except that the focus of attention is on the end products of his institutions, and the "charter" of the institutions is equated with their "function."

I stress that the prehistorian cannot reconstruct any activity undertaken by a given society unless that activity produced some preserved material evidence. Binford, on the other hand, has claimed that it is possible to "recover, both from the nature of the populations of artifacts and from their spatial associations, the fossilized structure of the total cultural system" (Binford 1964: 425). This statement would seem at first glance to contradict what I have just said. I do not really think it does. Binford does not mean to imply that we can reconstruct an extinct linguistic system, for example, from prehistoric materials. However, the linguistic system as part of the general system of communication in a given society is also part of the mechanism of socialization, and the nature of the process of socialization certainly cannot be denied to influence the patterning of activities in the society, right down to the form of the tools made and used by social units.

■ THE NATURE OF SOCIAL UNITS

While it is relatively easy for the prehistorian to discern patterned occurrences of elements and to infer from them some of the parameters of the activities which produced them, and at least some of the norms governing their performance, it is much harder to determine the nature of the social units which performed those activities. In this stage of analysis, prehistorians have tended to refer to the patterned materials they observe as the end products of activities undertaken by corporate groups like those observable in one or another modern society. Once more, caution is necessary. In the first place it is unfortunately fair to say that few significant advances have been made from the study of modern peoples, in the ascertainment of "the extent to which the behavior patterns entailed in exploiting the environment affect other aspects of culture" (Steward 1955: 41) since Steward's formulation of the method of "cultural ecology"; there is really no body of data available in analogy with moderns that can be applied to this problem without numerous intervening assumptions. Were usable data available, even if all extant social groups were found to exhibit a given correlation between social structural type and activity patterns, I am not prepared to admit that it is justifiable to assume that past social groups with many or even most of the same activity patterns necessarily also had the social structural type that is their modern correlate. I would expect to find among extinct cultural systems at least some relations between social structural type and activity pattern that are totally unrepresented among modern societies.

Another criticism of the equation of archeological materials with the activity patterns of corporate groups can be directed at a general confusion about the nature of social groups that is manifest in that equation. Social anthropologists have long recognized that not all social groups are corporate. A corporate group can be defined as one which has a body of collective rights and duties, an "estate," vested in all members and activated in diverse situations, so that it can be said to be a "multipurpose" group (Fortes 1953; Nadel 1951: 160). In addition it may have longer existence than the life span of any member. All members of a corporate group may act as a body on occasion for the performance of some activity, or, on the other hand, only some of the members may cooperate as representative of the group as a whole. In contexts where they act as group representatives, they are recognized and recognize themselves as such, and their way of acting and their organization then follow from the rules of organization of membership in the group and its way of acting (Nadel 1951: 161). However, members of a corporate group may cooperate in contexts in which that membership is irrelevant. The structure of a hunting party need not be based upon the same principles as the structure of a composite family, even where all members of the hunting party are members of the same composite family. Some of the dimensions of group organization must vary, at least in the relative intensity with which they are stressed, as the group performs different functions.

Even though social anthropologists have tended to focus their attention on the corporate groups in society, those groups need not be the only important groups, or even the most important ones for the day-to-day survival of society as a whole.

Special-purpose groups made up of members of one or more corporate groups, co-operating to perform specific tasks and, perhaps, immediately dissolving after a very brief existence, are the basic units of action in society. They are also the units responsible for the accumulation of archeological materials. While such parties may, in fact, frequently coincide with corporate groups, the prehistorian cannot assume that they do; he must prove that they do, where possible, and this involves distinguishing the two conceptually for analytic purposes. To be of utility to the prehistorian, the definition of the "social unit" must include special-purpose groups or "parties" as well as corporate groups. (Since the culturally patterned activities of individuals can be as important to group survival as those of multi-person groups, the most utilitarian definition of the "party" is: any number of individuals [from 1 to n], who contribute to the performance of a given activity.)

■ A GENERAL ILLUSTRATION OF THE USE OF THE MODEL

The application of this theoretical framework to the study of prehistoric materials does not produce any spectacular insights about their significance. In fact, its results are not nearly as interesting or emotionally satisfying as the probably greatly misleading caricatures of prehistoric lifeways which have often been derived by the misuse of analogy. It necessitates the slow and painstaking isolation of regular types of associations of materials, and their formal equation with activity types. Only much later may an attempt at functional definition of those activities, based on the characteristics of artifacts and contexts, be made. Each activity type must first be assumed to be the result of the behavior of a distinct party type. Next, detailed examinations of the formal characteristics of the artifacts which indicate the techniques of their manufacture and reflect motor habits involved in that process (Binford and Quimby 1963), combined with microscopic study of variations in their wear characteristics (Semenov 1964), and analysis of the distribution of associated materials in the clusters may aid in the discovery that ranges of variation of these characteristics overlap for some clusters and are distinct for others. This will hopefully permit the identification of parties which are multipurposive, or involved in multiple activities. Perhaps membership characteristics of a party may in future be determinable from the recognition of individual idiosyncrasies in artifact manufacture and use. These studies in conjunction with an examination of the configuration of between-cluster spatial relationships and cluster size (the "proxemic" pattern of each occupation [Hall 1966]) may be expected to lead eventually to the discovery of the boundaries of identity-conscious social groups.

■ CONCLUSIONS

The system just outlined affords a systematic, objective method for the control of selected culturally significant aspects of archeological evidence for the purpose of intra- and interoccupation comparisons. It makes possible control over activity type,

as an example, permitting eventual study of variations in party makeup or size, or of the variation between functionally equivalent units indicative of activity performance by different identity-conscious social groups. Starting with evidence from one site, we may hope to extend these comparisons first to a few other sites, then gradually over the totality of the prehistoric period, as more excavations conducted to recover comparable evidence are completed. This method, an extension of the technique of “controlled comparison” (Eggan 1954) to archeological evidence, offers the only secure means of acquiring an understanding of the nature of the types of prehistoric institutions and the mechanisms which contributed to their maintenance or transformation (Eggan 1954: 748).

It is certainly desirable for all of us, as anthropologists, to work toward increased communication, and to make our findings as intelligible as possible to each other. But no anthropological subdiscipline has yet elicited the laws governing the structure and operation of cultural systems. The idea that prehistorians must interpret their evidence solely in terms of inferences derived from social and cultural anthropology is as fallacious as the idea that interpretations of the behavior of modern groups must be derived from prehistory. Each of the subdisciplines of anthropology studies but one part of the total spectrum of cultural behavior. No segment of the spectrum is any more important than any other. All must be combined if we ever hope to understand the nature of culture in all its dimensions, and, hopefully, from that understanding to derive general laws regulating the structure of cultural systems, their interrelationships, and the processes whereby they are transformed.¹

NOTES

Sackett’s unpublished paper entitled “Archaeological Interpretation in the Upper Paleolithic,” delivered to the AAA annual meeting in 1965, also incorporates some of the same elements in a model of cultural systems. This paper did not come to my attention until it was circulated for the Man the Hunter Symposium, after my essay was completed.

1. I am aware that many of the ideas expressed here are the results of the genius of others, especially F. C. Howell, L. R. Binford, J. D. Clark, J. Sackett, R. Klein, and, more recently, J. Deetz and D. Damas. I suspect that the ones I consider original are also secondhand, and that I have simply forgotten where I borrowed them. The total configuration is my own, however. It was helpfully criticized by R. Klein, C. Merbs, and S. Tax, and students at the University of Chicago, while I was writing the drafts. I am grateful for the advice I followed, and apologize for having ignored the rest.

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The Fat of the Land

NOTES ON PALEOLITHIC DIET IN IBERIA

In discussing the difficulty of interpreting prehistoric behavior from the evidence in the archeological record, Christopher Hawkes characterized the study of technology as easy, inferences about subsistence economics as operationally laborious but relatively simple and straightforward, reasoning about social-political institutions as much harder, and the study of religious institutions and spiritual life as hardest of all (1954: 161–62). It is scarcely possible to dispute his general diagnosis, which expresses a basic tenet of prehistoric research. Nevertheless, Hawkes's statement hides a paradox; in specific cases a great deal is known about other aspects of subsistence-related technological systems, but there is very little unambiguous evidence for diet during the Paleolithic period, simple though that study theoretically ought to be. The subject is far knottier than is generally granted, and authors who undertake to produce an original synthesis of dietary data find themselves forced by the nature of the subject to speculate more than they might wish.

The deficiencies of our dietary analyses are not solely due, as is so often the case in Paleolithic research, to any absolute paucity of potentially relevant data, for data of certain kinds are abundant in many sites, although we often fail to collect them. Good prehistorians are generally aware, at a theoretical level, of the potential of the data, and appropriate data-gathering techniques are available. Several simple, readily

practicable, and often inexpensive methods for recovering information relevant to the study of Paleolithic diet exist, such as flotation (Struever 1968). Even though no methods can yield more than partial pictures of dietary practices, their consistent application to Paleolithic site sediments would increase our recovery of such information by several orders of magnitude.

Major obstacles to the study of dietary evidence stem, in my opinion, from two factors. First of all, it is unfortunately true that the study of Paleolithic prehistory bears at least its share of scientific inertia. Its practitioners usually profess an interest in reconstructing prehistoric lifeways, recognizing the great potential importance of information derived from the study of contextual evidence (including the topographic situation of the site; the nature of contained sediments; chemical, radiological, and biological residues; and the positional and numerical relations of recovered data). In practice, however, most of us still place overwhelming emphasis on the analysis of artifacts in stone and bone and the chronological ordering of artifact assemblages, relegating to a secondary position the study of all contextual materials except those useful in climatic reconstruction or dating. Unfortunately, stone and bone tools studied as such provide little evidence of diet.

The collection procedures required for maximal recovery of Paleolithic dietary information are undeniably time-consuming. For example, at the Mousterian site of Abric Agut, in eastern Spain, for every bulk sample that yielded seeds when subjected to flotation, there were 25 that yielded none; furthermore, we were overjoyed that our recovery ratio was so high. Obviously, an intensive attempt to gather dietary data requires a substantial shift in the mental set and excavation priorities of the average Paleolithic prehistorian, who has been trained to dig to recover artifacts and identifiable bones, and to invest only a minimum effort in collecting suites of samples for sediment, pollen, and chronometric analyses. Intensive sampling for dietary study also demands additional personnel on the field and laboratory teams and thus increases excavation costs. Perhaps it is understandable (though not excusable) that such sampling has not been a customary part of the average Paleolithic excavation.

Even in those rare cases where materials with significance for dietary studies are routinely collected by prehistorians, they are ordinarily gathered for other reasons, and their potential contribution to the study of subsistence is frequently ignored or undervalued. So, for example, faunal material lacking the diagnostic characteristics which permit species identification is simply discarded by many investigators, often without counting or weighing the fragments or examining them for marks of intentional human activity. Only because all such specimens from the Mousterian levels at Cueva Morín, in Cantabrian Spain, were carefully examined were we able to discover that the bones from Upper Level 17 are not primarily food remains.

A second set of considerations is at least as great an obstacle to the study of Paleolithic diet. Excavators who have conscientiously collected samples of contextual materials for analysis sometimes discover to their great frustration that specialists competent to identify the remains and explain their significance are impossible to find or are not interested enough to help. This problem still plagues our work at Cueva Morín. These two factors have interacted to produce an unsatisfactory state

of affairs in which the total amount of solid evidence available is insufficient to support broad generalizations about subsistence patterns.

■ THE LIMITS AND POTENTIALS OF DIETARY DATA

Although we often assume that certain categories of organic material recovered from Paleolithic sites are residues of meals eaten by prehistoric people, that assumption may be unwarranted in any specific case. Some such items may be the raw material or by-products of manufacturing processes unrelated to diet. Evidence about food, fuel, and raw material *acquisition* (hunting-gathering operations, butchering techniques) is commonly reported as though it were direct evidence for *consumption*, which, of course, it is not.

Data potentially relevant to dietary studies often have ambiguities that can only be resolved after thorough and thoughtful study. Some of the reasons for these ambiguities are outlined below.

The prehistorian reads the records of the past in its relics and the situations in which they are discovered. For the most part, only the relatively imperishable relics remain, but the recovered items may provide no evidence at all about diet, or the picture they present may be biased, owing to its incompleteness. True residues of human activities in an archeological site are never a fair sample of all the imperishables resulting from those activities, because a single site is not an entire prehistoric settlement system. Furthermore, prehistorians never recover a fair sample of all imperishables in a single level; some always go unrecognized, and imperishability in the archeological sense is a relative condition anyway. In addition, the materials recognized as important at any stage of our discipline's development are almost never distributed uniformly over the surface of an undisturbed archeological level, and because we very seldom excavate a level completely, we always miss some of them. This injects another element of bias into our interpretation. Even if we could recover an unbiased sample of all diet-related imperishables produced by an extinct human society, it would not give a complete idea of the diet of the times, since a large proportion of any past meal may have consisted of foodstuffs that do not leave anything we now recognize as a durable material trace (beverages, boned meat, greens, ground meal, and so on).

For present purposes, we may distinguish two kinds of prehistoric evidence. When a substance in which we are interested is itself recovered, the evidence for its presence is unequivocal and may be called primary. Sometimes the substance itself no longer exists in recognizable form, but other indications of its presence, such as traces of chemical decay products, may be detectable with appropriate procedures. This evidence is, of course, secondary, but in rare cases it may be virtually as unequivocal as primary evidence. Crosscutting this distinction is another, which can only be made when the purpose of the investigation is known. That is the dichotomy, familiar from legal usage, between direct and circumstantial evidence. In dietary studies, we try to determine what was actually consumed by past human groups; any direct evidence depends on proving that the material in question really found its way

to the human gut. As a result, there are just two kinds of direct evidence for food consumption. When a body is as well preserved as those of the Tollund, Grauballe, or Borre Fen corpses, an actual analysis of stomach contents may be possible (Glob 1969: 56–57; Helbaek 1969: 207–8); no such miracle of preservation is known for the Paleolithic period. The only other direct evidence for food consumption is the presence of food remains in hominid fecal material (coprolites). A few possible hominid coprolites have in fact survived from very remote periods (R. Leakey 1971: 67; de Lumley 1966), but they are very rare and none is identified with certainty from the study area discussed here. Even where it does occur, such direct evidence can never provide a complete picture of past diet; it only gives us partial information about the represented meals, which are in turn an infinitesimally small proportion of all the meals eaten by the individuals in question.

In the overwhelming majority of cases where primary evidence of potential food materials is recovered, we still have no more than circumstantial evidence of their consumption. The strongest kind of circumstantial evidence would be the discovery of hominid tooth marks on the material, but I know of no unequivocal case of such data from a Paleolithic site. I am sure that among the masses of unidentifiable bone fragments from Paleolithic sites some will eventually be found with convincing tooth impressions, but finding them will require much closer attention than is ordinarily extended to bone debris.

The primary materials with dietary potential that one may ordinarily hope to recover, with care and luck, from at least some sites are the range of durable animal, plant, and edible mineral remains. For animals these include bones, teeth, antlers, mollusk shells, otoliths, scutes, carapace and plastron fragments, and (rarely) hair, horn, scales, and bits of beetle elytra. For plants, carbonized plant material and opal phytoliths (microscopic remains of the siliceous skeletons of plants) are our primary evidence. Unfortunately, species identification from some of these materials (phytoliths, for example) is still so difficult and our knowledge about them so rudimentary that their analysis has not yet made the contribution we hoped for a few years ago. Pollen is ordinarily no more than secondary evidence for the presence of plants in a site and is very unreliable, circumstantial evidence at that, since it is ordinarily transported to the site by currents of air or water, or on the bodies of animals or people, or on clothing, or enters in other ways beyond conscious human control. However, when pollen from a plant species occurs in sediments in large clumps, the deliberate transportation of flowers to the site may be indicated, as has been claimed for a Mousterian level at Shanidar (Leroi-Gourhan 1975). Edible mineral salts, easily leached from archeological levels, may perhaps be recovered from dry sites someday.

Prehistorians do a much better job of collecting most kinds of primary evidence of plant and animal remains in the levels they excavate than they do with the trickier collection of secondary evidence. However, secondary evidence is extremely important to sound interpretation, since a number of materials would be undetectable otherwise. Among the most important kinds of secondary evidence for dietary studies are chemical traces and microbial spores and particles (Burrows 1968; Graczyk

1971; Graham 1962; J. Jay 1970). For years, it has been recognized that the decay of organic materials (bone, kitchen wastes, fecal material, etc.) in an occupation horizon results at least temporarily in detectably higher phosphate levels than those characterizing adjacent layers that had lesser organic content. Under appropriate conditions the higher phosphate values may persist for millennia, and a stratigraphic profile will show the relative intensity of organic detritus accumulation in each level. Such information is quite crude compared to the results of chemical studies made possible by modern technology. Spectroscopy (Britton and Richards 1969), X-ray diffraction studies (Brothwell et al. 1969), and neutron activation analysis (Jervis et al. 1963) can detect and measure tiny quantities of trace elements, permitting the recognition of such characteristic and complex molecules as amino acids in archeological horizons.

Spores of certain microbes (some bacilli, yeasts, molds, and fungi) persist in recognizable form in Paleolithic horizons, and it is theoretically possible that some prehistoric spores can be identified and perhaps even cultured. Since some microbes (obligate parasites and obligate saprophytes) are only associated with one or a very few specific host media, concentrated patches of these forms would suggest the former presence of long-vanished animal or plant tissues. Even virus particles may someday be identified in Paleolithic horizons. The major obstacle to the search for Paleolithic microbes is the great difficulty of securing uncontaminated samples, but the prospect of recovering evolutionarily antecedent forms of "antibiotic" microbes has enough potential to interest large pharmaceutical companies, and with their help we may hope to see important advances in "prehistoric microbiology."

Food consumption is the last stage in a variable sequence of subsistence-related events, some of which may provide other kinds of circumstantial evidence relevant to dietary reconstructions. Food acquisition is the first step in the sequence. Food may of course be eaten immediately where it is acquired. Unless such foraged meals are detected as coprolites or stomach contents, they leave no durable trace in the archeological record. Among some societies today, much of the total dietary intake is consumed on the spot; this is especially true for small, perishable items such as berries or shellfish. In some groups where there is a pronounced division of food-acquisition activities by age and sex, during certain seasons children and women may regularly satisfy their major dietary requirements for days at a time in this way. Prehistorians must always be aware of the possibility that they are recovering remains of the meals of just one segment of the population, and so their observations may have only partial validity for the society as a whole. At present there is no apparent way out of this dilemma.

When food items are not consumed as they are being collected, they may be brought to what will later become a recognizable archeological site. This may be anything from an ephemeral resting place used while certain activities are undertaken at some distance from the group's headquarters, to its temporary or permanent "living area" or base camp. In this case, durable remains of diet-related activities may perhaps accumulate at the site. Where a temporary surplus of foodstuffs is available, a society may develop special techniques for storage over shorter or longer

periods. Storage pits, “silos,” cairns, or tanks may be constructed to contain these materials and protect them from competitors, large and small. Careful study of such features may provide evidence that they were indeed used for foodstuffs instead of serving some other purpose, but the proof is not easy. Nor are all the possible food-storage devices represented in the archeological record, since food may be kept in perishable containers such as boxes or skin bags, or may be protected by suspending it high in the air, or placing it on a platform atop a post or in a tree. Nevertheless, the features that do survive may provide significant evidence of subsistence practices.

Perishable foodstuffs also need to be preserved if they are to be stored beyond the normal period of their “palatability.” Opinions about palatability vary widely from group to group, of course, and prehistorians must keep their own ethnocentric biases from influencing their interpretations in this context. There eventually comes a time, however, when most biotic materials in most environments become so rotten that they are toxic to humans. Many societies have discovered techniques that effectively delay this decay process for appreciable periods.

Where there are cold seasons, foods may be preserved by chilling, because near-freezing temperatures slow down the metabolism of food-spoilage microorganisms. Roots and tubers may be kept in dark, humid containers at 6°C for several months. At temperatures of 0°C, fresh meats will keep for a week or more, and at -18°C most meats other than organs may be kept up to two years and on thawing will have virtually the same palatability as when they were first frozen (J. Jay 1970; Paul and Palmer 1972).

Heat will also slow or halt the microbial spoilage of food, since high temperatures can destroy all or most of the decay organisms present. One advantage of the “perpetual stewpot,” where fresh food is added to the pot each time a portion is eaten, is that the food is regularly reheated. Unfortunately, direct evidence of food preservation by these techniques would be unrecognizable in most Paleolithic sites.

Since the metabolic processes of microorganisms require water, food may be preserved by drying. If food is dried by the sun or the heat of a fire, direct archeological evidence of the process is unlikely to result. However, drying may be done by plasmolysis, which occurs when the food is surrounded by high (hypertonic) concentrations of salt or sugar. In some cases, residues of those substances might survive but by themselves would be no more than circumstantial evidence of food preservation. Residues of other chemical substances used to kill microbes or retard their growth might eventually be recoverable. Wood smoke, for example, contains antimicrobial chemicals (aldehydes, alcohol, phenol, cresol, and others) that add their action to the preservative effects of drying and heat (J. Jay 1970: 117).

Undesirable microbial action can also be slowed in certain cases by subjecting foods to the intensive growth of specific microorganisms that the human gut tolerates. This encouraged growth results in a controllable fermentation, which produces an unfavorable environment for the undesirable decay-producers. Sour cream, pickles, yogurt, cheese, and alcoholic beverages are familiar fermented foods on our tables, but the process is not restricted to modern industrial society. Some food-gathering peoples, especially in northern latitudes, use controlled fermenta-

tion to preserve meat, fish, and berries. Traces of the microorganisms responsible for fermentation, or the lactic and acetic acid resulting from their metabolism, may someday be recovered from Paleolithic sites.

Most human foods may be consumed raw, without special preparation. Infrequently, hunter-gatherers use foodstuffs that must be treated in special ways before they become edible. In California, several genera of highly nutritious acorns are so rich in tannic acid that this substance had to be removed before the acorns could be eaten. Sometimes the nuts were hulled and buried for long periods. Alternatively they could be dried and ground into meal with mortar and pestle; the meal was placed in baskets or shallow basin-shaped depressions and then repeatedly soaked with water. As the water passed through the meal, it leached out the bitter tannins. The cyanic acid in wild plum pits and buckeyes was removed in the same way (Kroeber 1953).

Cooking is the most widely used technique of food preparation. Primary evidence that potential foodstuffs have been subjected to the action of fire is widespread, in the form of charred animal remains. Carbonized plant remains are more rare but have been found in Paleolithic contexts. These materials may have been burned for other reasons, either accidentally, or as part of the food-preservation process, or because they were used as fuel. But, where such material is abundant, we should be able to rule out one or more of the possible explanations on the basis of the nature of the materials, the pattern of charring, and the contexts in which the items were recovered.

Fireplaces are ordinarily no more than circumstantial evidence of cooking, since fires may also have been used to provide warmth and illumination. In addition, from at least Solutrean times, fires were used to make flint more workable in the toolmaking process, especially when pressure-flaking was involved. Thus, caution should be exercised in interpreting the remains of fireplaces as indicating that food was cooked. Paleolithic hearths are often not informative—although they exist in considerable variety, most seem to be variants of the open fireplace, with or without draft trench or reflector. Possible exceptions are the hearths at two Upper Paleolithic sites in the Corrèze (Coumba del Boitoü and le Pré-Neuf), which contained slab-walled chambers that may be ovens, and the pits that may be ovens on the peripheries of a large hearth at Dolní Věstoniče (see Breuil and Lantier 1959: 104; Klíma 1963: 125; Perlès 1976: 680–81). Occasionally, large patches of partially carbonized vegetation have been found, and these might be the remains of smoldering fires built to smoke meats, although other interpretations are possible.

These lengthy introductory remarks illustrate both the wide range of data about diet which can be recovered (at least theoretically) from Paleolithic sites, and the many interpretive problems the prehistorian faces. We must always be wary of conclusions based on isolated finds. Reliable information can be obtained only where a number of lines of evidence converge.

Paleoenvironmental reconstructions provide us with the data needed to assess the past potential of a region for hominid subsistence. They afford much background information about resource availability that may clarify the hard evidence

of actual behavior. Naturally, even the most reasonable attempts at assessing the potential offerings of the area accessible from a site will not tell us what actually happened in history; at best they show us what might have been. There has recently been a resurgence of interest in studying the environment as a key to understanding past behavior (see, for example, Higgs 1975; Higgs and Vita-Finzi 1972). The approach taken by the “site catchment analysts” (who seem to assume that whenever a resource is available, it will be exploited) glosses over both the known complexities of hominid behavior and the great difficulties involved in reconstructing prehistoric environments in useful detail. Nevertheless, it is self-evident that we cannot properly evaluate the finds from an occupation level unless we see them in the context of their relationship to the prehistoric environmental setting.

■ APPENDIX

We are all inclined to accept the affirmations of other archeologists without hesitation. Sometimes, we should be less uncritical, especially when those affirmations concern prehistoric food practices. I have given some of the reasons that we should only accept such affirmations after careful scrutiny in this chapter. The impact of those considerations on archeological dietary interpretation can best be seen if a few examples are given.

Plant Remains and Diet

A large mass of carbonized vegetation found in one level at Torralba might have been thought of as a cooking fire or an area where plant food was prepared. However, it is just as likely to have been a large smudge, unrelated to cooking or the preparation of plant food. In the Mousterian levels at Abric Agut, on the other hand, I have interpreted the remains of charred seeds including that of a sea-beet that were found close together in one Mousterian level as food remains. I came to that conclusion based on the limited area of the distribution and the fact that the seeds were charred (Freeman 1981).

Incidentally, I now know from our experience with flotation at the Magdalenian site of el Juyo that seeds can be preserved without charring. There, the distribution of the plant remains recovered by flotation suggests that what we found was the result of periodic house cleaning: the seeds were found in the area where structure walls met floors, rather than in the centers of rooms, whatever the original reason for their presence. Seeds such as “stick-tights” (*Bidens*) could have been introduced to the site on animal skins or clothing. Others, for example blackberry seeds, might be the remains of human food, but might also have been introduced by rodents.

At Shanidar, Arlette Leroi-Gourhan has identified clumps of pollen from flowering plants, indicating the former presence of whole flower heads in one Mousterian grave. The flowers may have been placed in the grave to heal the dead person, since several of the species have medicinal value (Leroi-Gourhan 1975). But the reasons for challenging this interpretation are given in Chapter 9.

Remains of Invertebrates

Both at Devil's Tower and the better excavated Gorham's Cave nearby, the Mousterian levels contain shellfish that could come from storm beaches. That is especially worth considering at Devil's Tower, where the shellfish list includes *Tritonium*, *Lucina*, and *Pecten*, deep-living or free-swimming species that would probably not have been caught alive by Mousterian food collectors. Their shells might of course have been picked up as oddities and brought to the site. The species list is also much more extensive than it is when the shells are more likely to represent food residues. This is not to deny that some of the shellfish (and the represented land snails) could be the remains of meals; some marine mollusks even show signs of burning (Garrod et al. 1928; Waechter 1964).

On the contrary, I find little reason to doubt that most of the thousands of mollusk shells in Magdalenian Level 8 at el Juyo were food remains. The species represented are overwhelmingly of two genera: limpets (*Patella*) and winkles (*Littorina*). Both are easily collected from the area between the tides and from the splash zone; they were distributed in lots about the size of a human head, as though they had been discarded as garbage after meals; a few were charred; some contained small-backed bladelets that may have been used to sever meat from the shells. Where there is little or no evidence that the shells served a technological function or were deliberately perforated as decorations, quantities of shellfish remains do seem best explained as dietary items.

Birds and Small Fauna

It is obviously not the case that every animal bone recovered from an archeological horizon need be an immediate reflection of prehistoric diet. As an example, the Acheulean Aridos quarry site JR-AR-01 yielded a series of small faunal remains that closely resembles the list of prey hunted by the raptorial black kite, that could have perched at the meander edge where the site was located (Santonja et al. 1980). This suggestion has been rejected by Mourier-Chauviré (1980), the avifaunal expert, but for reasons that I do not find convincing. I am also inclined to think that the skeletal parts of aquatic birds recovered at Torralba and the birds, anurids, and rodents from Ambrona died natural deaths unrelated to their potential dietary use by early people.

Dorothy Garrod found over 30 species of birds in the bones from Mousterian levels at Devil's Tower, Gibraltar (Garrod et al. 1928). That list may have little or nothing to do with past human dietary preferences, however, since although some of these creatures could have been taken while nesting in the cliffs above the site, many are raptors or carrion-eaters (eagles, hawks, and buzzards) that would have been large and ferocious enough to have been formidable prey. They survive by hunting just such creatures, the other sorts of migrating birds in the faunal assemblage, as they nest or rest on the overlying cliffs.

The presence of barn owls at el Juyo when the cave was not occupied by humans is apparently attested indirectly by our discovery in the Magdalenian levels of

the remains of the very small animals (rodents, etc.) that make up their diet. We do not believe that these small creatures were sought as food by humans. The small mammal remains from some sites may be represented because their pelts were used to make clothing.

Larger (Mammal) Bone

The faunal makeup of the bone assemblage recovered during the excavation of most sites is not a fair representation of all the animals hunted or eaten by prehistoric humans. In the first place, smaller animals are usually overrepresented, because it is simply harder to drag the large body parts of a bison or elk from the kill site to a camp than it is to bring home a whole rabbit. Then again, larger bones may be saved or moved about as raw material for tool manufacture. At Cueva Morín, some bones were apparently weathered for several seasons to free them from their periosteum, and even if they were originally food-related, the lapse of time between the relevant meals and toolmaking complicates their dietary interpretation (González Echegaray and Freeman 1998). At el Juyo, too, elk shoulder blades were decorated and burnt, indicating that they are something more than simple food remains. That is the case as well for some other bones. Elk ribs were also used as shovels and elk acetabula as lamps at that site, and in one case a cervid metapodial was turned into three “dice.”

There is a single case of an elk rib bearing human tooth marks at el Juyo. Judging from the impressions, the dental arcade that produced them belonged to a young, perhaps pre-adolescent, individual. The rib must have been soft (perhaps boiled?) or the bite very intense to leave the marks we found. But the impressions could have resulted from biting to relieve the intense pain of a surgical operation.

A famous prehistorian and student of Paleolithic art has speculated at length about the meaning of the “fact” that the species shown in cave paintings are not the same, or present in the same proportions, as the ones represented by the bones in archeological levels. While the calculation may perhaps be true (for specific caves it may not be), his observation is essentially meaningless. It certainly does not imply that the painted animals were the ones the artists could not get enough of, or were trying to attract magically, or beheld in visions.

Cooking Pits at Altamira

We excavated in the vestibule of the famous painted cave of Altamira in 1980–1981. In Level 2 (Magdalenian) we recovered two pits filled with mammal, bird, fish, and (very abundant) mollusk shells (Freeman 1988). Most of the thousands of shellfish remains recovered were the shells of limpets (genus *Patella*). These were part of the fill dumped into the pits once they had been emptied. The presence of ash in the pits, the presence of charred bones, the abundance of mollusks, and the limited number of mollusk species found all seem to indicate that the pits had been used for cooking.

Conclusions

In my experience, there usually are so many complications in the interpretation of possible dietary-related items that most conclusions about Paleolithic food practices, while they may be plausible conjectures, remain no more than conjectures nonetheless. In virtually all cases, implications of excavated remains for dietary interpretation are dubious at best. Only the abundance of shellfish remains seems to provide some direct dietary information, but as Devil's Tower shows, one must approach their analysis with caution.

Nevertheless, I still continue to believe that the occupation residues found in well-excavated sites can provide evidence for Paleolithic diet, if studied with sufficient care. Virtually all the large mammal bones recovered are likely the remains of past meals, but there is no guarantee that the animals documented were consumed together at the same time, or that their proportional representation mirrors their relative abundance in past diets, or even that they were the only animals consumed by the inhabitants of a site. Plant foods can be especially difficult to identify: even where they are recovered, plant parts cannot be assumed to be remains of meals or attempts to cure disease or stanch wounds simply because modern examples are thought to be good as food or medicine; virtually all plants have such uses. Mollusks seem to be easier to interpret (cautiously) but one must always be aware that their food value may have been very much less than was the case for other potential diet-related items. The remains of birds and the smaller mammals, amphibians, and reptiles can be very hard to interpret. In some cases, storage devices or cooking pits may be identified and provide (usually) indirect evidence for dietary practices. The identification of chemical or bacteriological residues of vanished foods is another route to dietary interpretation that has scarcely been explored.

The difficulties inherent in dietary interpretation from archeological residues, while not insurmountable, are far greater than Christopher Hawkes realized. In fact, paradoxical though it may seem, in many cases I think that it would be far easier to reconstruct a past socio-political system or some of a society's religious beliefs than to reconstruct ancient subsistence economics from archeological evidence.

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P A R T



AN OVERVIEW OF THE PALEOLITHIC

There are two chapters in this section. Their scope is broad and has implications that go far beyond my limited field of experience. Although most of my own research has been centered on Spain, in the first chapter of this section I attempted a more ambitious synthesis of all we thought we knew about the Paleolithic past some thirty years ago. As one of the very few U.S.-trained prehistorians who has been privileged to excavate sites from all three Paleolithic periods—Lower (Torralba, Ambrona, Castillo), Middle (el Conde, Morín), and Upper (el Conde, Morín, el Juyo, Altamira)—I feel that my opinion may have some validity yet. I stand by most of the observations in this chapter, and particularly those having to do with the utility of an information-theoretical approach, the lack of any strong adaptive advantage accruing to the hominid groups who invented stone tools, the rudimentary nature of Oldowan artifacts, the gradual nature of cultural evolution, and the difference between “technique-oriented” and “region-and-resource-oriented” adaptations. One might well ask why I have chosen to include an article based on such evidently outmoded data. The answer is that although my analysis may not conform to the most up-to-date fads, its conclusions still seem to hold. The test of the validity of reasoning in the field of paleoanthropology has always been its conformity with the facts, and in this case the Paleolithic realities still seem quite congruent with my conclusions.

If I were to rewrite this chapter right now, it would of course be different in some respects. I would add several observations. I would be careful to distinguish between fully effective cultural systems and fully modern cultural elaboration. There is now more evidence of chimp tool manufacture, this time in stone. The date of the earliest stone industries associated with hominids must now be pushed back considerably in time. The dawn of “wild harvesting” would be restated as Solutrean rather than Lower Magdalenian. Several sites, such as Dmanisi, los Aridos, Romani, Sidrón, Atapuerca, and many others, have been more recently discovered or newly excavated and ought to have been mentioned. Particularly relevant to the Spanish case, I would now discuss the suggested classification of some gracile *Homo erectus* as *Homo antecessor*. I would also deal with the implications of more recent finds such as *Ardipithecus ramidus*, *A. kadabba*, *Sahelanthropus tchadensis*, and *Orrorin tugenensis*, some of which seem less specifically early hominid than they are close to the root of our relationship with chimpanzees. In light of current debates about the “Hobbit,” I would also have to rethink my feeling that once the Neandertals disappeared, being replaced by modern *Homo sapiens sapiens*, only a single hominid species survived. Last, I would revise the number of stone tool types as a function of collection size downward. Better data than were available thirty years ago suggest that through the Middle Acheulean, in any reasonably sized assemblage, one tends to find about as many types as the square root of retouched pieces, although during the Upper Paleolithic that figure often reaches, but seldom exceeds, twice the square root, rather than the 2.5 times the square root indicated in the text. (I do not claim that this is a “law” but rather the result of empirical observation, and others, using different definitions of “tool types,” will arrive at somewhat different formulations. The fact remains that, excavation techniques being equal, no Upper Paleolithic assemblage will prove to contain many more types than an earlier assemblage of comparable size, although such is sometimes expected to be the case.) None of these changes, abundant though they are, affects the general validity of my conclusions, and I still maintain that they are still more reasonable than any alternative and did not just seem so at the time when they were written.

What kind of analysis can be done when all that is known is the spatial distribution of sites with different contents? Geographers have devised several tests for data of this kind. First of all, there are the “nearest neighbor” tests that show whether sites have a tendency to cluster in the landscape instead of being distributed more or less evenly over it. An even distribution might be expected if all of the sites represented more or less the same range of activities and if the essential resources for survival were also evenly distributed. On the other hand, if those resources tended to be found only at specific places, we might expect that sites would cluster around those places. (These are the common assumptions most people make about Paleolithic sites and their locations.) My first application of such a test showed that Cantabrian Paleolithic sites are clustered rather than uniformly distributed; however, differences in past resource availability did not seem to be the whole explanation. The techniques of site catchment analysis may have appeared to offer the means to their analysis, but obviously there was no way to reconstruct early landscapes in sufficient

detail for use, and the assumptions the theory makes about the distances Paleolithic people might have been willing to travel for access to resources were not realistic.

Geographers have also studied distributions using Thiessen diagrams or Voronoi tesserae, but usually the technique has assumed that sites are not all equivalent: modern cities, shopping centers, and so forth are both hierarchically arranged and dependent on ease of transportation from centers to ancillary sites. On both counts, geographers have studied distributions of places that are thought to be very different from the supposedly “egalitarian” Paleolithic sites. But an attempt to apply this descriptive test shows us that these well-tried geographic techniques for the description of site adjacency, commonly in use for the study of the settlements and trade routes of much later periods, may be of utility in the study of Paleolithic settlements. What is more, they reveal the existence of previously unsuspected hierarchies in our data.

The chapter on Voronoi tesserae is an adventure into rarified theory. However, the data for this chapter are less satisfactory than those for the rest, so its conclusions are speculative at best and need to be verified with more complete information. Nonetheless, the test raises some interesting possibilities and should change our way of thinking about the complexities of Paleolithic life.

By Their Works You Shall Know Them

CULTURAL DEVELOPMENTS IN THE PALEOLITHIC

Mastery over nature began with the development of the hand, with labour, and widened man's horizon at every new advance. He was continually discovering new, hitherto unknown, properties in natural objects. On the other hand, the development of labour necessarily helped to bring the members of society closer together by increasing cases of mutual support and joint activity, and by making clear the advantage of this joint activity to each individual.

—FRIEDRICH ENGELS

“The part played by labour in the transition from ape to man” (1896)

INTRODUCTION

From the materialist viewpoint essential to the paleoanthropologist, cultural systems are socially—rather than biologically—transmitted behavioral complexes by which some organisms mediate their relationship to their surroundings, including other organisms (Kummer 1971).

As the cultural means of adaptation becomes fully efficient, it serves to mediate between organisms and environment in several ways. First, it alters some set

of natural resources, selected deliberately or unconsciously by members of society from among the larger range of environmental offerings. Second, it keeps some set of natural environmental factors which could be deleterious to their survival from impinging directly on a sufficiently large number of the organisms to permit the social group to survive. Third, it provides for the socialization of new members, and provides them with some shared set of cognitive orientations. Fourth, it orders the culture-bearing organisms both with respect to each other and with respect to their access to the set of relevant natural resources. In the process, it secures the satisfaction of at least a minimum essential set of biological and psychological requirements for the necessary number of organisms sharing this means of adaptation. By satisfying those needs more efficiently within the social context than would be possible outside it, the cultural system ensures the replacement of individuals who leave the socio-cultural group permanently by new recruits (Aberle et al. 1950; Kummer 1971). Cultural adaptations effect changes in the natural environment, and it is the culturally altered environment to which the species must then adapt, biologically as well as culturally. The most successful, fully efficient cultural systems available to modern men create largely artificial environments characterized by such features as many-family urban residences; rapid long-distance transport; controlled indoor climates (and accidentally altered outdoor climates); deliberate large-scale and long-term information storage, retrieval, and manipulation systems; and modern drugs, medicines, and health care. The implications of such thoroughly altered ecosystems for the biological evolution of our species are extremely important. Even though cultural alteration of environment must have been thousands of times less drastic during the earlier history of hominid existence, it has not been a completely negligible factor for at least the last two and a half million years.

Clearly, judgments about the effectiveness of the cultural adaptive systems of our ancient ancestors and relatives must be largely based on the durable material traces of their activities. (Some evidence of undeniable value is also provided by the skeletons of the animals themselves, but this chapter is only peripherally concerned with hominid body morphology.) Material residues of prehistoric human activities include both recovered artifacts—objects made or altered by man and their contexts—the containing sediments; associated biological, mineral, chemical, and radiological materials; and the positional and numerical relationships between these categories. Most earlier excavators deliberately or unconsciously focused their attention on lithic implements above all else. That is understandable. After all, lithics are ordinarily the longest-enduring intentional material products of human craftsmanship. However, we now realize that artifacts alone are potentially far less enlightening about past lifeways than is the total configuration of artifacts, contexts, and relations.

An evaluation of human capacity, based on preserved material residues of human behavior, can be successful only insofar as the variety of that behavior is directly reflected in aspects of the recovered materials. Occupation residues are regarded as analogous to communication channels: they are the media by which information is transmitted from the prehistoric past to the modern world. The information they

contain has been stored in material form—just as though the residues were written documents—and an appreciable part of the information can be decoded and understood by prehistorians and paleoanthropologists. But lithic implements contain only a very small part of the message from the past. As communication channels, their capacity is limited; the total variety of information they preserve is restricted to an extreme.

The size and nature of available raw material, the limited technological means available to shape it, the size of the human body itself, the strength of the average stoneknapper and tool users and the kinds of tasks in which tools were to be used, all imposed severe constraints on their variability. The shape and size of a useful stone tool were not subject to unlimited arbitrary variation. By themselves, lithic implements can give us only the feeblest reflection of the complexities of mental processes of prehistoric men.

In contrast, the variety of information effectively transmitted in any modern spoken or written language is immense. That is because languages employ a sizeable number of arbitrary symbolic units of information (e.g. phonemes, letters), which may be recombined in several different ways, so that the number of different symbols which might potentially occur at any place in a message is great (Carroll 1955). Information theorists commonly measure the amount of information transmitted in “bits” (binary digits), each of which can be thought of as a single dichotomous specification (a division of alternative meanings into applicable and nonapplicable sets). Each bit added doubles the total number of distinct meaningful items that may be produced. Written English has been estimated to use about twenty-six bits of information (that would be sufficient to produce all the written words in current use), but no single individual uses anywhere near this theoretical capacity. College graduates may have vocabularies surpassing 100,000 English words (most of which would not be used in ordinary conversation). If each of the 100,000 words were equally probable, 17 bits would provide more than adequate information-carrying capacity for their unequivocal transmission. Daily speech might require only 13 or 14 bits. Since all transmissions are affected to some extent by “noise”—interference which obscures or alters the content of parts of the information sent—all communication systems incorporate a certain amount of redundancy to ensure that the meaning will get through.

It is impossible to specify with precision the number of bits of information which might be carried in lithic implements considered as communication channels; for one thing, that number changed from time to time and place to place. Nonetheless, the variety of information which can conveniently be stored in flaked stone implements within the constraints I have mentioned has always been low—probably never more than eight or nine bits at any time. All other things being equal, the amount of information which can potentially be transmitted by a lithic implement must vary directly with the overall size of the artifact, the extent to which it is altered by re-touch, and the number of distinct working edges it displays. Obviously, more plastic or malleable materials are often relatively more easily subjected than chipped stone to decorative treatment which does not affect whatever technological functions they

may possess. Such materials and their decorations have a considerably higher potential capacity as communication channels than stone tools. Joining several individual pieces to form a single compound tool further increases the potential variety of information the tool can transmit. Nevertheless, even at their most variable, single implements considered in isolation can carry no more than a small fraction of the potential information which can be gleaned from the same implements analyzed in the total contexts in which they were recovered. That is because so much additional information is stored in the nature of surrounding sediments and associated materials, relationships between the frequencies of recovered items, and the positions in which they are found. To maximize information recovery, we should look on the occupation horizon as a whole rather than the individual implement or the artifact type as the communication channel. Unfortunately, that obviously cannot be done with collections from early excavations, and we are often forced to rely exclusively on the lithics.

Even under ideal conditions, information from the prehistoric past does not get through to us entire and unchanged. As a communication channel, the prehistoric record is exceptionally “noisy.” The information we can recover has been extensively altered by several kinds of interference and it is only after the most careful and intensive efforts by the analyst that the message may be decoded. Something is known about the nature of the noise in the fossil record, and we are gradually learning how we may allow for part of it, at least, in the decoding process.

We may express the relationship of the information in the fossil record to the relationship in the living system which produced it as follows:

$$\begin{aligned} \text{Information in fossil record} &= \text{Information in living system} \\ &+ \text{Generator noise} \\ &+ \text{Noise during transmission} \\ &+ \text{Receiver noise} \end{aligned}$$

Each of the three kinds of noise adds some irrelevant information to the message produced by the living system (Beerbower 1968).

Let us suppose that the living system is the total set of activities involved in butchering an animal. The tools and bones left on the ground, and the contexts in which they are found, are the information received. During “message generation,” noise is added by random error, and by both deliberate and unconscious alteration of the abandoned items. A tool unrelated to the butchering process may be accidentally lost in the butchering area. Because he intends to use them in another operation, prehistoric man removes some of the butchering tools to another location. As men move around, artifacts are unconsciously kicked out of the places where they were originally deposited.

As the occupation area is abandoned, transmission noise begins to affect the message in the ground. The ravages of time take their toll, as perishable materials gradually disappear and imperishables are broken down to smaller sizes by the passage of men and animals and the weight of overlying sediments. Gravity, frost, and slopewash move the materials downslope and realign them, sorting them by size at

the same time. If the earth dries and cracks, or ice wedges form, foreign material may drop into the cracks and be incorporated into the site sediments. Large parts of the site may be completely eroded away, and thus lost forever. Animal disturbance also affects site sediments: animals may burrow into the deposits, removing site materials and adding material from other horizons. The role of earthworms in reworking sediments has been discussed by so eminent an authority as Charles Darwin (1896). In the process, worms may completely remove intervening sediments from between two layers of occupation debris, making them appear to be a single level. If a site continues to be occupied by man, material gets well scuffed about, and later items are trodden into earlier levels.

Last, a considerable amount of noise is added during reception of the message by the living prehistorian. Some of the interference comes from legitimate error: what the excavator believes to be a representative sample of information about the lifeways of a prehistoric group may in fact reflect just one or a few specialized activities. The solution of a particular problem crucial to an understanding of the prehistoric past may require that special effort be devoted to collecting data relevant to that problem, to the relative neglect of some other kinds of information. It is also true that none of us really knows what *all* the potential sources of information in a prehistoric occupation may be, or how to go about collecting that information, and so we all fail to gather much material which may prove to be of critical importance in the future. A good deal of the interference, however, comes from real blunders on the part of the prehistorian. There are a great many so-called prehistorians who should never be allowed near a site. Even the best scholars occasionally become careless in collecting or analyzing data, and thus contribute misinformation to the decoding process. And, try as we may, we all inevitably misinterpret a part of the information we do collect, simply due to the fact that science progresses, and the interpretations which seem most likely now, will certainly be altered somewhat by future insights. By the time the transmitted information has been altered by these factors, the original nature of the butchering-camp may be very hard to recognize.

The fact that so many potential sources of error can be enumerated should not be discouraging—that we can recognize them indicates that we shall eventually be able to deal with them. As time goes on, we are continually learning how to evaluate, predict, and control these causes of interpretive error. For the time being, we must recognize that the most trustworthy evidence about prehistoric lifeways can be gathered by the excavation of largely undisturbed, single relatively short-term deposits of occupation debris. The amount of such material available is still infinitesimal when we consider the vast areas of the earth and the immense periods of time which have been witness to hominid evolution. When Washburn published his 1959 (Washburn 1965) paper on this theme he qualified his conclusions as speculative because of inadequate data. Even though pertinent evidence has been accumulating at a heartening rate during the last 15 years, the observations in this chapter cannot be more definite or conclusive than those Washburn made. There is still so little material, and what we have is so unlikely to represent the whole fairly, that

every small addition of empirical data can be expected to change these speculations radically.

■ THE “CULTURAL CAPACITY” OF NON-HUMAN PRIMATES

The general anthropological literature in English abounds with mistaken caricatures of the uniqueness of the human condition. Among those crucial potentiating capabilities claimed as unique to the hominid family, whose absence would preclude the development of fully effective cultural adaptations, are toolmaking, symbolic behavior, and consciousness of self-identity. However, modern laboratory and field studies of non-human primates show that these faculties are not exclusively restricted to hominids. Van Lawick-Goodall's observations of the toolmaking behavior of wild chimpanzees demonstrated that those pongids not only manipulate suitable found objects as tools but also regularly modify naturally occurring raw materials to increase their suitability for the tasks at hand. Her descriptions of the production and use of “termite-sticks” are too well-known to require further comment (Goodall 1965). Premack (1971) has reported what seems to me convincing evidence of a well-developed “symbolic capacity” in one great ape. He succeeded in teaching a chimpanzee arbitrary values for a set of plastic shapes which the chimp then used, often in completely original combinations, in apparently “intelligent” communication with the experimenter. Only some symbols were used as object-names; others were given quite abstract linguistic content. By manipulating symbols already learned by the experimental animal, Premack was able to teach her entirely new linguistic concepts. There is bound to be some reticence to accept his results, but I have not seen any compelling disproof of them. The criticism that the animal did not, herself, invent the values with which the plastic forms were endowed is no contradiction of his conclusions, since most human symbolic behavior is learned in the same way, not invented by each individual. Gallup (1970) showed that chimps soon learned to distinguish their own mirror reflections from other individuals.

Isolated anesthetized animals, marked with indelible color in parts of their anatomy not directly visible to them, showed recognition that marking had occurred after observing their reflected images.

These experiments and others show how much of what we have naively considered to be part of an exclusively human behavioral domain is shared with other animals, especially our closer primate relatives. The gulf between the behavior of *Homo sapiens* and that of the pongids is obviously immense, but it is equally clearly a quantitative rather than a qualitative one.

It is true that what I have called a fully effective cultural adaptation is restricted at present to members of the species *Homo sapiens*. However, the fact that modern man has developed such an effective adaptation does not imply that the cultural systems of his earlier ancestors were as efficient adaptive mechanisms. In fact, an examination of the material residues of their behavior quite certainly shows that not to have been the case.

EARLY LITHIC ARTIFACT ASSEMBLAGES

The earliest convincing evidence of implement manufacture on a more elaborate scale than that practiced by chimpanzees is the occurrence of stone artifacts in deposits from the Omo Valley in Ethiopia and the area just east of Lake Rudolf in Kenya. The occurrences in question are stratigraphically latest Pliocene or basal Pleistocene in age and have been dated at between two and three million years (lower part of the Shungura Formation, Omo), and around 2.61 ± 0.26 million years (Koobi Fora tuff A at KBS). One occupation site about 2 million years old is known from primary depositional context in the Omo. Site FtJi2 is a scatter of lithic artifacts on what was a temporary land surface in a back swamp or marginal flood basin. An exposure of 10 square meters has yielded 95 small vein-quartz lumps, pebbles, and flakes, some of which are utilized, but none are intensively retouched (Merrick et al. 1973). The amount of information stored in these pieces is minimal (not more than about two bits). At Koobi Fora, artifacts (in fresh condition) and bone occurred in the base of an aeolian tuff, or at the interface between this and lower fluvial deposits. Some vertical scatter was noted, and it is not yet clear whether the accumulations represent one or several phases of hominid occupation. There are a number of spatially discrete artifact-rich occurrences in comparable stratigraphic situations at Koobi Fora: only two (FxJj1, FxJj3) are known in some detail. At the time of occupation, the sites were apparently located along ephemeral water courses in a generally swampy flood plain. The excavated artifact series from FxJj1, exposed over some 45 square meters, totaled 122 pieces (excluding manuports) after the 1971 field season. Of these, 7 pieces were chopper/cores and 115 were flakes; most pieces are made of lava, but there are a few quartz and chert artifacts. Associated bone includes *Hippopotamus* and pig tusk, antelope teeth, and other ungulate remains. Some of the antelopes were very old at time of death. At FxJj3, excavation of some 20 square meters of intact strata produced 112 artifacts: one is quartz (a flake), the rest are lava. There are four chopper/cores in the series, 107 whole or broken flakes, and a hammerstone. Preliminary analysis of the fauna identified some fragments of *Hippopotamus*. The patchy artifact scatter at FxJj1 seems to be restricted to an area about 8 meters in diameter. At FxJj3, areal limits are not known. It is possible that both occurrences represent single, ephemeral, but relatively intense occupations rather than repetitive visits or long-term occupations of lower intensity. The excavator assigns the recovered lithics to the Oldowan industrial complex (Isaac et al. 1971; M. Leakey 1970). A third somewhat earlier occurrence has not yet been described.

At Olduvai Gorge, a series of very early occupation floors has been excavated and results of that work have been extensively published by M. D. Leakey (1971, 1975). The tuff-sandstone sequence Hay designates the Upper Member of Bed I produced assemblages assigned to the unevolved Oldowan complex. It is estimated that this member may have taken 50,000 to 100,000 years to accumulate. The sequence has been dated to around 1.70–1.75 million years (K-Ar) (Hay 1971). In addition to several vertically diffuse scatters of artifacts and fauna, there are five real “occupation floors” atop old land surfaces, with minimal vertical spread. The diffuse scatters

are generally embedded in claystones representing old mudflats into which materials sank or were trampled during the course of hominid visits (of unknown frequency or duration). The mudflats were apparently not exposed long enough for complete stabilization and adequate intensive weathering to produce paleosols like those underlying the true occupation floors. The latter occur on land surfaces, along the marshy eastern margin of a former saline lake, the southeast shallows of which were periodically freshened by streams flowing from nearby volcanic highlands. One typical Oldowan assemblage was found in a comparable situation in lower middle Bed II at the MNK locality. The horizon has not been dated but is certainly younger than the only securely dated level in Bed II (Tuff II^A, whose age is thought to be about 1.7 million years. The range in dates for the Upper Member of Bed I and Tuff II^A in Bed II correlates well with the calculated age of Middle Villafranchian faunal-bearing deposits in the Auvergne. Such a correlation is certainly not discordant with the composition of faunas from the Olduvai horizons in question (Hay 1971; M. Leakey 1971, 1975).

Leaving aside the vertically diffuse occurrences, occupation sites with unevolved Oldowan assemblages are so rare, and their contents and spatial patterning are so variable, that few regularities in site typology can be defined. Spatial segregation of activities is indicated by the presence in DKIA of a 4.25×3.65 meter circle of loosely piled stones, enclosing an area within which occupation debris was relatively rare compared to the situation outside the ring. At FLK Main, on the "Zinjanthropus" floor, small bone fragments, light-duty tools, and small debitage were largely concentrated in a 6.4×4.6 meter area, bounded on the south and east by a relatively artifact-free zone some 2.4–2.7 meters wide. Beyond the clear zone, there is a dense scatter of heavy-duty artifacts and manuports, as well as most of the larger bone fragments. The significance of these undoubtedly patterned distributions is still elusive, however. The faunal content of the Olduvai occupation floors is quite variable. In some levels (DKI level 3, "Zinj" Floor) the bone remains which are likely to be residues of hominid meals are largely from bovids, suids, and such larger mammals. At FLKNN (level 3), a number of individual broken-up tortoiseshells were found on an occupation floor, with bits of several kinds of small animals and some large mammal remains. FLKNI (level 6) is probably a butchering site; it yielded much of the skeleton of a young but very large elephant. The bones of this creature have been moved about, but the distribution suggests that the animal may have been mired or died and been butchered on the spot. Possible hominid coprolites from one level in Bed I contained bits of lizards, rodents, insectivores, and birds (M. Leakey 1971). It looks as though the site occupants had very catholic tastes; they may have relied on smaller mammals, reptiles, birds, and amphibians for much of their diet, utilizing whatever windfalls they were fortunate enough to obtain in the way of carrion or scavenged fresh carnivore kills, and perhaps occasionally killing a large mammal themselves. There is no evidence for deliberate selection among the available sources of animal food on the part of the authors of unevolved Oldowan assemblages.

All the occupation floors but one (FLKNNI, with only 17 artifacts) produced far more *debitage* than shaped or utilized tools: unretouched flakes and core and flake

fragments make up from 70 to 90 percent or more of the total assemblage. All the sites produced spheroids, subspheroids, and/or unaltered manuports; sometimes these were abundant in comparison with the remaining artifact categories. In my opinion, such pieces are the only possible candidates for identification as weapons in any Oldowan lithic assemblage. Conceivably the abundance of manuports and spheroids on some occupation floors is a result of intentional stockpiling of missiles near favored hunting localities, or for defense at “living sites.” On the other hand, stockpiling involves rather complex behavior patterns, and it is equally possible that hominid occupations were simply sited at or very near localized sources of raw material. Stone used for artifact manufacture—lavas, quartz, quartzite, and chert, primarily—was available locally in outcrops or as stream-transported cobbles.

Generally speaking, deliberately retouched flake tools are rare and neither the overall form of the artifact nor the shape of the retouched edge seems to have been standardized. Although Mary Leakey has, for convenience in description, recognized several subgroups in the large tool series, most of these seem to me to be more the product of the classifier’s sense of order than the result of attempts at standardization on the part of the implement-makers. The spheroid–subspheroid–core–discoid–chopper–heavy-duty scraper categories in particular seem to be segments of a spectrum of more or less continuous variability. If the suggestion that Oldowan implement makers had not imposed extensive standardization on their lithic products proves true, I am equally convinced that the lack of standardization does not reflect any structural incapacity of the toolmaker’s hands for precise manipulation. Among the pieces from the lowest occupation horizon in Bed I there are some small artifacts with regular, diminutive flake removals which I am sure could only have been produced by a hand capable of a well-developed “precision grip.”

Artifacts from different Oldowan occupations are sometimes distinct in appearance. Size differences, as well as differences in proportional representation of major artifact groups or raw materials, are documented. The inter-assemblage variations noted can plausibly be ascribed partly to chance, partly to the ready availability of different stone sources in the close vicinity of the different occupations, and partly to the uses to which the tools were put. I find no convincing evidence for intentional stylistic variation between the artifact assemblages from different localities in either the published descriptions and figures or the available cast series. There seems to be no evidence that the toolmakers at any locality were on the average producing a superior product to that made by hominids from any other locality. In itself, that is suggestive. The fact that any variation in the ways different groups made Oldowan lithic assemblages is obscured by the general lack of standardization of the artifact series may show that selection pressures to improve the learned repertoire of artifact-making skills were not especially intense. This should mean that the artifact-making behavioral repertoire was not as crucial to group survival as is usually suggested.

Sites in the Omo, at East Rudolf, and at Olduvai provide evidence that at least two distinct hominid lineages coexisted in East Africa during the period when Oldowan assemblages were made. Which one or more species may be the authors of the artifact assemblages is unknown, and cannot be determined on the basis of

the evidence now available. The data at hand certainly do not preclude the possibility that all of them may have made stone tools. Some time ago Mayr proposed that, once culture as a means of adaptation appeared, there would be only one major niche open to a man-like creature, so that no more than one hominid species could exist at any given time (Mayr 1950). While this may be true (for a given region) once culture assumes a major role in adaptation, the Oldowan assemblages really do not in themselves provide convincing evidence that this was yet the case. It is more reasonable at present to suggest that, although the Oldowan complex provides the first recognizable, unequivocal evidence for the beginnings of culture as a hominid means of adaptation, this evidence only shows the application of implement-manufacturing techniques not much more sophisticated than those observed among chimpanzees to durable raw materials. As vehicles of information, the unevolved Oldowan lithics are extremely primitive. The very earliest stone implements may convey no more than two or three bits of information. The lack of information stems from a corresponding lack of system (regularity, pattern) in the artifact forms produced and the techniques used in their production. Until artifact attributes form consistent nonrandom patterns, noise blankets any meaningful information they may contain. The small degree of patterning represented in even later Oldowan artifact series may reflect limited behavioral control due to restrictions on the mental capacity of the toolmakers.

Due to considerations of preservation, the appearance of the first stone tools in the fossil record probably seems far more revolutionary to the prehistorian than it actually was. The fact that the first major expansion of hominids out of the African continent did not occur until perhaps two million years after the first stone tools are recorded supports the suggestion that the adaptive advantage they conveyed was quite minor.

At Olduvai Gorge, Oldowan occupations from Beds I and II exhibit obvious continuities in industrial characteristics: Developed Oldowan assemblages from Bed II are apparently somewhat more patterned versions of their early Oldowan predecessors in Bed I. Small proportions (about 6 percent of tools, on the average) of true handaxes, with no evident typological precursors in earlier horizons, are found in several Developed Oldowan (B) levels. Aside from this small increment of new types, the assemblages in question (BK II, TK II, SHK, FC West, MNK main) are homologous with other Developed Oldowan occurrences.

Classifying Developed Oldowan artifacts is only somewhat easier than is the case for early Oldowan pieces. Deliberate retouch is more regularly represented and often continuous enough to permit the recognition of a few major flake tool categories (scrapers, perforators, notches, burins, *outils ecaillés*). Multiple working edges are more common. However, neither the overall form of the flake employed nor that of the working edge is consistently classifiable into a manageable number of distinctive and regular groups. Variability is still the rule, and the large-tool series is no less variable than the flake tools.

There is no evident difference between the kinds of hominid activities attested for the Developed Oldowan levels and those suggested by the Early Oldowan sites. Relatively undisturbed Developed Oldowan occupation-residue scatters seem to be

quite restricted in size: on the order, say, of 20–30 square meters in area (although erosion has removed part of each). At the FC West living floor, bovinds, equids, crocodiles, and hippos are the most frequent forms in the rare faunal series, with suids, tortoise, and elephant also noted. There is still no convincing evidence from any single site that the larger animals were deliberately hunted, but taken all together, the sites in Bed I and II indicate the regular exploitation of animals in a restricted region, and this is itself suggestive. The occurrence of large numbers of individual animals in some levels seems to hint at either deliberate or accidental animal drives. A “whole herd” of small antelopes (*Phenacotragus recki*) was found in one level at SHK (apparently not associated with tools). Natural phenomena, other than predator activity, which would account for such finds are hard to imagine. Even if hominids did not kill large game regularly, they at least scavenged it regularly, and of course, taking the smaller animals is well within the capacity of other higher primates. The ratio of artifacts to bone is higher in Bed II occupations than it was in Bed I, partly reflecting the more regular utilization of larger animals.

Proportional difference in artifact content between Developed Oldowan assemblages is sometimes quite marked, and the assemblages are more varied in content. Choppers no longer absolutely dominate the assemblages as they did in some Bed I occupations. These observations suggest that a process of artifact diversification and functional specialization was well begun. Some occupations have relatively large proportions of types represented rarely or not at all in other sites in Bed II. Probably the occupations themselves are beginning to be functionally specialized. Such systematic differences add at least one or two bits of additional information to the record.

One site in Bed II (EF/HR) has over 50 percent true bifaces, with few spheroids/subspheroids and no battered nodules or blocks (the latter two categories are relatively abundant in other Developed Oldowan occupations). The bifaces are extremely variable and hard to classify consistently. They are larger on the average than bifaces from other Bed II living floors. Mary Leakey has assigned the EF/HR assemblage to the Early Acheulean, claiming that the Acheulean and Oldowan represent “two distinct cultural traditions, perhaps made by two different groups of hominids” (M. Leakey 1971). It is clear that the distinction recognized is based in the last analysis on the large proportional representation of bifaces in the EF/HR collection more than on their morphology or other characteristics of the assemblage. In fact, in other respects the assemblage is quite similar to that from Developed Oldowan occupations. I rather believe that this kind of difference is more likely to reflect special artifact function and the specialized nature of tasks undertaken at EF/HR than a stylistic difference setting one hominid social group or evolutionary phylum off from another. As I see it, the Early Acheulean and Oldowan assemblages from Olduvai Gorge Beds I and II are as typologically similar as one can expect for a scanty sample from a single variable but completely continuous, evolving spectrum of lithic industrial development. The criteria which have been proposed at different times as indicative of the distinctive nature of the Acheulean are the presence of large proportions of bifacial tools, or the production of large flakes and their use in the fabrication of bifaces (M. Leakey 1971, 1975). But there do seem to be Developed

Oldowan bifaces on large flakes, which weakens the second criterion, and by the first criterion most well-excavated Acheulean sites from later time ranges would not themselves qualify as Acheulean.

Tools are known from two Australopithecine-bearing cave breccias in South Africa: Sterkfontein and Swartkrans (Mason 1962). Although there is no question about their association with the hominid deposits, excavation techniques have been crude at best—due to the indurated nature of the sediments (breccias). Mary Leakey calls both artifact series Developed Oldowan B (M. Leakey 1971); both contain crude and irregular bifaces in otherwise Oldowan-like contexts. The sites are interesting for two reasons. At Sterkfontein, the only hominid represented is the gracile *Australopithecus africanus*. Those who maintain that this Australopithecine was not a toolmaker have not convincingly explained this occurrence. At Swartkrans, remains of the robust Australopithecine were found in a pink breccia with bones of another hominid—*Homo erectus* (“*Telanthropus*”). Three cobbles from this site are said to be fire-spalled. If so, they are the earliest evidence for fire in a hominid site. In both cases, the presence of tools proves the recovered debris is in part, at least, the residue of hominid occupation, not just the remains of carnivore meals. Other, probably later “Developed Oldowan” assemblages are reported from the Melka Kontouré region (Ethiopia), Ubeidiya (Israel), and Aïn Hanech (Algeria). Derived chopper/chopping tools are known in abundance from earlier Pleistocene geological deposits in Atlantic Morocco.

Assemblages with numerous bifaces are occasionally represented. At Peninj, two very early “Acheulean” horizons described in preliminary fashion by G. Isaac produced numerous, extremely “unpatterned” cleavers; the Peninj beds contained a mandible of the robust Australopithecine. The assemblages may be broadly contemporary with the Bed II Early Acheulean at Olduvai. Acheulean industrial development is well documented in Beds III and IV at Olduvai but as yet the occurrences are not fully reported and all may be disturbed. *Homo erectus* seems to be the author of some or most of these later assemblages.

■ RADIATION TO EURASIA

From the first appearance of convincing worked stones to the first spread of hominids out of the African continent a vast period of time intervened—at least 2 to 2.25 million years. The process of populating the usable African landmass seems to have been painfully slow, and our early hominid relatives must have found themselves precariously near the brink of extinction at many times. That they possessed an adaptive edge is clear from their eventual success, but the edge must have been infinitesimally small for hundreds of millennia.

Aside from one occurrence, which may still be problematical, there is no convincing evidence for population of Europe prior to the mid-Pleistocene. The exception is the Vallonnet cave on the coast of southeastern France. There, two choppers, three trimmed pebbles, two utilized or retouched flakes, and some *debitage* were found in deposits containing a Late Villafranchian fauna (Howell 1966; De Lumley,

Gagnière, Barral, and Pascal 1963). If the artifacts are not intrusive, they demonstrate an early and tenuous invasion of the European continent, an invasion which may have been ephemeral. Early man (*Homo erectus*) apparently first reached Southeast Asia late in the "Lower Pleistocene" (Poetjang beds, eastern Java). Nothing is known of his cultural inventory, nor are we better informed about the tools of his mid-Pleistocene Javanese descendants. From the paleoenvironmental evidence at hand, it seems that all these early sites except the Javanese occurrences were found in open country: stream banks, dry streambeds, lakeshores, and sea beaches seem to have been preferred localities. Probably such situations would have made game more visible to the hunters, and, where present, fresh water and succulent vegetation in the vicinity would have served to bring men and animals together. Even in the Javanese cases, where tropical forest predominated in the surrounding region, the hominid sites were in open micro-environmental settings. (It is even conceivable that regions of contemporary volcanic activity were especially favorable for the establishment of early hominid populations in the densely forested tropics.)

If evidence for a lower Pleistocene hominid radiation out of Africa is sporadic and tentative at best, Eurasian human occupation residues become quite common during and after the Elster glaciation. There is increasing evidence that the spread of hominids into Europe was a multipronged affair. Unlikely as it may seem, one route of population spread crossed the Strait of Gibraltar from North Africa, probably moving northward along the Portuguese littoral to colonize the Iberian interior via the major east-west valley systems by late Elster times. The only other route may have proceeded around the Mediterranean coast, but from the evidence in hand there may have been a second Mediterranean crossing from the Eastern Maghreb to the Sicilian and Italian coasts, followed by a further spread up the Italian boot to the Riviera and France on the one hand and, perhaps, Central Europe on the other. The Pyrenees apparently constituted a major barrier to communication between France and Spain, since industrial development seems to have proceeded independently in each area for a very long time. On the other hand, the Mediterranean must have been a much less formidable barrier than we have suspected, since industrial complexes from Iberia and the Maghreb continued to be typologically so similar as to indicate continuous intraregional information exchange from Elster through Hengelo (Freeman 1975). For migration of most terrestrial organisms, the Strait of Gibraltar acts as a "sweepstakes route," across which spread is highly improbable, although it does occasionally occur (Simpson 1962). However, from Elster on, the strait served as a readily traveled corridor for human movement (which probably proceeded in both directions after the first crossings). Strangely, there is no acceptable evidence for pre-Eem colonization of European Russia (Klein 1966). The arrival of early man in China, probably contemporary with Elster in Europe, is seemingly a continuation of the population radiation which established *Homo erectus* in Java during the lower Pleistocene.

The establishment in force of hominids on the European landmass is astonishingly sudden. Admittedly, our temporal discrimination for mid-Pleistocene events is very coarse, and occurrences separated by more than 10,000 years often appear

synchronous to us. However, compared with the snail-like pace of hominid expansion throughout Africa, the mid-Pleistocene radiation still must represent an exponential increase in rates of population growth. Man suddenly became tremendously successful.

We do not yet really know exactly what factors conferred the new adaptive advantage on early man. There is good evidence that all the colonists knew how to control fire, but there are strong suggestions that fire may have been utilized earlier. The migrants are all relatively large-brained, but new finds in East Africa suggest that cranial capacity among much earlier hominid groups may have been highly variable, and that at least the upper end of the range of variability overlapped with the later average (R. Leakey 1973). So far as we can tell, the toolkits of the earliest Europeans are not one whit more sophisticated than those of considerably earlier human groups in Africa. None of these factors seems an adequate explanation. In any case, the new adaptive advantage was almost certainly not conferred by any single sudden discovery or development, but a concatenation of increased capability in several domains. The kinds of change which would confer such an advantage without leaving direct durable traces in the tools men used or the shape of their bodies are basically of two sorts: more efficient organization of activities and increased efficiency in communication. The two are really sides of the same coin. An increased appreciation of the regularities of nature, better appreciation of the characteristics of the range, improved ability to predict when and where resources would be available, better scheduling of the exploitative round, sustained cooperation in the food quest, the avoidance of duplication of effort, and the capacity to respond to differential seasonal or local availability of resources by temporary segmentation or reaggregation of the social group are all factors which would be enhanced by (and some are absolutely dependent on) an ability to communicate complex information in unequivocal fashion which transcends any nonlinguistic signaling system not itself derived from articulate language. In my opinion, it was not the invention of new technological devices, but rather the ability to use available devices in innovative, better organized, and more efficient ways, which provided the essential advantage that ensured man's spread. It seems likely that culture became a fully effective means of adaptation only in mid-Pleistocene times.

The effectiveness of culture as a major adaptive mechanism is mirrored in a further factor. Prior to the mid-Pleistocene, differing adaptations are reflected more in the variety of hominid body morphology than in cultural diversity. From the mid-Pleistocene on, there is no really convincing evidence that different hominid species were ever again sympatric. Hominid body form continued to respond adaptively, but the brunt of the process of articulating man and nature was thenceforth borne by culture.

■ CULTURAL CHANGE THROUGH EARLY WÜRM

Several mid-Pleistocene artifact series from Europe and Asia have recently been rather loosely referred to as a sort of attenuated Developed Oldowan. The collections in

question are sufficiently idiosyncratic to render that usage inadvisable. Probably one should use local terms (Buda industry, Choukoutienian) to describe them.

The Elster (Biharian) occupation at Vértesszöllös is sited atop travertine-calcareous mud deposits of the ancient Atalér floodplain (Kretzoi and Vérteš 1965). The occupation layer is itself overlain by travertines and loess, the latter containing mammalian microfaunas indicative of full glacial conditions. During occupation, climate was more temperate and the regional setting was one of relatively extensive forest cover. Remains of larger mammals (primitive bovids, cervids, horses, rhinos, bear, beaver, and wolf) are abundant and there are localized accumulations of charred bone. Fragmentary remains of *Homo erectus* are associated. The more than 2,500 lithic artifacts include diminutive choppers and chopping tools, notches, perforators, scrapers, utilized and retouched flakes, *debitage*, and cores. While there is still a marked degree of fuzziness at the limits of each apparent type, there seem to be regular modes of attribute association—more regularity than in the Olduvai Bed II Developed Oldowan. Several pieces have multiple working edges. Some very tiny tools show carefully controlled retouch. No detailed faunal study or plan of the spatial distribution of recovered materials is yet available.

Choukoutien Locality 13 and the Basal Gravel at Locality I provided three stone artifacts in sediments apparently deposited under cold climatic conditions, which may be contemporary with late Elster. The main hominid-bearing levels at Choukoutien probably accumulated under interglacial conditions (Holstein equivalent?), to judge from recent palynological and faunal studies (Hsu 1966; Kahlke 1968). The large artifact series from the *Homo erectus* layers is made primarily of vein-quartz and sandstone, which undoubtedly contributes to the crude and unpatterned appearance of the industry. Choppers and chopping tools, scrapers, perforators, burins, bolas, hammers, and battered cobbles are represented (Chia 1964). Several bones bear conclusive evidence of deliberate human alteration (Breuil 1939). Evidence for fire is abundant. Excavation techniques during the earlier exploitation of this overwhelming (over 40 meters depth of deposits) site were totally inadequate to permit meaningful socio-cultural interpretation (Black et al. 1933). Lithic artifacts are also known from what seem to be somewhat earlier deposits (but perhaps still Holstein?) in the Lantian area. They are much similar to the Choukoutien pieces. Although they have not generally been recognized as such, there are rare bifacial implements (mostly partial bifaces) in the Lantian mid-Pleistocene collections (Dai 1966; Dai and Chi 1964). Information on these occurrences is still sketchy. The Asiatic cases offer, in my opinion, the only possible potential examples of continued isolated cultural development from an industrial base antedating the invention of bifaces, but I would not be at all surprised if that proves not to have been the case.

Much fuller information comes from recent work at three early Acheulean sites in Europe: Terra Amata on the French Riviera and Torralba and Ambrona in north-central Spain. Human utilization of the Spanish sites took place during an Elster cold phase. Torralba and Ambrona were located on the edges of a well-watered, marshy valley dissecting the vast waterless uplands which divide the Ebro/Tajo drainages. The local availability of water and succulent vegetation attracted large game in

considerable numbers. There are several levels of occupation at both sites, and the evidence from all is consistent; Torralba and Ambrona were butchering sites where animals were killed, and meat was processed prior to removal to as yet undiscovered living sites. Only large game was regularly taken (horses, cattle, rhinos, elephants, deer, reindeer, some carnivores). Hunting practices may be characterized, however, as opportunistic: no animal large enough to spot over the grass- and sedge-covered valley bottom was neglected. The evidence that different individuals from several species were killed, disarticulated, and butchered all at once suggests cooperation in periodic game drives and organized sharing of the product by cooperating social groups. Animals were driven into marshy situations where escape was difficult, and then dispatched and disarticulated on the spot. Certain favorable points near especially mucky spots or ponded water were chosen beforehand, and stockpiles of throwing stones, and probably some finished tools and raw material, were deliberately accumulated near these natural traps. Game was repeatedly driven to the same preselected kill sites. (Fire-drives may have been seasonally practicable.) The mired animals were stoned, burnt, or speared, and disjointed where they fell. Each of the several participating social units received a portion of every animal killed. The social units sat apart from one another to finish the preliminary processing of their booty, and then carried off the choice product to their living areas. Only the undesirable residues of carcasses were left behind. Tools were left where they were used, probably because the prehistoric hunters intended to return and reuse them in the not-too-distant future. As many as seven social units, each of them probably composed of several (5–7?) individuals, evidently shared in tasks performed in some Torralba levels, and the amounts of meat carried away must have been formidable (up to 9,000 kilos) to judge from the number of animals represented and the proportions of their skeletons which are missing (*all* the meatier body parts were carried away). Cooperation in the extensive game drives attested and carrying away anything like this quantity of meat would have required substantial numbers of able-bodied participants. The larger social group from which the hunters came may have numbered over a hundred individuals, and although such large population aggregates might have been feasible only from time to time, all could have used a single encampment during the periodic hunts.

The tools used by Torralba hunters were no more sophisticated than those known from Africa at a comparable period. Using multivariate statistics, the analysts have been able to discern a number of activity-specific toolkits, each of which was used to perform a restricted set of operations related to the butchering process. Differentiation of artifact function is very marked—there are few truly general-purpose tools (by the way, these do *not* include bifaces, which at Torralba were used to batter open the robustly buttressed skulls of elephants and wild oxen). Heavy-duty flensing and boning were done with one set of equipment, fine slicing with another, the butchering of large skulls with yet another, and so on. Each of the attested activities was performed in a different area.

Most of the recovered implements are flake tools, and there is considerable typological variety in the collection. There are quite numerous multiple-edged arti-

facts, but there is no significant tendency for given different types to be combined. The overall shape of the flake tools is not tightly standardized, but the shape and size of retouched working edges is much more so. Bifacial tools are quite rare (they are entirely absent from one Torralba Acheulean level). There is no evident “stylistic” difference between the artifacts produced by Torralba hunters and those produced by the Ambrona groups. In fact, both collections are virtually indistinguishable from North African Acheulean artifact series of comparable age. Worked wood (including a slim spearpoint) and bone are also represented in the Torralba artifact series. Evidence for the use of fire is abundant, but no true hearths were found. The sediments occasionally contain small bits of ochre and one large fragment from Ambrona seems to have been worked (Freeman 1975, 1978; Freeman and Butzer 1966).

The sophistication of scheduling and organization of hunting and meat-processing activities evidenced by the regularities in the Torralba/Ambrona data are such that it is inconceivable that they could have been sustained without language. While no structural remnants were represented at Torralba or Ambrona, there is good evidence for the construction of at least temporary shelters in several levels at Terra Amata—stone walls, postholes, and stone rings that may have wedged the bases of other posts form oval patterns that may outline huts. True hearths occur. The site is probably a temporary (warm-season?) base camp, on a bay near the mouth of a small river. The artifact inventory contains numerous choppers and chopping tools, but is clearly Acheulean, with partial bifaces and picks also represented (De Lumley 1966). Strangely, there is no evidence for particular attention to strictly coastal resources at Terra Amata or any other Early or Middle Acheulean site, although the littoral was clearly utilized.

The evidence from these early sites prefigures developments throughout the long period from Elster through early Würm. All the well-excavated sites are interpretable as variations on a single adaptive leitmotif, in my opinion. Throughout this lengthy period, man managed quite well as an opportunistic hunter/gatherer. Apparently his techniques and organization were highly successful, but little experimentation with tried-and-true methods seems to have been tolerated. A close examination of large numbers of single artifact types from one occupation shows each to have encompassed substantial variability—a simple convex sidescraper, for example, may be longer or shorter, steeper or flatter, more or less convex, made on a lateral flake margin or a wide extremity, yet it is still demonstrably the same. If tools of the same type, but from sites thousands of miles from the first, are added to the collection, variability does not increase in any systematic way. The raw material used in the new group may exert its influence, but otherwise we could probably lose the second group in the first. There are interregional boundaries, like the Pyrenees, across which notable differences in artifact inventory can be discerned, but the size of the areas in which artifact series are homologous at any given time is remarkable. The fact that artifact types from a single site are internally quite variable, while the ranges of variation within a given type from different sites in a wide region overlap, largely indicates that there is little or no deliberate stylistic information imposed on the stone tools. Nor is there the sort of unconscious stylistic load that is often incorporated in

the products of distinct modern identity-conscious socio-cultural groups. While this may simply be accidental, since it is not easy to alter lithic artifacts without affecting their functions, it may reflect an adaptive reality. Conceivably, intergroup boundaries were not as purposefully maintained, signaled, and defended as they are among most modern societies. The distinction of “we” from “they” may have been adaptively dysfunctional as long as human groups were small and resources abundant. Appropriate mates would be hard for an adult to find in a coresident group whose maximum numbers were only on the order of 100 people (or less) of all ages. Open group boundaries might have lessened the tensions of contact between social units, facilitating intergroup movements of personnel; the occasions for such movement might have been regular and formalized (periodic aggregations of large numbers in a restricted area) or unpredictable and informal, occurring at chance encounters. In either case, permeability of group boundaries might have been quite advantageous to survival. Idiosyncratic attributes which may characterize the artifacts from more restricted regions begin to appear only in the Middle Paleolithic but even then they are almost certainly not the result of deliberate stylistic differentiation.

Formal differentiation and functional specialization of artifact types proceed gradually throughout the Lower and Middle Paleolithic period. The absolute number of different tool types the analyst may recognize varies directly with the total number of shaped tools recovered (probably this partly reflects the fact that the number of discrete tasks performed by prehistoric men at a locality varied directly with the intensity of occupation; partly it reflects sampling effects). As a general rule, the total number of different types in Oldowan and earlier Developed Oldowan assemblages varied as the square root of total shaped tools recovered. For earlier Acheulean assemblages, the number of distinct types is about 1.5 times the square root of total shaped tools, and by the end of the Acheulean, the figure is on the order of two and a half times the square root, an average figure which, incidentally, is only very rarely exceeded during the rest of the Paleolithic.

Formal differentiation of artifact types seems to have occurred as the result of gradual, cumulative, and probably almost imperceptible incremental changes due to cultural drift. There were no obviously revolutionary inventions or drastic innovations—the course of overall change seems smooth. Retouch, which covers both surfaces of bifacial implements, becomes more regular, finer, and obviously better controlled. All the basic elements needed to maximize the potential of lithic artifacts as information channels are present by the late Acheulean except the regular use of pressure flaking and the punch-driven blade technique. Masterful implements were being made on very small blanks, where necessary to economize raw material. On the other hand, where raw material was abundant in large sizes, a prefiguration of modern mass production techniques, Levallois flaking, had appeared. Levallois technique ensures the serial production of several analogous implements from a single specially prepared core with a minimum number of flaking operations. Conservative of energy, the technique can be relatively wasteful of raw material.

Bone and wooden artifacts are preserved with some frequency despite their perishable nature. At Kalambo falls, wooden clubs and possible digging sticks were

recovered from Acheulean contexts. Some Mousterian occupations (Morín and el Pendo, in Cantabrian Spain) show that the techniques acquired for stoneknapping could be successfully applied to bone to produce a wide range of handsome flaked implements (González Echegaray, Freeman et al. 1971, 1973). The first tentative experiments in engraving bone occur in Acheulean and Mousterian contexts.

Until Early Würm the evolution of artifact morphology reflects continued design improvements which make working edges and whole tools more efficient for use in a small number of primary operations. Adaptations reflected in the artifact inventory may be called “technique-oriented.” Slicing tools become more efficient as slicers, crushing tools become better suited for crushing; each implement type becomes better adapted to a specific kind of manipulation. The primary operations in question may be performed on a variety of materials: skins, vegetable fibers, wood, and meat can all be sliced with the same cutting edge. There is no evidence that any tool was specially tailored to work on one material alone. Naturally, if artifact design is not immediately related to the specific resources manipulated, artifact series will provide little reflection of major environmental difference. At a butchering site, a cleaver might be used to batter open an elephant skull; the same cleaver might later be used to chop down a tree, to split open large bones, etc. Because the specific nature of the manipulated raw material is irrelevant to cleaver design, cleavers in cold temperate environments could be formally identical to cleavers from hot savanna or tropical river valleys. This observation is probably the key to understanding why widely separated Lower and Middle Paleolithic artifact assemblages may look so similar.

As time goes on, the differentiation of activities within single occupations becomes increasingly easier to discern. Different tasks are done in different places, and with some frequency activity-specific areas are set off from each other structurally. Dry walls divide cave interiors into two or more compartments. Convincing dwelling remnants are preserved at several Late Acheulean and Mousterian sites. Hearths, pits, postholes, and mounds are features known from many later (especially Mousterian) sites. Some small pits seem to be food-storage facilities. The differentiation of areas within single occupations is paralleled by a clearer functional differentiation of distinct occupations. It seems possible that differences between Clactonian and Acheulean sites or between some Mousterian facies are activity related rather than stylistic. Specialized quarry/workshop sites, hunting/butchering camps, and base camp/living sites are well defined even in the Lower Paleolithic.

Towards the end of the period, there is evidence that some sites in favorable localities were occupied year-round. That is true, for example, for some Mousterian sites in France and northern Spain. In the Spanish case, where resources were probably available all year round, this was accomplished by locating the base camp at a central point with respect to the distribution of exploited resources, but it would not have been feasible without long-term storage in areas characterized by marked seasonal scarcity (Freeman 1973). Opportunistic exploitative strategies were universally the rule; occasionally, hunters managed to trap and kill large numbers of animals of a single species, probably by accident. But in no case is regular reliance on a narrow range of productive resources attested.

We know that some Acheulean and many Mousterian groups made deliberate use of coloring material—worn-down “crayons” of mineral color are frequent finds. However, we do not know what was being decorated. Aside from a few enigmatic engraved doodles on bone, no Lower or Middle Paleolithic artistic productions survive.

For thousands of millennia, the hominid dead were apparently ignored or disposed of with other garbage. The first evidence for the separation of the bodies of deceased men from those of animals and from food debris appears in Middle Paleolithic contexts. The treatment of Neandertal dead at sites like Teshik-Tash, Shanidar, and La Ferrassie involves ceremonial complexities which are already highly elaborated—bodies are interred in specially prepared graves capped in some cases by visible mounds, and mortuary offerings, which may include food, flowers, and/or the tools essential for daily survival, are included with the remains. Most authorities agree that the authors of these reverential ritual practices are morphologically members of our own species, although there are still a few who advocate specific distinctions between Neandertals and modern men. Certainly the complexities of behavior evinced by this evidence hint at belief systems so elaborate as to fall within the range for fully modern men.

While cultural change is a slow and gradual process, the rate of replacement of given artifact types and industrial complexes seems to have accelerated geometrically. The Oldowan may have lasted 2 million years, the earlier Acheulean a million or so, the Middle and Late Acheulean perhaps three to five hundred thousand together, and the Middle Paleolithic industries less than a hundred thousand. While one generally gets the impression that Lower and Middle Paleolithic industrial evolution was nearly stagnant, that is only because continued acceleration at essentially similar rates resulted in strikingly rapid industrial succession during the Upper Paleolithic, so that earlier developments seem slow by comparison.

■ ACCELERATED CHANGE IN THE LATEST UPPER PLEISTOCENE

A great deal of importance has been attached to the advent of anatomically modern man and its supposed correlate, the appearance of blade and burin industries. In fact neither of these factors had as revolutionary effects as is usually supposed. As a result of continued formal and functional differentiation of lithic artifact series, prefigurations of the early Upper Paleolithic blade/burin industries had sporadically occurred in North Africa and Southwest Asia long before Hengelo, but without any devastating and long-lasting result. However, the tempo of industrial replacement had become many times faster than it was in the Lower and Middle Paleolithic, and this contributes a misleadingly revolutionary allure to the Upper Paleolithic.

In fact, earlier Upper Paleolithic adaptations were not noticeably different from those documented for the Middle Paleolithic, and opportunistic exploitative strategies continued to be the rule. While treatment of raw material and flaking techniques in earliest Upper Paleolithic (Chatelperronian) contexts in Europe dif-

fer appreciably from those common earlier, there are nonetheless marked continuities between Mousterian and Chatelperronian artifact types (González Echegaray, Freeman et al. 1971, 1973). Similar continuities are noted for the transition from “Middle” to “Upper” Paleolithic complexes in Southwest Asia. However, with time some important developments do occur which distinguish Upper Paleolithic adaptations.

Even during the Middle Paleolithic, suitable living areas were gradually being filled by human populations, although density remained well below the carrying capacity of the area. By the Upper Paleolithic, man had learned to exploit the northern steppe and perhaps the tropical forest as well. As populations increased, the exploitation of locally available resources intensified and diversified. Shellfish, present only occasionally in Middle Paleolithic sites, began to be utilized more extensively. More small animals were taken. Some creatures had been neglected by some communities during the Middle Paleolithic, seemingly because their exploitation was dangerous or too costly to warrant the necessary expenditure of time and effort. Upper Paleolithic peoples found it necessary or desirable to collect those creatures. Nocturnal burrow-dwelling fur-bearers are regularly represented in Upper Paleolithic occupations for the first time. Their presence almost certainly shows that their hunters were employing self-acting devices (traps, snares) to take them. The first evidence for the construction of pitfalls, the pitfield at Les Trappes in the Dordogne, is probably Upper Paleolithic in age. Perhaps certain small fur-bearers were hunted exclusively for their pelts. All lines of evidence indicate an increasing awareness of the potential regionally available resources and the development of means for their acquisition (Freeman 1973). To be effective, many of the new devices had to be designed specifically for use on one particular resource (as is the case for certain traps, fishhooks, weapon points, nets, and so on). Upper Paleolithic industries are characterized by a shift from “technique-oriented” adaptations to “regional- and resource-oriented” adaptations. As an inevitable consequence, the tools used in one small region are often quite distinct from those used in another. The process of interregional differentiation proceeded quite rapidly, so that by about 18,000 BC, Solutrean weapon points from one small river valley can easily be distinguished from the points made just a few kilometers away. Probably some of the differences noted are conscious stylistic devices that set off the product of one identity-conscious socio-cultural group from that of another.

Conclusive evidence for the presence of multicomponent composite tools is also first documented during the Upper Paleolithic. Some of the new tools are sophisticated devices conferring considerable mechanical advantage. True arrowheads may exist in North African Aterian collections, and they are certainly present in the Levantine Solutrean (Parpalló, Ambrosio). Composite foreshafts for Magdalenian weapons have been preserved, still in connection. Spearthrowers characterize Magdalenian collections. Microliths designed for hafting as points and edges of composite darts or spears are also documented. The information carried by these new channels is often infinitely multiplied by nonfunctional decoration of their surfaces. The common presence of bone implements in Aurignacian, Solutrean, and

Magdalenian artifact inventories provided an abundant and relatively easily decorated medium for artistic expression. Crude baked-clay figurines are known from Eastern Europe. Some of the engravings on Upper Paleolithic bone implements seem to be a sort of notation, but certainly not all the tally-marked bones are lunar calendars, as has been suggested.

Artistic representations are commonplace parts of the European Upper Paleolithic inventory (Ucko and Rosenfeld 1967), and masterful engravings, paintings, and sculptures are known from portable objects or the walls of the caves sometimes used by prehistoric men. These representations give us a vivid glimpse of the animals men hunted and sometimes of the men themselves. The potential of these representations for stylistic analysis has not yet been realized. Their study could indicate the spatial position of social group boundaries in the prehistoric past, and tell us a good deal about socialization practices. Their quality probably also indicates the presence of at least part-time craft specialists.

Standardization of artifacts reached a peak during Upper Paleolithic times. From some Solutrean levels we have laurel- and willow-leaf points that are almost exact duplicates in size and shape. The same is true for some Magdalenian bone harpoons. Unprecedented control was being exerted over morphological and metrical attributes of these implements.

An impressive variety of storage facilities is known from Upper Paleolithic occupations. These vary in shape as well as size, and some pits show evidence of heat treatment to harden their walls against rodent and insect penetration. Structures are also variable. Large elongate surface buildings, small square and round ones, pavements, rings of stones or bones marking former tent emplacements, and semi-subterranean structures are known. There was apparently much variability in the internal appointments of these dwellings and in the management of internal space, characteristics which should correlate to some degree with the size and organization of the group of occupants. While some sites were probably occupied by very small social units, other large sites (especially caves) may have sheltered great numbers at least on a periodic basis. That seems to be the case for some of the larger decorated "sanctuaries," which probably were used as major ceremonial centers serving all the people from an extensive region. These differences in site size and arrangements are certainly correlated with major differences in social organization during the latest Pleistocene.

There are some suggestions of status differentiation within Upper Paleolithic societies, especially in the differential treatment of the dead. However, in every case the burial sample is very small, and these suggestions may be misleading. A few graves do include great numbers of beads or other personal adornment. Possibly some Solutrean laurel leaf points, apparently too delicate to have been used as tools, may have served as badges of rank, but, once more, there is no conclusive evidence on the subject.

Networks of long-distance trade are attested by the occurrence of goods transported over great distances in some Upper Paleolithic levels. Perforated Mediterranean shells are found in Atlantic Europe; alien raw material has been used to make

tools in sites hundreds of miles away from the source. Data at hand are still insufficient to permit real understanding of the nature of these networks.

Strangely, the total number of artifact types recovered from any given Upper Paleolithic occupation is seldom greater than that from Middle Paleolithic occurrences, given collections of comparable size. The average number of types remains about 2.5 times the square root of total retouched tools. But the Upper Paleolithic as a whole seems characterized by far greater diversity, because of the great rapidity of industrial turnover. The European Upper Paleolithic in its entirety only spans about 25,000 years, and the whole temporal duration of the Magdalenian in all its manifestations is only about 6,000 years.

By the end of the Upper Paleolithic, industries are so well patterned and the behavior reflected in the occupation residues is so apparently understandable that the analyst must constantly fight a tendency to regard the authors of the industries as in every way like himself. They seem so similar to us that it is dangerously tempting to believe that they thought about themselves and the universe in our terms, and that our experiences must be identical. Of course, they are nothing of the sort, and that sort of reasoning will not lead to valid conclusions about the prehistoric past. Nevertheless, the conclusion is inescapable that their behavior was as intricate and sophisticated as our own, and that they were completely modern in all important senses.

The single most important step in cultural development during the Upper Paleolithic occurred sometime between 18,000 and 14,000 BC. It was a major shift in exploitative strategies which came as a logical culmination of earlier adaptations. Instead of reliance on a diversity of subsistence resources, the new strategy entailed intimate and intensive concentration on a very few especially productive wild resources (Freeman 1973). Those resources were regularly cropped, intensively enough so that they would maintain a high rate of increase but not so intensively as to exhaust them. In Cantabrian Spain, this shift to "wild-harvesting" came with the Lower Magdalenian. The resources chosen were red deer, winkles, limpets, and snails. Many red deer were harvested at once from the local populations, probably by massive game drives, taking advantage of the fact that local deep cover during the height of the glacial cold was broken up into small isolated stands. Some Lower Magdalenian sites contain no mammal bone but red deer, and in tremendous quantities (50+ individuals) at that. In France, reindeer and horses may have been harvested in this way. Shellfish may have been the mainstay of diet, seasonally. Shifting site location reflects the new exploitative strategies, with some sites located on the coasts for easy access to limpets and winkles, and others at the edge of the uplands, close to alpine mammals, in regions where *Helix* could be collected in abundance. Probably there was a seasonal alternation between the highland (summer?) and coastal (winter?) sites. Exploitation of the limpet (*Patella vulgata*) soon eliminated the older and larger individuals entirely from the natural populations: Magdalenian sites no longer yield the large specimens common in earlier levels. Below a certain minimum size, limpets do not yield enough meat to make their collection by normal methods rewarding or even feasible. Yet in Azilian and Asturian levels, many individual specimens below this minimal size are represented (de la Vega del Sella 1923). The

conclusion seems inescapable that wild-harvesting had been carried one step further by the immediately post-Pleistocene hunter-gatherers. As far as I am aware, the only practical way to collect such small specimens in any quantity is to cultivate them deliberately. Masses of seaweed, nets, or frames of stakes and branches can be located in the supralittoral and intertidal zones frequented by the molluscs; their spawn, collected on an artificial substrate, can be stripped from it periodically and the cycle started anew. The suggestion that any preagricultural group may have known how to cultivate a natural resource seems daring, but, after all, the suggestion requires behavior little more sophisticated than that involved in repeated wild-harvesting of any kind, and the evidence that wild resources were periodically harvested by at least some late Upper Paleolithic communities is absolutely incontrovertible.

SUMMARY

While stone tools in and of themselves offer but a pale reflection of prehistoric hominid capacity, an examination of all the many categories of evidence preserved in well excavated, intact occupation floors affords a more meaningful approach to the understanding of prehistoric lifeways. Few suitable well-studied occurrences are available, but an examination of the meager evidence in hand forces a reevaluation of tenets we have tended to regard as having almost the force of revelation.

The difference between man and his primate relatives is certainly a quantitative rather than a qualitative one. Man possesses no mystical attributes, “symbolic capacity” or whatever else they may be called, which create any great gulf between him and “non-humans.” The road to man’s present state was long, rough, and steep and required much effort in the traveling.

The invention of stone tools marked no drastic revolution in the hominid condition. Our forebears were little more than clever apes; the difference between their abilities and those of modern chimpanzees was relatively minor. Tools alone did not make man as we now know him; he is the result of a multifaceted adaptive process. The hominid adaptive commitment involved an ability to learn to manufacture extensions of the body, their production and utilization in organized social contexts, and communication, and it is impossible to separate these factors. Several ways of being “human” were tried at first, and those that failed probably did so because of an inability to communicate or to coexist effectively, or an inability to articulate the processes, not because people could not make effective implements. One of the important observations that can be made from the study of both the past and the present is that there has never been any direct, one-to-one, correlation between hominid physical type and “culture.” Extinct hominid species evidently made and used artifacts of the same kinds as those produced by their adaptively successful contemporaries.

At one time, the prehistoric record as we understood it was full of apparent “revolutions”—the Urban Revolution, the Food-Producing Revolution, and the Blade-and-Burin Revolution are familiar examples. With closer scrutiny and more information each of these revolutions has tended to evaporate into thin air. And that is understandable. There are no revolutions in prehistory; there is only adapta-

tion—the continuous and gradual readjustment of organisms to constantly changing ecosystems.

Man's cultural beginnings were small and feeble, but they provided the means of attaining an immense adaptive advantage. The transmission of learned behavior is much more rapid than the transmission of genetic material. Information acquired as the result of a new experience can be passed on immediately to other members of society. Through the continuing socialization process, man's evolution has become Lamarckian, in a sense. In culture, man "inherits" acquired characteristics. As a result, fully effective cultural systems allow great flexibility and rapidity of response to changed circumstances. But, for a long time this potential was not recognized. While socialization processes were still relatively difficult, communication still rather rudimentary, and noise very high, it was a safer strategy to concentrate on conveying a limited amount of easily understood information which would be generally applicable regardless of circumstances. As long as hominids were scarce, the opportunity for contact with individuals whose experience was widely different from one's own must also have been extremely limited, and short lifetimes would tend to remove from society those older individuals whose experience was greatest. As a result, the potential variety of information available through socialization in any given community was normally very small. The cultural means of adaptation nonetheless was advantageous.

Gradually, humanity spread and filled the most suitable land areas of Africa and Eurasia. Gradually, ability to communicate and to organize activities became more efficient. By the mid-Pleistocene, occupation complexities suggest advanced communication techniques and highly efficient organization for exploitation. As population grew and interpersonal and intergroup encounters became more frequent, the variety of available information increased. Culture change, so slow before, accelerated at a geometric rate. Each successive major industrial complex lasted only about half as long as its predecessor. Probably that exponential rate of acceleration remained constant until the invention of writing. The Upper Paleolithic marks no revolutionary break in the acceleration rate, just its natural continuation. By 30,000 years ago, the turnover of industrial complexes had become so rapid as to appear to mark a radical break with the past, but that is simply an illusion.

Judging from the abundant artistic representations and decorative motifs in Upper Paleolithic contexts, and from the presence of systems of notation, it is clear that the capacity of late Paleolithic man was as well developed as our own. And, in fact, by the end of the Pleistocene, man was already experimenting in controlling a variety of natural resources in ways which would have preadapted him for the development of agriculture and animal husbandry. All that remained was the discovery of those resources with the greatest potential for domestication and exploitation. The development of agriculture in the Near East and Asia must be considered as basically no more than happy local results of the application of techniques which were probably being tried with varying success over much of the habitable world. Paleolithic developments thus prefigure essentially all the basic elements on which were based the development of agriculture, urban life, and, indirectly, the modern industrial civilizations.

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Much of the content of this chapter was stimulated by discussions I have had over the past decade with K. W. Butzer, F. C. Howell, G. L. Isaac, and R. G. Klein, and I have naturally built on the excellent publications on this theme produced by several other eminent scholars (Beerbower 1968; Breuil and Lantier 1959; Leroi-Gourhan 1971; Oakley 1958, 1968; Singer et al. 1957; Washburn 1965). I am also deeply grateful to Chao-Mei Lien, for having called my attention to important recent work by Chinese scholars, and for having summarized the contents of the papers I myself cannot read, as well as for her contribution to many informal discussions about Chinese prehistory.

The original aspects of this chapter are speculative enough so that they may seem somewhat controversial, especially since I was not able to examine all the artifact collections I have discussed at first hand. Nonetheless, I am encouraged by a statement made by K. P. Oakley, in a paper (1968) on the same subject: "If one is embarking on a speculative hypothesis at all, one may as well be thorough going!" I thank the editors for having permitted me to do so.

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Paleolithic Polygons

VORONOI TESSERAE AND SETTLEMENT HIERARCHIES IN CANTABRIAN SPAIN

Sometimes, the application of an unusual analytical technique to a body of commonplace data produces information as interesting as it was unexpected. This chapter discusses suggestive patterns made by drawing Thiessen polygons (also called “Voronoi tesserae”) around Paleolithic sites in the autonomous political region of Cantabrian Spain, where prehistoric investigations have been especially intense over the last few decades. The simple geometric patterns resulting from this purely mathematical procedure suggest that sites used during each of four periods fall into previously unrecognized hierarchical arrangements, that generally agree with informed evaluations of the “importance” of their assemblages, but that have no straightforward explanation in the purely environmental terms that are the prehistorian’s conventional fallback.

Settlement studies are of the greatest interest to Paleolithic prehistorians and other archeologists. Yet despite the immense amount of data that have been gathered from Paleolithic sites during more than a century and a half of explorations, we can still not reconstruct the settlement systems corresponding to any Paleolithic complex anywhere. We have begun to recognize the characteristic signatures of some of the recurrent “tasks” undertaken during individual Paleolithic occupations of a site, but site classification has scarcely proceeded beyond the obvious distinction

between open-air and cave sites, the differentiation of quarry/workshop sites from “butchering” sites, and of both from a heterogeneous category of other sites that probably includes some “base camps” and others that are almost certainly functionally specialized for sets of activities whose signatures have not yet been determined. It is our fond hope that, by means of careful excavation (in fact, *only* by that means) we may eventually assemble the data needed to evaluate changing site functions, so that we may see how contemporary occupations fit into their proper position in a network of interrelationships, and identify the part each played in the larger settlement systems of the Paleolithic. But as yet, that is only a hope.

Our excavations already show us that some stratigraphic sequences are much longer and some occupation levels immensely richer in contents than others. We usually explain such differences in terms both vague and conjectural. The unverified postulate that all Old Stone Age societies must have been “simple and egalitarian,” with little specialization of statuses, has generally been extended to the sites as well, and the idea that (roughly) contemporary sites might actually occupy positions in a graded settlement hierarchy (a possibility commonly entertained by those who study the archeology of later and presumably more complex societies) is infrequently considered in literature about the Old Stone Age. In cultural studies, Thiessen polygons are part of the analytical battery of geographers and others who analyze relationships of centers to satellites, in settlement hierarchies. As such, their use would usually be considered out of place in Paleolithic prehistory. If others have applied this procedure to Paleolithic data (and I presume someone must have) I am ignorant of the fact.

The work that follows is a rough outline—a preliminary heuristic sketch for further exploration, rather than a finished study. It maps Voronoi polygons about sites from four major Paleolithic phases in Cantabria, Spain. The area considered is not a natural region but the autonomous political region of Cantabria. This arbitrary selection was made for convenience and can of course be challenged, since there are sites in both Asturias and the Basque provinces that would have added other polygons to the eastern and western periphery of the studied area. However, it is justifiable. The omitted sites are far enough from the peripheral Cantabrian sites that their addition would alter my results minimally.

I realize that there are other possible objections to my choice of area and sample, but I do not believe that they invalidate this research. The northern boundary of the mapped area falls in the sea off Cantabria’s coast. Since there are no known underwater sites, those on the immediate coast might be expected to be bounded by fewer neighbors than are ones further inland, but this theoretical objection is actually of little practical importance, since “coastal” sites prove to have relatively numerous neighbors, during at least some periods. The southern boundary of the mapped area coincides with the highest mountains in Cantabria. During the Paleolithic, human occupation was essentially absent above about 600 meters. Bounding the study area here seems eminently reasonable, since the high uplands, extending in a wide east-west band along Cantabria’s southern border, were evidently an important barrier to habitation throughout the Paleolithic. Of course,

there probably are as yet undetected sites within the land area included in the study. But undetected sites should be scattered more or less at random over the landscape; there is no reason to think that they would be concentrated in any particular area at the expense of others. Exploration of Cantabria has been relatively thorough and uniform. Sites have been sought assiduously by local amateurs, professional archeologists, and expert speleologists, so there is no reason to assume that any part of the study region has been less thoroughly surveyed than any other. It is true that most known sites are in caves. But Cantabrian bedrock is mostly limestone, and caves are ubiquitous.

Underrepresented sites are thus likely to be open-air sites buried deep below the surface. There has been a good deal of capital construction—roads, railroads, tunnels, and extensive building—and much quarrying. From all evidence to date, open-air sites must have been very rare compared to sites in caves. There is no reason to believe that any part of the region is disproportionately rich in buried open sites, and the very few of these that are known were probably mostly quite small and have been extensively disturbed. The near-absence of open-air sites in our sample is a fact no one can remedy at present; the only way to proceed is to work with what we do have.

The next step of my exercise was to determine how to divide the Paleolithic universe in Cantabria into manageable and meaningful units. Acheulean localities with any guarantee of integrity are too few to be interesting. The earliest phase of regional occupation that is both reasonably distinctive and has enough sites for useful comparison is the Mousterian, if facies differences are disregarded. All the Mousterian sites are in caves, except Unquera. Early Upper Paleolithic sites with Chatelperronian or Upper Perigordian tools are not common in Cantabria, but there are several with Aurignacian occupations: combining them into “Early Upper Paleolithic” sites produces a second unit. There are enough well-documented Solutrean and Magdalenian sites so that each complex could be considered separately, although it was not possible to subdivide either group further. I excluded from consideration all surface collections, all mixed and dubious sites—those where older collections have been lost or are not sufficiently diagnostic, and those recently tested sites that so far have produced inadequate samples for attribution—despite the fact that they appear on some published lists. I may possibly have excluded some sites that should have been included, but I don’t think I have omitted any important site or included any dubious case. Where two or more sites are so close together that their plotted positions would coincide at this scale (the four sites in the Castillo hill, or the two sites of Rascaño and la Bona, for example) only the largest or principal site was plotted.

Sites mapped for each of the four “periods” compared are listed in Table 5.1. For the Mousterian and the Earlier Upper Paleolithic, there are ten sites each. Sixteen Solutrean sites and twenty-five Magdalenian sites are identified. Several sites appear on more than one list—a few are on all. More detail on sites and occupation contents is available in the excellent summaries by González Morales and González Sainz (1986) and Straus (1992).

■ TABLE 5.1. *Site adjacencies in descending order*

<i>Mousterian</i>	<i>Early U. P.</i>	<i>Solutrean</i>	<i>Magdalenian</i>
<i>First Order</i>			
Castillo (6)	Castillo (6) Morín (6)	Castillo (7)	Castillo (7) Altamira (7)
<i>Second Order</i>			
Pendo (4) Morín (4) Cobalejos (4) Busta (4) Ruso (4)	Pendo (4)	Morín (6)	Pendo (6) Morín (6) Juyo (6) Camargo (6)
<i>Third Order</i>			
Cudón (3)	Cudón (3) Altamira (3) Salitre (3) Rascaño (3) Camargo (3) Otero (3)	Pendo (5) Altamira (5) Salitre (5) Carranceja (5)	Fuente (5) Pila (5) Salitre (5) Carranceja (5) Cobalejos (5) Otero (5) Cobrantes (5)
<i>Fourth Order</i>			
Hornos (2) Fuente (2)	Hornos (2)	Cobalejos (4) Camargo (4) Fuente (4) Bona (4) Haza/Mirón (4)	Rejo/Cabras (4) Cualventi (4) Busta (4) Rascaño (4) Truchiro (4)
<i>Fifth (and Lower) Order</i>			
Unquera (1)		Cualventi (3) Hornos (3) Ruso (3) Mirón (2) Chufin (2)	Chora (3) Hornos (3) Loreto (3) Cuco (3) Peñajorao (3) Valle (2) Hermida (1)

The approximate position of each site was determined by scaling in two dimensions (elevation was not included) from site maps with scales of about 800,000 to 1 (8 kilometers to the centimeter) published by González Morales and González Sainz (1986). Distances were scaled to the nearest millimeter (about 800 meters) only. Since my aims in this exercise were purely exploratory, I saw no need for greater precision at this point. There are practical problems in determining precise site location. Many sites are not located with any accuracy on existing topographic maps, and the approximate positions of latitude and longitude published for some sites may use either the Greenwich or the Madrid meridian without specifying; a few sites cannot now be located closer than a few tens of meters in any case, since they have been destroyed by quarrying. The results of this preliminary study indicate potential

enough to warrant a greater investment in accurate site location, and I intend soon to locate each site as precisely as possible on the ground, using a global positioning indicator. For the present, largely heuristic purpose, the scaled relative locations used here are adequate.

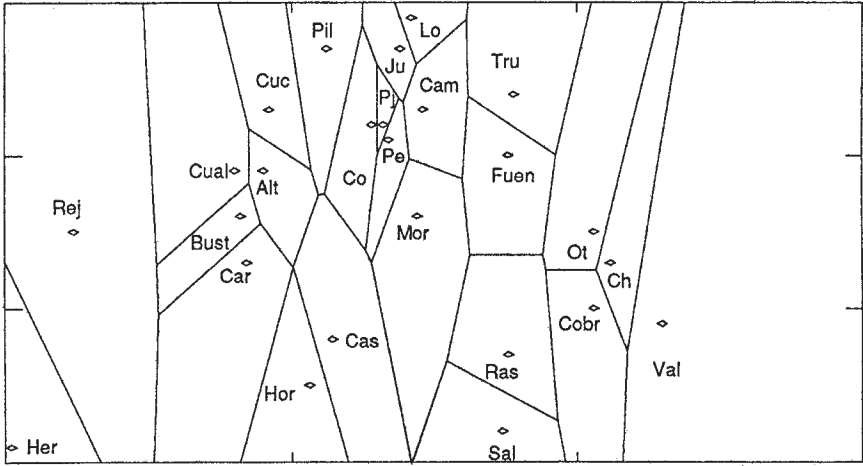
I did not include topographic detail on the plots I used. The sites are at relatively low elevations, and movement between them is not obstructed by intervening barriers due to the presence of high mountains, irregularities in coastline, or impassable bodies of water. Nor do streams seem to have been magnets for human occupation. This may be due to the fact that much drainage is subterranean. Some sites (e.g., Castillo) are located along rivers or permanent streams, but many are not (e.g., Morín, el Juyo, Altamira) and there is no evident tendency for settlement to follow the course of waterways at any period.

From plots of scaled relative positions, Voronoi tessellations (Thiessen diagrams) were generated for the set of sites for each period. In this procedure, polygons are drawn around each site so that any point within a site's surrounding polygon is closer to that site than to any other. Such boundaries have proved analytically useful in such fields as geography, ecology, psychology, and other social sciences, as well as in civilizational archeology. In archeological application, evaluations of distributions about "central places" have principally been employed in studies of the areas, or the numbers of minor settlements, that might have been linked to different political or economic centers in the past (see, for example, Haggett 1966: 115–52; Hodder and Orton 1976: 51–63; Renfrew and Level 1979; Orton 1980: 188–94).

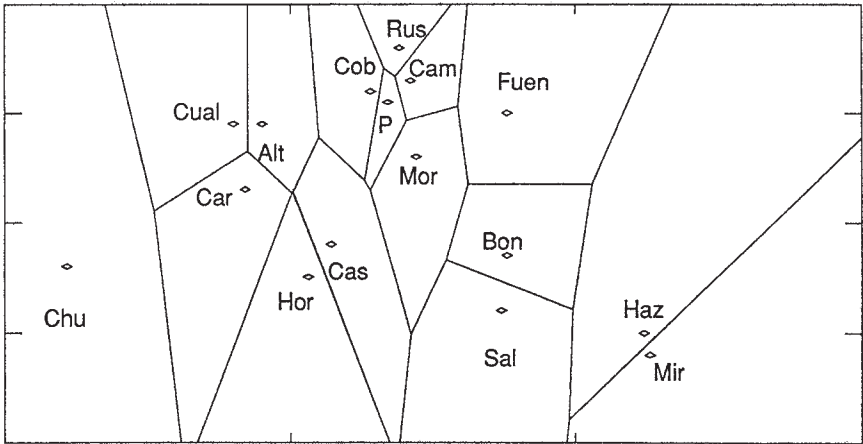
In the days before electronic computers were generally available, the corner points of linear boundaries could be determined by geometric construction or calculation, but the process became laborious if the number of centers was at all large, and plotting errors crept in. Nowadays, anyone with a good desktop computer and the right software can produce the diagrams with accuracy and ease. The SYGRAPH program incorporated in the statistical package SYSTAT has what is probably still the best Voronoi module, and was the program used here. In my opinion, a major defect of the program is that the total area included in a plot varies, as do the maximum two-dimensional coordinates of the sites it contains. It is so difficult to rescale the plots to compensate that I have not done so. Consequently, even though the maps are about the same size, a site that appears on more than one map will not occupy the same position on each, and distances between identical sites will seem to vary on different maps, as the scale of the area included on the maps differs. Since I am interested in relative positions only, these "defects" are irrelevant, however annoying.

Figures 5.1 through 5.4 show the resulting diagrams. While other aspects of the patterns might be analyzed, a few are especially interesting.

The first is the way in which polygon size varies in each of the four phases. In general, median polygon size decreases through time, as one might expect from the fact that site numbers in the study area generally increase from phase to phase. The exception is the change from smaller median polygon size for Mousterian sites compared to the larger Early Upper Paleolithic polygons—and in this case, site numbers are equal.

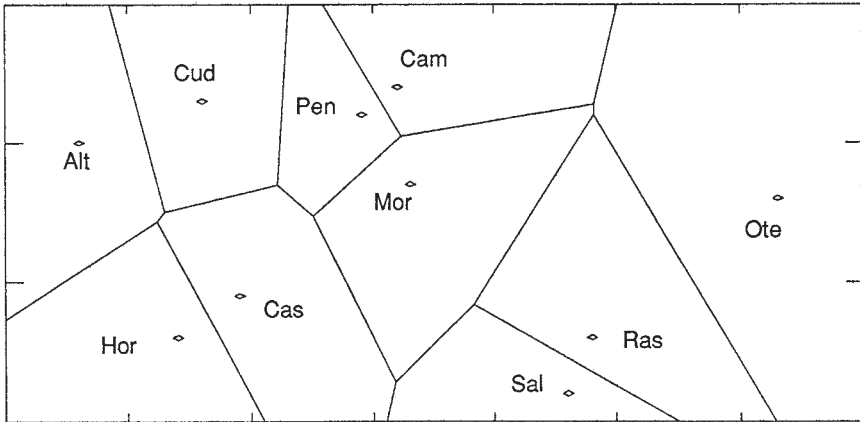


■ FIGURE 5.1. *Magdalenian*

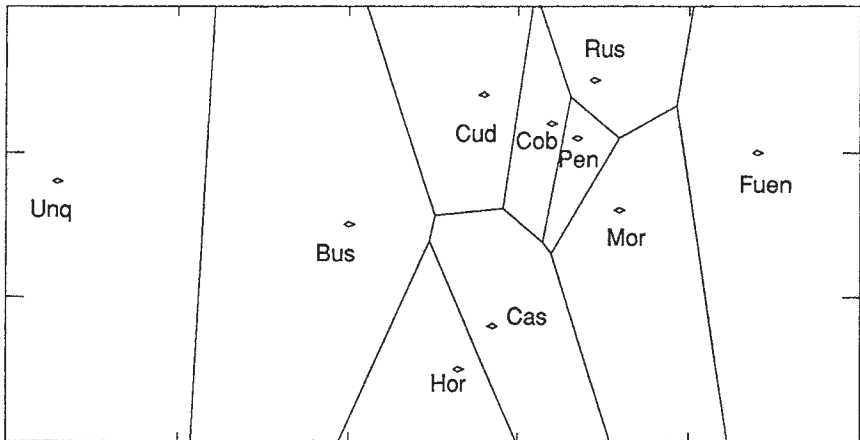


■ FIGURE 5.2. *Solutrean*

Increasing site densities are often assumed to correlate with increasing population density, but interpretation is actually more complicated. The phases do not represent equal time periods—the Mousterian plot covers a much longer temporal range than do any of the others, duration being shortest for the Solutrean, somewhat longer for the Magdalenian, and much longer still for the Early Upper Paleolithic. The possibility that seasonal or otherwise specialized sites were more abundant during some phases than during others is an additional complication; in fact, some Magdalenian levels at Rascaño and el Juyo are known to have been the loci of quite specialized extractive activities. The comparison thus has no straightforward implications for population studies.



■ FIGURE 5.3. *Early Upper Paleolithic*



■ FIGURE 5.4. *Mousterian*

All other things equal, one might suggest that polygon size may have some relationship to the size of exploited territories or “site catchment areas” about each site. But, especially for the earlier phases, there is simply no way to reconstruct the prehistoric landscape in sufficient detail to check this suggestion. If anything, there seems to be little or no relationship between the size of any given polygon and the probable abundance or variety of resources that were most likely available therein. The increase in median polygon size from Mousterian to Early Upper Paleolithic seems to mean that in the latter phase, sites were more regularly spaced over the exploited landscape; this interpretation must be qualified, however, since the occupations I excluded as dubious or mixed include some that had questionably been assigned to the Earlier Upper Paleolithic. It is nevertheless a fact that sites on the Solutrean and Magdalenian diagrams show a greater tendency to clump together than is true for

earlier phases. That might suggest an increasing tendency to locate all sites in especially rich areas. Rascaño and el Juyo suggest that more probably later sites, specialized in the extraction of a limited set of resources, were located in areas where those resources were at least seasonally especially abundant: sites for coastal exploitation near the richest rías or rocky shores, specialization on alpine mammals in upland sites. If that is correct, sites should have been becoming increasingly interdependent over the region, as settlement location became part of increasingly focused extractive strategies and subsistence systems that must have involved growing networks of intraregional (seasonal?) transport or exchange. But even if this scenario is correct, it will not explain the locations of many sites, nor the sizes of the polygons around them.

Other intriguing information comes, not from the size or location of the individual polygons, but the number of adjacent polygons each contacts. The number of neighboring sites whose areas directly contact the area about a central site is often called the “contact number” by Haggett (1965: 51) and other geographers. I prefer the term “adjacency” (from graph theory) to that of contact number. A site’s area is “1-adjacent” when it abuts only one other polygon, “2-adjacent” when it is bounded by just two others, and so on. Adjacency thus quantified can be treated as a set of integers that can be evaluated or combined mathematically: sums, means, and medians can be calculated from them as from any other integers. Adjacency differs from site to site within a period, and average adjacency varies from period to period. This provides a means of scaling sites and settlement systems: the sites from any phase may be arranged in a hierarchical order from greatest adjacency to least. The resulting order is surprisingly suggestive (Table 5.1). In fact, the ranked site list is one of the most interesting results of this essay.

Adjacency for ten Mousterian sites ranges from 1 to 6, with mean 3.4, median and mode each being 4.0. For ten Earlier Upper Paleolithic sites, adjacency ranges from 2 to 6, while the mean rises very slightly to 3.6, but median and mode drop to 3.0. The sixteen Solutrean sites range from 2 to 7, mean being 4.13, median and mode each being 4.0. For 25 Magdalenian sites, adjacency ranges from 1 to 7, and the mean is 4.4, median and mode each being 5. Fisher’s exact probability tests detect significant difference (at the 0.05 level) between adjacency patterns in the Mousterian, Early Upper Paleolithic, and Later Paleolithic (Solutrean + Magdalenian) phases, whether the distribution of sites is considered by order in the list, or by adjacency number. No significant difference appears between these values when the Solutrean and Magdalenian plots are compared.

The Early Upper Paleolithic pattern is like the Mousterian pattern in more ways than it is like the later Upper Paleolithic. Nevertheless, it is well individualized, and its difference from the Mousterian pattern is quite real. After the Early Upper Paleolithic, there is a significant jump in both maximum and average adjacency, with a further rise in the Magdalenian. Were one or even a few other sites added to the plots for any period, these global contrasts would probably be little changed.

A larger proportion of sites falls into first- and second-order ranks during the Mousterian than is the case in other phases, while there is a disproportionate con-

centration of third-order sites during the Earlier Upper Paleolithic. Interestingly, sites of first and second order are separated by an “adjacency gap” during both the Mousterian and Early Upper Paleolithic phases: first-order sites are 6-adjacent, while second-order sites are 4-adjacent, and there are no 5-adjacent sites. Despite that fact, during the Solutrean and Magdalenian, sites of any order always have adjacencies at least one degree higher than Mousterian or Early Upper Paleolithic sites of the same order. As we shall see, that is an important finding of this exercise.

What, if anything, might these mathematical patterns have to do with cultural adaptations?

Unless virtually all the sites of the period are now drowned offshore, the Acheulean occupation of Cantabria seems to have been at best ephemeral and discontinuous. Only during the Mousterian, and probably relatively late at that, do people seem to have established a firm foothold in the region. It is relevant that faunal evidence shows that Cantabrian Mousterian peoples made little use of either maritime or alpine resources, so not surprisingly, except for surface scatters of artifacts (some of which are usually but doubtfully attributed to open-air Acheulean occupations), sites were not located either very near the coasts or in the highlands.

One might imagine that pioneering settlement of the relatively unfamiliar Cantabrian lowlands proceeded with the spread of many more or less independent small settlements, maintaining only sporadic contact with a very few larger, more populous local centers. “Peripheral” sites on expanding frontiers have few neighbors. More adjacent “centers” might be the sites settled earliest, or those especially favored, either from the standpoint of availability of resources or ease of communication with other regions. The Castillo complex is unusual: it included two (perhaps three) closely neighboring Mousterian sites, Castillo and la Flecha, at about the same elevation on the sides of a single hill; otherwise, Mousterian sites do not “clump” closely together. These relatively elevated caves were ideal locations for game-spotting over an unusually large expanse of the broad Pas valley and adjacent lowlands. But Cantabria was (and is) an especially well-endowed natural region, and neither well-excavated assemblages nor the best paleoenvironmental reconstructions suggest that there was much variability in the kinds or quantity of resources easily accessible from the settlements. Even where controlled excavations provide evidence for the local performance of specialized activities (as at Morín), about the same range of resources was involved as is the case for the other, seemingly more “general-purpose” occupations. It seems likely that most sites were relatively self-sufficient, and engaged in about the same range of subsistence-related activities.

The Early Upper Paleolithic pattern seems from the archeological evidence a continuation of the Mousterian. Faunal assemblages suggest that a greater variety of resources were familiar and consistently exploited, but that in other respects, the approach to subsistence remained one of broad-spectrum, generalized, or opportunistic resource exploitation. Most sites continued to be relatively small, and the number of “occupants” was limited where there is evidence for such a calculation. The principal breaks with Mousterian patterns are the presence of two equally adjacent centers—contiguous to each other—and the drop in modal adjacency. Multiplication

of first-order centers may reflect an incipient regional differentiation, with shorter distances from low-order sites to the center in each region. Even though one of the two centers is much closer to the coast than the other, which is near the uplands, there is no indication of differential use of environmental potential—shellfish or alpine creatures are not especially abundant in either. The drop in median and modal adjacency, indicating more uniform site spacing through the utilized lowland zone, was perhaps coupled with a general equalization of the number of functions served by most sites. The picture is consonant with the interpretation that a majority of sites of the time were occupied by groups of about the same size, exploiting very similar sets of resources, and mostly doing so in similar ways and for similar reasons, without much functional differentiation between them.

The Castillo “clump” had dissolved, and only Castillo itself was utilized: perhaps central places had become less tolerant of very close neighbors than they formerly were. There is only one second-order site, suggesting a widened “gap” in functional diversity between centers and other sites. However, once more, the nature of site functions is not self-evidently only economic, or just subsistence-related.

The Solutrean phase lasted for a much shorter time than the earlier Upper Paleolithic. Despite this fact, Solutrean sites are over half again as abundant as they were earlier. Some (but not all) of this increase almost certainly reflects increased population density; on the other hand, some certainly reflects increased site specialization. Beginning in the Solutrean, there is a marked growth of the tendency for sites to occur in localized clumps. This may be due to the introduction of strategies of settlement location that preferred sites where some small set of productive resources was locally very abundant. The range of utilized resources had been broadened substantially, to include a greater representation of shellfish and fur-bearing carnivores. But instead of these being part of a continuing generalized, more or less opportunistic pattern of broad-spectrum exploitation, they augmented a pattern, best documented in Asturias by Straus, Clark, and other colleagues, that seems to have been shifting to the selective, concentrated exploitation of a limited number of particularly productive resources, such as herds of red deer.

As site numbers increase, the average area of site polygons inevitably decreases. During the later Upper Paleolithic (especially the Magdalenian), many polygons are quite small. While it is impossible to prove a relationship between polygon size, an artificial geometric construct, and the size of territories actually exploited from each site, it would be very strange if no such relationship existed. When polygon size decrease correlates with growing site specialization, we should find a corresponding general increase of site packing, especially about local centers, as ease of movement of goods or personnel between sites becomes more important. That is exactly what happens, from the Solutrean on.

During the later Upper Paleolithic, maximum adjacency rose to 7, and even in the Solutrean, sites that are only of fourth order are 4-adjacent—as well connected as second-order sites in earlier phases. During the Magdalenian, average adjacency increased still further and there was a real explosion of second-order (6-adjacent) sites. The growth in numbers of many-adjacent (5+) sites in the later Upper Paleolithic, and

the higher adjacency of lower-order sites, compared to the Early Upper Paleolithic and Mousterian, suggests that regular or sustained contact between sites of any order—not just peripheral sites and their centers—had become increasingly important to settlement strategies. At the same time, the decrease in average polygon size indirectly suggests that extractive efficiency had increased, either by the introduction of new technological means for production, processing, storage, and distribution or by improvements in the organization of social units responsible for these processes. In this case, both seem to be involved. Size standardization is evident in Solutrean leaf-shaped pieces, and new kinds of tools abound, including (in the Magdalenian) an abundance of cheaply made, interchangeable tool edges (backed bladelets and microliths). But more efficient organization, including greater functional specialization of occupations, was at least as important a part of the picture. We know from Altamira and Juyo that specialization of occupations had grown, on both economic and non-economic fronts. Alpine animals were then quite commonly hunted where they dwell, and shellfish collection produced true shell middens in some coastal sites. Concentrated exploitation of locally abundant and productive resources, such as limpets at Juyo and Altamira, red deer herds at Juyo, or ibex at Rascaño, had evolved to become, in a real sense, the periodic “harvesting” of renewable wild foods.

Magdalenian sites often had multiple alternative (sequential) functions: at el Juyo, red deer were harvested when they were abundant; then, perhaps as the deer herds replenished themselves, limpets and winkles were harvested on the coast; evidence from one occupation at el Juyo puts its “functional mode” (Freeman 1977) in the past cultural system well outside the range of ordinary economic activities. Some occupation functions probably had a seasonal component, while other specialized activities might have been undertaken on a periodic but non-seasonal basis, others were only quasi-periodic, and still others were highly irregular.

When site dispersal over a given landscape is uneven, rather than regular, as site numbers and density increase, it is mathematically inevitable that average and maximum adjacency must rise. What is not inevitable—in fact it is surprising—is the fact that at each period, the sites with greatest adjacency are the sites with the archeologically most productive (“richest”) contemporary occupations. Here, one sees most clearly the connection between our mathematical exercise and past cultural “fact.”

During the Mousterian phase, there is only one first-order site complex, whose adjacency is 6: the caves of Castillo. Mousterian levels at Castillo itself are the richest in all of Cantabria. Mousterian Level Beta at that site produced over 3,100 re-touched tools, and Level Alpha over 2,800. No other Mousterian occupation level has produced anything like such quantities of material. There were no 5-adjacent sites, but 50 percent of Mousterian sites were 4-adjacent, placing them in the second order. They include el Pendo, Morín, and all the caves with substantial or multilevel Mousterian occupations.

In the earlier Upper Paleolithic data set, Castillo is once more a first-order site, but it is joined by another 6-adjacent cave, Cueva Morín. With nine levels (one Chatelperronian, three Archaic Aurignacian, three evolved Aurignacian, and two

Perigordian), including structures and burials, Morín is arguably as rich and important an Early Upper Paleolithic site as any but Castillo, edging out even the long and impressive sequence at el Pendo, though the latter certainly comes close. And el Pendo does follow closely in adjacency and order.

Castillo is again the only first-order (now 7-adjacent) Solutrean site in the Cantabrian autonomous region. No other Solutrean site in the region—not even Altamira—comes near it in archeological importance. The fact that Altamira is placed in a rank lower than Morín (I would have guessed it would rank at least as high) is the only respect—the single case—in which my subjective estimate of site “importance” failed to agree with position of the site in the adjacency hierarchy. I suspect that the Voronoi diagram is at fault. The discovery of one or two new Solutrean sites to the south or southwest of Altamira would eliminate this disagreement.

During the Magdalenian, Castillo continues to be a top-ranked site, as one would expect from the size and richness of collections from the old excavations. Altamira has become its equal, and that is not surprising. Regardless of the small size of the collection from the early excavations that can be attributed with certainty to the Magdalenian, work in the 1980s shows that this deep level in the Altamira “Cocina” must have been as incredibly rich as it was areally extensive. New dates on engraved shoulder blades from Altamira previously considered to be Solutrean place them instead in the range of the Magdalenian, and indicate once again how severe the problem of confused stratigraphy and level mixture is for those materials found in the early 1900s. Despite its long, rich Magdalenian sequence, el Juyo is a small site with evidently limited, specialized functions, and its lower placement does not surprise me. La Pila is another case that might rank somewhat higher, but as yet there is too little published information from that interesting site to justify formulating any confident expectation.

In general, the agreement between ranked adjacency values from Voronoi polygon constructions and informed archeological assessments of site importance is truly impressive. What could possibly be the reasons for such substantial coincidence between a prehistorian’s evaluations of the relative archeological importance of Paleolithic occupation sites, on the one hand, and an abstract, purely mathematical construct that uses only site latitude and longitude to draw geometric figures about the sites, on the other? I have suggested above that economic behavior and the increasing functional specialization of sites through time are partial explanations of the Voronoi tessellations. But, however useful and interesting, they tell only one part of the story—that part having to do with average sizes, numbers, and changes through time. They cannot by themselves explain why a particular site occupies a particular position in the adjacency hierarchy.

One might suggest an explanation in strictly socio-economic terms: that as exploitation of the diverse resources of different habitats in an area became more efficient, there was an accompanying need to rigidify hierarchical principles of organization in order to ensure the redistribution of desirable goods that were not found uniformly throughout the region. As we have seen, in later phases of Cantabrian occupation, some sites had access to and extracted goods not available elsewhere—ibex

in the uplands, mollusks on the coasts—but the evidence suggests that this is only a partial explanation.

Caves suitable for occupation are abundantly represented throughout Cantabria, but few are high in the adjacency ranking or archeologically important—some apparently ideal sites were not used at all during the Paleolithic, and others have occupations only during one or a few Paleolithic phases. Of these lower-ranking sites, many are positioned where raw materials for tool manufacture and resources for subsistence were as easily accessible as they were at Morín or Altamira. It is possible, even probable, that the continual privileged position of the Castillo complex is partly due to the particular geographic position of its caves, on one of the best routes leading from the Cantabrian coast over a high pass (the Puerto del Escudo) to the Spanish Meseta. But neither geographic position, nor topography, nor favorable environmental setting, alone or in combination, is enough to account for the high adjacency of Morín, Altamira, or the other high-ranking sites. These cases seem to me to call for other explanations. It is quite possible that adjacency correlates more directly with importance: that a site is rich and intensively occupied simply because it is surrounded by many other sites. The converse may of course be true: important sites may be magnets that attract other settlements. In either case, access to, or ease for movement of, consumable goods may be less important than accessibility to services or other kinds of resources—people of special status (e.g., arbitrators, chiefs, curers or other ritual practitioners, prospective marriage partners), essential information (e.g., traditional lore, customary law, technical instruction/training in tool manufacture, fighting, or performance), ritual activities (e.g., collective initiations, world-renewal rites), or sacred places and ritual paraphernalia (e.g., shrines and their contents, ancestral homes, and perhaps even the painted caves themselves). There is of course no reason why economic exchanges, feasts, etc., could not accompany such transactions without being their central focus.

Some years ago, Margaret Conkey wrote a fundamental paper on stylistic elements in Magdalenian bone artifacts (Conkey 1980). On the basis of a comparison of the broad range of decorative elements on bone tools from Altamira with the more limited ranges found at other sites, Conkey suggested that the Magdalenian system of subsistence and settlement shifted between sites occupied by separated, small, and ordinarily independent groups, each with its proper, unlimited stylistic repertoire, and focal sites, with a range of bone decoration encompassing most motifs, where those small units periodically united into maximal social aggregates. These “aggregation sites” would have been the loci of a number of functions, including, perhaps, economic exchange, the performance of seasonal ceremonies, the rites of initiation, and so on. Despite the suggestive nature of her work, there has been little new evidence to evaluate her suggestions. The Voronoi tessellations are evidence that tends to reinforce her conclusions. If she is right, Castillo probably played a role comparable to that of Altamira during the Magdalenian.

As Barbara Bender (1981) pointed out, societies adapt not just to ensure population survival, but to ensure social reproduction. Increased productivity, she suggested, is correlated with social intensification. While there may be exceptions,

the Cantabrian record certainly seems to exemplify her conclusions. It indicates increasing productivity, culminating in the wild-harvesting adaptations of the later Upper Paleolithic. The Voronoi diagrams suggest the growing hierarchization of structures of alliance that should be as much cause as concomitant of economic intensification.

I cannot claim to have explained to my own complete satisfaction the coincidence between adjacency hierarchies calculated from the Voronoi polygons, on the one hand, and archeological evaluations of site importance, on the other. But it seems certain that such a coincidence does exist, and that other factors than the strictly economic ones that are our usual recourse may be required for its explanation.

Two potential practical applications of this exercise to fieldwork come immediately to mind. Archeological survey, surface collection, and limited stratigraphic testing in a small and largely unexplored region produce maps of sites with materials from different phases of occupation. Where survey is thorough, the construction of Voronoi tesserae from survey maps may give hints of the structure of land use and possible hierarchical relationships between sites even before any excavation is planned. The polygons could potentially help plan excavation strategies, indicating which sites might be of especial interest due to their central (or their peripheral) location. Alternatively, when Voronoi tesserae are plotted for a relatively well-explored region, such as Cantabria, and archeologically important sites have lower rank or adjacency than seems reasonable (Solutrean Altamira is a case in point), it may be advisable to search harder for sites in immediately adjacent areas.

Despite the fact that these are preliminary results, they suggest that the plotting of Voronoi polygons, and the construction of adjacency hierarchies for sites, are useful exercises even in Paleolithic studies, and may point the way to further investigations that will lead to a clearer understanding of the organization of prehistoric settlement systems.

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P A R T

III

THE LOWER PALEOLITHIC

The next two chapters discuss the evidence from the sister Acheulean sites of Torralba and Ambrona on the Spanish Meseta. The first and most extensive simply details the excavators' finds and the interpretations. Our conclusions were challenged, and one critic claimed that what we had recovered were simply the remains of scavenged animals and often unrelated stone tools. Some of these criticisms were dealt with in the first of these chapters. In it, I applied some "innovative" statistical techniques that I had adopted for the study of Lower and Middle Paleolithic materials (these techniques were already well-known to the practitioners of other disciplines and had been proven in those fields). The Torralba site is one of the richest and most important Acheulean sites in Europe, and it is lamentable that no monographic study of its excavation has yet appeared. Chapter 6 was an attempt to remedy this lack by providing a brief summary of our finds and their interpretation.

The difficulty of surviving as a scavenger during a cold climatic episode in the mid-Pleistocene of mid-latitude Europe is addressed in the second of these chapters. Strangely, there is still widespread reluctance to accept the idea that our earlier ancestors could have killed their food; the idea that through the mid-Pleistocene people were restricted to scavenging for their meat is still commonly held. That idea persists despite contrary opinions, such as those of the late authority S. W. Washburn,

George Schaller, I. Eibl-Eibesfeld, and many others who know about scavenging (and hunting) in Africa firsthand. After all, if other primates are known to be facultative hunters, why should that ability be denied to early people? A major argument in defense of this position is the relative scarcity of scavengeable meat in mid-Pleistocene mid-latitude Europe as compared to the East African situation at the same time.

Torralba and Ambrona

A REVIEW OF DISCOVERIES

INTRODUCTION

In the 1960s, F. Clark Howell began a program of multidisciplinary investigations at the Spanish Mesetan sites of Torralba and Ambrona that quickly became classic. Torralba and Ambrona retain among the best-preserved, most carefully excavated, and informative mid-Pleistocene localities known from Western Europe to the present day. It is my belief that in the future these excavations will be increasingly recognized as among Howell's foremost contributions.

This chapter reviews the work of the team that excavated and analyzed Acheulean residues and bones at Torralba and Ambrona under Howell's supervision and outlines the implications of the analysis of those residues.

The conclusions reached by Howell's team in the 1960s seemed interesting but unexceptionable at the time. Careful attention to microstratigraphy revealed several stratified levels of paleontological and archeological materials. Intimate spatial associations of tools and faunal materials and some otherwise seemingly inexplicable marks on the bones suggested that humans had visited the site to hunt or at least to butcher large game animals, although it was always recognized that some of the animals could have died natural deaths without human intervention and might have had nothing to do with hominid scavenging or butchering at all. We stated that

elephants were neither the exclusive nor the principal object of human attention: that other animals, especially horses, were as abundant or more so in some levels. We detected and recognized geologically caused rearrangements of residues, some due to faulting and more impressive ones due to freeze-thaw cycles in a harsh climate, and we were attentive to the possibility of winnowing and realignment due to flow in sheets and channels, though we could not detect edges of channels in any part of the Torralba Acheulean deposits. We carried out extensive analyses for paleoenvironmental reconstruction, including the contour-mapping of old temporary surfaces. We knew that carnivores had been present at least occasionally at both sites and had occasionally gnawed at a bone. Worked wood—cut and hacked, and sometimes charred—was recovered, as was charcoal in abundance, but nothing we could definitely identify as a hearth.

Statistical analyses indicated that the visible spatial associations we could see were part of larger patterns of consistent and repeated frequency relationships. In the 1970s, T. P. Volman showed that the frequency relationships detected when whole levels were compared had a spatial component in individual levels, that different sets of stone tools and body parts were consistently found in different parts of the ancient landscape: marshy waterlogged low-lying areas were the loci of death and discovery of carcasses, and the loci of preliminary disjuncting of body parts. Higher areas were the setting of intermediate stages of butchering and bone breaking. Still higher and drier were the few situations where final processing of carcasses took place, with some amount of stone flaking or tool repair (Freeman 1978).

Throughout the 1960s and 1970s there was little in the way of challenge or contradiction of those interpretations, although new studies of site formation processes and taphonomy suggested by the late 1970s that some revision of interpretations was necessary. Beginning in 1981, however, those conclusions were disputed, often with little or no justification and less regard for the facts. Certainly conclusions reached 30 years ago can stand a deal of revision in light of new information and criticism. But the conclusions Howell's team reached about Torralba—conclusions for which I take a major share of responsibility—were not simply evaluated and evenhandedly criticized; the interpretations were distorted by the critics to become unrecognizable caricatures, and the caricature then savaged. Now some say that results from the Torralba/Ambrona excavations, where "faunal assemblages are in disturbed context" (Villa 1991: 206), are unreliable or at least suspect. To advance science, those critics advocate dismissing our results, to rely instead on other sites, whose deposits are in fact no more intact, whose stratigraphy is no less complex, whose age is no less uncertain, whose samples are smaller, whose excavations were if anything less carefully controlled, and whose excavators have proposed interpretations no less "simplistic" or "anecdotal" and "unsystematic" (Villa 1991: 202, 204) than those we proffered.

The best answer to criticism comes from the sites themselves; were the data they provide better known, much of the debate about them would evaporate. A chapter of this length unhappily cannot do justice to excavations whose results require substantial monograph publication. The final monograph on Torralba, fin-

ished in the early 1980s, has been ready for press for some time, and at one time was even accepted for publication. Its appearance has paradoxically been delayed several years due to just such misconceptions about the site and its residues as a prompter publication might have dispelled.

Despite the deplorable impression that very little about the sites has seen print, part of the information to be reviewed here has long been available. For Torralba alone, there are more than a dozen largely nonrepetitive articles in English, based specifically on the analysis of recovered materials and distribution patterns from the site; together they total more than 300 pages. Though there are fewer sources about Ambrona, some quite extensive preliminary treatments of our work there have appeared (for example, Howell, Butzer, and Aguirre 1963; Howell and Freeman 1982; Howell, Freeman, Butzer, and Klein 1992). This chapter reviews aspects of the research conducted at Torralba and Ambrona during the 1960s and 1980s, in light of the most salient questions that have been raised.

The chapter is intended as a clarification of the record, not a debate with critics: truth is usually not well served by rhetoric. In the few passages where irritation mars the presentation, I beg the reader's indulgence with my loss of patience. I intend simply to state the facts about Howell's (and later, our) excavations as I understand them and to make it clear that for any understanding of mid-Pleistocene adaptations in mid-latitude Europe the data they offer must be taken into consideration.

Dismissing the results of research at Torralba and Ambrona is unwise—it would mean casting aside a great deal of important information about the nature of environmental change, site-formation processes, and hominid adaptations. Even the most vocal critic of our work cannot help admitting, in the midst of a slighting comment about my procedural inadequacies, that “the interaction between hominids and faunal remains seems clear. In fact, the results are not in conflict with the results that Freeman obtained” (Binford 1987: 95). Encouraged by so forceful an advocate, even an analyst as short-sighted as I cannot fail to be hopeful that a new overview will sharpen our vision of the significance of these Mesetan sites.

THE EXCAVATIONS

History of Research

On June 17, 1909, the Marqués de Cerralbo visited the hamlet of Torralba del Moral near Medinaceli in Soria, Spain. There, in 1888, trenches cut for the Madrid-Zaragoza railway had revealed bones of extinct Pleistocene mammals, including huge elephants. To his surprise, Cerralbo found Acheulean handaxes and other stone tools in association with these remains (Cerralbo 1909, 1973a, 1973b). This high-altitude site (1,113 meters above m.s.l.) was soon famous as one of the earliest human hunting stations known from Europe, though Cerralbo did not live to see it published *in extenso*. After Cerralbo's death in 1922, despite the site's recognized importance, no one returned to explore it until the 1960s.

It was of course Howell who initiated new fieldwork. In 1961 he also rediscovered the Ambrona site, an analogue to Torralba, situated at a slightly higher elevation

(1,140 meters above m.s.l.) about 3 kilometers away. Though Cerralbo located and tested Ambrona some time prior to 1916, it was only known from the briefest published references (Obermaier 1976: 190, 1925: 180), before Howell's work.

Beginning in 1961, Howell directed three seasons' excavations at Torralba, removing most of the site sediments left intact after Cerralbo's extensive excavations. The seven-week 1961 season proved that a portion of the site was still undisturbed, yielding hundreds of animal fossils and scores of stone artifacts. However, the site stratigraphy proved much more complex than suspected in 1961, when most finds seemed to come from a single archeological level. As a third-year graduate student at Chicago, I joined Howell's team as an assistant in 1962, and after an initial three-week period excavating at Ambrona with Howell and Dr. Pierre Biberson, I spent six weeks working at the Torralba site. Again in 1963, I spent a month digging at Ambrona—Thomas Lynch supervised work there until his departure in June—before undertaking ten weeks' work at Torralba as site supervisor. Emiliano Aguirre undertook limited excavations at Ambrona in 1973 to improve the on-site museum. As co-director with Howell and the late Dr. Martín Almagro, I returned for full-scale excavations at Ambrona in 1980–1981. Howell alone directed one last season there (1983), in which other excavations at el Juyo in Cantabrian Spain kept me from participating. In total, the 1980s excavations lasted 203 days.

Size of Sites and Exposures

The Torralba site was much smaller than its sister, Ambrona. When Cerralbo began work, it may have extended over as much as 3,000 square meters or perhaps slightly more. Although he gave a much lower estimate of his exposures at Torralba, we learned that he had in fact opened at least 1,000 square meters. For a careful, modern excavation, Howell's fieldwork was also undertaken on a very large scale, as its duration would suggest. In 1961, he exposed approximately 450 square meters over the site surface, and during 1962 and 1963, we dug another 576 square meters all told, evidently in a richer and stratigraphically more complex part of the site. (While we left some intact sediments at Torralba as a witness, they are neither contiguous nor easily accessible.) At Ambrona, the largest European Acheulean site known at present (more than 6,000 square meters were "intact," in 1962), Howell's exposures through 1983 attained the truly impressive extent of some 2,800 square meters.

Excavation Techniques

Methods of excavation employed at Torralba and Ambrona from the 1960s on were as close to state-of-the-art as Howell or I could make them under the circumstances. Obviously, appropriate tools and techniques must vary with the nature of deposits, the availability of water, and other factors. At both Torralba and Ambrona, some levels are fine clays that when dry come away in chunks, separating cleanly from the finds they encase, while others are fluvial/colluvial deposits that are sometimes sandy and friable, sometimes indurated to a near rock-hard consistency. At both sites,

excavators used small pick-hammers on the indurated and clayey sediments, as well as knives and trowels, “crochets,” and brushes of several kinds as digging tools.

The excavation was mostly done by workers—farmers from the surrounding hamlets—but they were as well trained and capable as most students I have since had on field crews of my own; some became as technically virtuous excavators as any I have ever known. They were adequately overseen. One trained student assistant supervised the four workers excavating in two contiguous squares, advising them as needed, measuring, drawing, excavating in particularly delicate situations, and so on.

In any excavation (if the excavator is honest), some materials are inevitably recovered “out of context,” and Torralba/Ambrona are no exception: some finds were made whose level was known but whose exact horizontal position, orientation, and so on were indeterminate. Others were recovered in screening. These pieces were not plotted, but bagged by square, sector, and level.

At all times, our excavations were conducted with careful attention to provenience and microstratigraphy. The procedures used were not perfect. They never are. But I do not see how anyone could have done a much better job of excavating Torralba and Ambrona than Howell and his crews. Speaking as one who has at least as much experience directing meticulous excavations as any other Old World prehistorian, I find no contradiction of that evaluation in the work of others since then. The excavations were visited and inspected by a large number of first-class excavators and sedimentologists. The methods used were praised at the time—in the 1980s no less than in the 1960s. Only one person has seen fit to challenge our procedures; I can only characterize her comments as both uninformed and unreasonable (Villa 1990).

Sediments and Stratigraphy

At both sites natural levels of deposition are primarily differentiated by texture, and we were as scrupulous as possible in detecting minor changes in sediments as we proceeded. At Torralba, there are 10 “major” archeological horizons that seem to have accumulated in colluvial screens, or in and on dry channel deposits, or in fans along a pond margin, or in the marshy shallows of a pond. Many recovered bones show localized—rarely complete—polish or abrasion, perhaps by waterborne sand flowing over partly buried pieces; some elongated pieces have polish or abrasion restricted to one or both ends, like the wear on expedient butchering tools of bone from some U.S. buffalo jumps described by Frison (1991: 302–308). Most archeological residues were found atop former temporarily stable surfaces, at the contact between layers of sediment that differed texturally. Very occasionally, the presence of a continuous sheetlike horizon of bones and artifacts within an otherwise uniform level was the only indication of a former temporary surface.

Field designations of levels differ from the final occupation designations (these are “final” only at Torralba). As excavation progressed, and some levels were subdivided (or in some cases where relationships between different spreads of material were temporarily unclear), field designations became cumbersome (“B4aa,” “Occupation X,” etc.). Microfaulting required that final correlation of Torralba levels be done in

the laboratory, using all maps and sections as well as three-dimensional stereoscopic plots and models of the site. Publications that appeared at different times reflect these changes, which has produced some confusion on the part of readers.

The earlier excavations of Cerralbo, in the form of wide trenches, cut through the upper site deposits, producing an interruption in distributions in all the levels affected. Though his trench did not always remove the basal levels, where it was present it appears as a blank in the distributions, and that gap reappears in the same area in maps of all the levels affected. It appears, and is clearly labeled, on the partial map of Occupation 7 published by Freeman and Butzer (1966). Binford, in the course of an *ad hominem* attack, suggested that the distribution gap, and the resulting apparent alignment of materials along its edges in several levels, may be “the structured result of differential erosion,” adding that “Freeman never considered this possibility since he already assumed the hominid behavioral cause of his structured results” (Binford 1987: 58). On the contrary, I have never suggested that the gap means anything other than Cerralbo’s trench, or that its structure is due to prehistoric cultural behavior. It is characteristically careless of Binford to suggest both that I have done so and to offer the innovative “reinterpretation” that the disturbance is instead really some sort of stream channel.

There were several cases of detached islets or spreads of archeological material that occurred atop a single temporary surface but were separated from each other horizontally by interruptions or large gaps in item distributions, sometimes caused by removal of the intervening surface by Cerralbo’s excavations, or happening to coincide with a zone of microfaulting. Since the Torralba stratigraphy is so complex, with fluvial/colluvial levels pinching out laterally or merging to produce a single surface where there were two before, I still believe that the only safe practice, in the absence of some obvious proof that the islets are contemporaneous (such as finding, in different islets, conjoinable bone or stone fragments, or bones from the same identifiable individual), was to keep their contents separate for analytical purposes, even when it seemed likely that they had accumulated at “approximately the same time.” Where solid evidence of contemporaneity was lacking, separation was consistently our practice.

There were nine such “sublevels” all told: four (designated 1a–1d) on the Level 1 surface, apart from the major contiguous expanse of Level 1 itself; two in each of Levels 2 and 4; and one in Level 3. Four of the individual spreads involved were quite small, but five were large enough to provide considerable material. Even without counting these segregated islands of material that occurred in the same deposits, the natural archeological strata distinguished in the field were finer and more numerous than the geological units of deposition recognized by Karl Butzer, who analyzed the sedimentology at both sites.

Vertical and Horizontal Control

We excavated by natural archeological strata, also recording absolute depths to the nearest centimeter below an arbitrary horizontal datum, whose position was marked on stakes in each square. Leveling (within a square) was sometimes done with line

levels; at other times “parallax triangles” were used, following the practice of the late François Bordes. In the 1980s excavations at Ambrona, an optical level was used for vertical control.

Horizontal control was provided by a grid of 3-meter squares, and all visible finds in each square were located with tapes and plumb-bob and piece-plotted at a scale of 1:20. When two pieces were found in direct and intimate contact, they were often given the same feature number. This was explained in notes on the plans, and in the site log or inventory such finds were differentiated as necessary by adding letters to the feature number. Unless the pieces were themselves very similar, the letters were not always placed on the piece labels themselves (there was little reason to do so, since the inventory was expected to resolve any possible confusion).

Orientations and inclinations of pieces were generally visible from or noted on the plans, but where recovered pieces were markedly disconformable to the lay of the stratum that contained them, special measurements, photographs, and notations were made. Naturally, we drew continuous sections showing both geological and archeological levels following all square walls, and abundant photography documents our procedures and finds and the stratigraphic distinctions we made.

Screening

At Torralba there was no available water for wet-sieving or washing finds (and in the 1960s, the needs of the Ambrona farmers for garden irrigation kept us from using the trickle of water seasonally available in the Río Ambrona below that site). Contrary to some of the critics (surprisingly, these include Klein: see 1987: 22–23), we did dry-screen samples of sediments at Torralba. In the 1962 excavations at Torralba, small amounts of sediment were sporadically passed through round screens with a mesh of about 5 millimeters. In 1963, we more systematically screened 15–20 percent samples of sediment (by square and level) from the archeological horizons, and 100 percent of the sediment from three selected squares designated as controls (screening did not include any of the later culturally sterile deposits overlying the archeological levels, though that procedure might also have been informative). In that year, the screens used were specially constructed large rectangular ones, still with a 5-millimeter mesh. The requirements of backdirt disposal dictated the technique employed: we unbolted the screens from their stands, lay them over wheelbarrows, and shoveled excavated sediment through them directly into the wheelbarrows. (Figure 6.1, taken to document the appearance of one edge of Cerralbo’s trench through the area we excavated, shows a screen and its stand.) Screening at Torralba yielded a disappointingly small amount of material.

At Ambrona, in the 1980s, in addition to dry-screening, it became possible to wash sediments in bulk through fine-mesh screens in the stream below the site. This, of course, permitted more complete retrieval of small finds, including microfaunal remains. The richest source of finds was the clayey pond/marsh sediment, much better represented at Ambrona than at Torralba. It is, however, noteworthy that washing did not yield appreciable quantities of small flaking debris.

RESULTS AND INTERPRETATION

Paleoenvironments and Site Formation Processes

The archeological deposits at Torralba and Ambrona were studied by K. W. Butzer in 1962 and 1963, and he returned to Ambrona during the 1980–1981 field seasons. What follows is a brief summary of his results, focused particularly on the Torralba site, digested from his most recent treatment to appear in the forthcoming monograph. His interpretation of the nature of the sedimentary column at that site is based both on his field examination of morphology, sediment sizes, and particle or item orientations, as well as on macroscopic and microscopic analysis of 77 sediment samples taken during the course of excavation and later processed by Dr. René Tavernier in Ghent. I have intercalated the results of the pollen analysis, based on identifications by the late F. Florschütz and J. Menéndez-Amor, to add relevant vegetational detail to the paleoclimatic picture. They analyzed 161 samples (of which many were sterile) taken in two partially overlapping series at 10-centimeter intervals through the Torralba column.

The archeological horizons are found in cold-indicative Pleistocene sediments lying above Triassic Keuper clays. Later lubrication and deformation of the plastic Keuper resulted in a series of microfaults, with thrusts of a few centimeters to as much as a meter, that affected the site sediments.

Following a series of sterile units formed under cold conditions, Member II^b of the Torralba Formation, up to 30 centimeters of coarse, subangular to subrounded gravel, incorporating fine lenses of clay, was formed (“A-Gravel”). The deposit suggests a frost-weathered detritus transported over some distance. Cobbles and larger rocks have been rearranged into stone rings of 25- to 40-centimeter diameter on slopes of 2–5 degrees, elongated into ellipsoidal “garlands” of rock on slopes of 5–10 degrees, and on even steeper slopes torn apart into stone stripes, perpendicular to the contours, or scatters in which individual pebbles, either point downhill or lie parallel to the contours. These are typical “patterned ground” phenomena of periglacial upland environments, attributed to seasonal or diurnal freeze-thaw cycles. The stone rings and garlands are contemporary with the accumulation of the A-Gravel or with the human occupation directly on top of them. Rare artifacts are found reworked in the gravels and clay lenses of this unit. However, the earliest archeological level coincides with the immediate surface of this gravel and appears to be coeval with local lenticles of light gray clay that indicate a shift from high-energy slope mobilization to low-energy subaqueous sedimentation. A single pollen sample from this clayey layer shows high AP values (76 percent), predominantly *Pinus silvestris*. *Sphagnum* spores suggest poorly drained or boggy ground near the site, while sedges are absent. The fact that the NAP is essentially all grasses, with a trace of *Artemisia*, indicates open vegetation on drier plateau surfaces nearby.

Size distribution histograms for rocks from all the archeological levels at Torralba reveal an abnormal frequency of large stones in this level, either indicating far more effective frost-shattering than can be found in recent analogues, or that the larger stones were concentrated in the site through human activity.



■ FIGURE 6.1. *Screens at Torralba (1963)*

The presence of stone rings, garlands, and stripes more or less contemporaneous with the earliest archeological level raises the question whether solifluidal transport or sheetwash disturbed the cultural associations of this particular horizon. From the orientation and dispersal of bones, it is obvious that some sliding has taken place, particularly on slopes exceeding 10 degrees. However, the limited rolling or wear of articular bone surfaces, the lack of size sorting of bones or artifacts, and the nearly articulated position of bones of single animals, all argue that, in general, such sliding has not destroyed the validity of cultural associations—independent of orientation.

Other archeological levels lack soil-frost structures and have not been so extensively disturbed. The best occurrences are in semiprimary context.

Most of the Acheulean occupations are concentrated in levels in Member IIc (Lower Gray Colluvium), disconformably deposited atop the “A-Gravel.” The A-Gravel is absent in the western sector of the site, where Unit IIc rests directly on earlier deposits, the contact distorted by congeliturbation structures. There are several well-stratified subunits and facies in Member IIc that range from gravel layers to unconsolidated, white to light gray or pale brown gritty sands with lenses of fine gravel and sandy silts. Periodic halts in deposition or episodes of erosion interrupt these deposits. This unit is a quasi-horizontal graded valley fill, with abundant fragments of thin-shelled aquatic gastropods (see later discussion) in most finer facies. Current-bedding is visible in some of the fine-sediment subunits. The gravel facies of unit IIc is characterized by angular to subangular shapes, containing some 19

percent pebbles fractured during transport. This suggests very short transport distances but only an intermediate intensity of frost-weathering. There is no evidence of soil-frost structures. Limonitic staining and mottling band the sediments, showing water-table fluctuations. Since these stains do not conform to the lay of the deposits, the water-table changes happened after the site deposits accumulated and even after some microfaulting took place.

A number of cobbles and boulders, varying in major diameter from 20 to 55 centimeters, were probably carried into the site area by Acheulean people, and numerous archeological horizons of variable area are found throughout this unit.

The range of horizontal facies from sandy clays to gritty sands, with some current bedding and discontinuous rubble bands, combined with the aquatic gastropods, suggests a predominantly fluvial depositional environment. A low- to moderate-energy stream crossed parts of the site, and incorporated some slope rubble during periods of intense overland flow, while ponding was not uncommon farther down-valley, at least during the early phases of accumulation. Climate was quite cold, but not as severe as during accumulation of the A-Gravel, and surface denudation was less vigorous.

Pollen spectra attest a cycle of shrinkage and later recovery of a swamp or lake near the site, and continued very cold conditions. Arboreal pollen drops to 36 percent before rising again to its former level. At that point pine forests must have been reduced to scrubby stands in a largely grassland environment. Other (rare) tree species are those that would fringe watercourses or ponds/lakes nearby. Preservation is unusually good for plant material, and bits of wood as well as other material are preserved. Identifications of macrobotanical remains, by Dr. B. F. Kukachka of the Wood Products Laboratory in Madison, Wisconsin, are mostly of conifers, among which *Pinus silvestris* is predominant—presumably brought to the site by humans—but they additionally include one bit of birch and another of *Salix* or *Populus*, that could have been obtained locally. Chenopod pollen increases with grasses in the middle of the series, when attractive and nutritious pasturage was most abundant. Sedges are represented in the earliest and latest pollen samples in the sequence, while *Artemisia* is always present. Increasing desiccation in the midpart of this member was likely due to physiological drought during the cold season, not to a total drop in precipitation: the presence of water lily in one of the grass-rich samples betrays the (perennial) presence of standing water from 1 to 3 meters deep.

The final bed of Member II rests on an eroded surface, attaining a thickness of 90 centimeters in a former topographic hollow in the northeastern part of the site. This "Brown Marl" bed is a compact, light gray to brownish gray marl, intermixed with lime-sand or grit. Diffuse limonitic staining as well as reddish-yellow mottling indicate oxidation in a zone of fluctuating water table. Some cryoturbation festooning is present. A ponded stream channel, spring seep, or the margin of a swampy floodplain is implied. Slope denudation was minimal and the environment was more temperate as well as wetter than during accumulation of Unit IIc, but never as benign as it would be during a full interglacial. Archeological materials in the Brown Marl are very localized in their occurrence. Pollen samples show an initial peak of

AP (80 percent +), declining thereafter. The last Acheulean occupation at Torralba occurs in this unit.

At Ambrona, however, occupations continue into Units IV (Upper Gray Colluvium and Gray Marls) and V (Rubefied Colluvium) of the Torralba Formation. Unit IV begins with moderate-energy fluvial deposition, becoming increasingly low-energy, and attests cold conditions with intensive seasonally concentrated runoff at first. At Ambrona this unit is terminated by gravels indicating a return to higher-energy conditions. There follow marly mixed slope and fluvial accumulations, indicating intensive seasonally concentrated runoff under cold conditions (Gritty Gray Marls) and then the Upper Gray Marls, low-energy ponded or lacustrine deposits in more temperate conditions (though temperate, climate was still some 5°C colder than today). Last come the stratified, in part lenticular, deposits of Unit V, resulting from moderate-energy footslope and valley-margin accumulation by surface runoff and frost-assisted gravity transfer, including alluvial fans at Ambrona. Intensive frost-weathering and vigorous denudation took place on higher slopes, with incomplete vegetative mat (very cold). Dr. Thure Cerling noted that small red quartz crystals in the gravels of this unit were so fresh, and their surfaces so free from abrasion, that they could not possibly have traveled far by hydraulic action (personal communication in Toth, *in litt.*). The final Acheulean occupation at Ambrona took place during the first of the moister episodes in this unit.

There are several distinct Acheulean occupations in the Ambrona deposits just as at Torralba (though they may be fewer in number); since the distributions are still not completely analyzed, they have been grouped into two larger sets in earlier descriptions: those from the Lower Unit and those from the Upper Unit.

Butzer notes that most of the major archeological horizons at both sites are found in seasonally active, valley-margin deposits, in close proximity to permanently wet ground. However, a minority of archeological levels—more at Ambrona than at Torralba—occur within more clayey swamp- or pond-edge sediments themselves, as though shallow water or waterlogged marshy areas were sometimes used for the accumulation of or disposal of archeological residues.

Though Butzer estimates that the accumulation of the Torralba Formation sediments may have taken some 125,000 years, and the Acheulean deposits may date between very roughly 420,000 and 450,000 BP, it must be noted that the deposits and the archeological materials they contain were not accumulated continuously, as Binford (1987) seems to suggest, but rather episodically; long periods of nondeposition and some erosion, and even longer periods when neither artifacts nor animal remains were accumulating in the site deposits, were followed by relatively brief moments of active site use by animals and/or humans, and then by other periods of disuse.

None of the occupations at Torralba is a pristine intact association in true “primary” archeological context, and if earlier papers have not made that sufficiently clear, it has not been our intention to deny it, as some secondary sources seem to suggest (see later discussion).

Size of Samples

If the density of finds at Torralba and Ambrona is not particularly high for well-excavated sites of their age and type, neither is it especially low. The very large size of exposures, coupled with good preservation of organic materials, should suggest that sample sizes of recovered artifacts, bones, and other materials of all kinds are likely to be larger, not smaller, than “average.” At Torralba, 2,141 bones and 689 stone artifacts were excavated during the 1962–1963 field seasons alone.

I find Villa’s (1990: 307) observation that this sample size is too small and sparse for reliable statistical analysis puzzling to say the least. It betrays a surprising ignorance of statistics; worse, it is fundamentally illogical, since she finds no such fault with the much smaller samples from the Aridos quarry localities, which together are less than half that size. In fact, from the published evidence, I see no more reason to believe Aridos a convincing intact butchery site than to consider Torralba the same. At Ambrona, Howell’s investigations produced vastly larger quantities of varied occupation residues: over 2,085 fragmentary remains of the single taxon *Elephas*, and more than 1,400 stone artifacts, have been found in the Lower depositional unit alone to date.

Lithic Artifacts

Stone artifacts are, of course, one principal evidence of human activity at Torralba and Ambrona. Various aspects of the lithic assemblages at these sites are interesting: the raw materials used, the composition of assemblages, the presence of wear traces, spatial associations with other evidence, including conjoinability, and relationships in abundance of specific sets of tools and particular animal species or body parts, are all informative in their respective ways.

Freeman (1991) provides a more detailed discussion of raw material use at Torralba. None of the raw materials used for stone tool manufacture at either site is local. Three basic kinds of stone are represented: cherts/chalcedonous flints, quartzites of variable grain size, and limestones. Although there are outcrops of porous limestone a few hundred meters from either site, they are not really suitable for tool manufacture and were not used. The Triassic clays underlying the site contain no stone raw material. The closest stone sources are suitable limestones a few kilometers from the site; the quartzites used are found no closer than 10 kilometers away, and the flints and cherts would have had to be transported scores of kilometers to the sites. One distinctive and rare kind of flint seems to have been imported from the Jalón drainage, more than 50 kilometers from the site. The Río Ambrona flowing past that site has none of this material in its bed—it could not, for the source is across the divide separating the site from the Ebro drainage, several kilometers downstream on that side. The most probable sources of commoner raw materials are downstream from the Torralba site in the Tajo/Duero drainages. Raw material from any of these sources would have had to be transported upstream to reach the sites, so it must have been imported by humans. At Torralba, aside from the fact that flints are not frequently used to make bifaces, the finer cryptocrystalline materials—

the flints and cherts—were not especially chosen for the manufacture of smoothly retouched working edges such as sidescrapers.

From the 1962–1963 excavations at Torralba, there are 689 stone artifacts, of which 63, or about 9 percent of the total, are geologically crushed (rather cryoturbated than rolled) pieces on flakes. Though they are or once were artifacts, their original typology is indeterminate, so they have always been excluded from detailed analysis of the stone artifact collections, leaving 626 identifiable artifacts. The total includes 1 battered polyhedron and 5 hammerstones (1 percent). Thirty cores and discs make up 4.8 percent of the collection. There are 36 or 5.8 percent bifaces, and 212 or 34 percent shaped flake tools. Minimally retouched/utilized flakes, 160, are 25.6 percent, and unretouched so-called waste, another 159 pieces or 25.4 percent; together they compose 51 percent of the artifacts in the combined collection. When just the shaped tool collection—the 212 flake tools plus 36 bifaces—is considered, bifaces are 14.5 percent of the total for all levels. Scraping tools (60) are 24.2 percent, notches (21) 8.5 percent, and denticulates (48) 19.4 percent of the shaped tool series. There are small proportions of burins (5.2 percent) and backed knives (0.8 percent), while perforators and becs are more frequent (10.9 percent). Two points were recovered. About 4 percent of the pieces are raclette-like artifacts with continuous abrupt retouch on much or all of the circumference. Unclassifiable variants (usually multiple-edged, prismatic-sectioned pieces) are quite numerous—10.1 percent of shaped tools.

From my counts, the lithic collection from the 1962–1963 excavations at Ambrona (all units) is more than twice as large: 1,520 total pieces. These were apparently not all included in Howell et al.'s earlier (1992) summary. The counts that follow are complete for the years in question: I studied the Ambrona artifacts piece by piece when they were on loan to the University of California in the 1970s.

My records show geological crushing to be much less evident than it was in the Torralba series: most of the 199 pieces with coarse abrupt retouch may well be heavily utilized, rather than cryoturbated. But, since the threshold of differentiation between deliberate, irregular, coarse retouch, and geological crushing is hard to draw consistently, they are excluded from the remaining calculations, leaving 1,321 undoubted artifacts. The 50 cores make up about 3.8 percent of that total. Minimally utilized flakes are 212 (16.1 percent) and waste flakes another 636 (48.2 percent) of these: together they constitute just over 64 percent of the collection. The “waste” series included 14 biface trimming flakes and a pseudo-Levallois point. Shaped tools are 391, or 26 percent of the total. The proportion of shaped tools is smaller than at Torralba, and other differences between the two sites also appear. The 47 bifaces (including 3 roughouts) make up 12 percent of the shaped tool collection, scrapers are 36.6 percent (more than at Torralba), notches 13.3 percent, and denticulates 14.8 percent. While notches are more numerous and denticulates less so than at Torralba, their summed percentage representation is about the same at the two sites. The proportion of unclassifiable tools is smaller (only 1.5 percent—multiple-edged pieces are rarer), while burins, perforators, and alternate burinating becs (1.2 percent) are about equally well represented in this shaped tool collection.

Despite the opinion of some authors, such figures—particularly the proportion of bifaces and ratios of unretouched or minimally utilized pieces to shaped tools—are not in any way anomalous for well-excavated Acheulean assemblages from stratified contexts. The proportion of bifacial tools is not particularly low, nor is it uniform from occupation to occupation. While in some units at both sites, there are few bifaces or none at all, there are major occupations with more than 15 percent bifaces (Torralba Level 3), and in Torralba Level 2a the total is nearly twice that (the Level 2a collection is very small). The proportion of waste and minimally retouched pieces would probably be considered low for sites located near contemporary sources of good raw material, but the stone at Torralba (as at Ambrona) was all imported from some distance—some of it from scores of kilometers away, as noted. There is very little evidence for primary flaking or workshop activities at either site, as one might expect from that fact alone. Nor would one expect a great many (but see later) conjoinable pieces at these sites, as compared to the situation at the Aridos or Pinedo quarries, where sources of good stone in reasonably large sizes were readily available locally as cobbles from river terraces—a point that I have tried previously to make, apparently without much effect (Freeman 1991).

While we have called the rather idiosyncratic Torralba artifact assemblage “Late Early Acheulean or Early Middle Acheulean” (*in litt.*), Santonja and Villa consider them typologically later in the Middle Acheulean, comparing our better formed bifaces to the cruder pieces from Pinedo. Pinedo’s age is itself in question, though it is respectably old, but even if it were Early Acheulean, the comparison would still not be conclusive. At Pinedo, a quarry-workshop site near Toledo, the biface series consists mostly of abandoned roughouts, not finished pieces, many of them on obviously flawed raw material. Naturally they look crude. An earlier (1987) study by Carbonell et al. also suggests that the Torralba series, though it may overlap in age with Aridos, is later than Pinedo, and possibly later than Aridos as well. They provide no new evidence for their assessment.

Wear Traces on Stone

Dr. Nicholas Toth of Indiana University examined the Ambrona artifact collections for traces of wear-polish (*in litt.*). He found that none of the tools from atop and in the “pebble-pavement” in the earlier part of the Lower Unit was suited to study: all had a “frosted” surface lustre that obliterated any use-polish.

Artifacts in clayey and sandy deposits of the Upper Unit (Va and Vb), including the fan sediments, were relatively fresh and 37 pieces were chosen as suitable for analysis. Of the larger flakes and retouched pieces, most had use-wear polishes, and where striations were present they were normally parallel to working edges, suggesting slicing. All wear patterns found are consistent with hide, meat, and (rarely) bone being the material operated on. In only one case was there wear indicative of “heavy” hide working, and no plant polish was observed. Toth concludes that the presence of little “unused” waste suggests minimal on-site flaking, and since micro-wear patterns are consistent, indicating animal butchery, while other patterns are

lacking, the site seems to have been specialized, rather than a base camp or some other general station.

Conjoinable Lithics

The study of conjoinable stone artifacts is an informative addition to the analytical battery of the prehistorian; despite a widespread misapprehension, it was not ignored in our work at Torralba and Ambrona. Dr. Nicholas Toth has had the Ambrona study under way for some time, but my knowledge of his results is too sketchy to include. I do know that there were conjoinable pieces in the 1962–1963 collections from that site: my notes indicate that feature 50D, IV, 7a, and 7b (two fragments of a quartzite chunk) can be rejoined and refit to 50F, IV, 13, and that 50F, IV, 1 is also attributable to this chunk (but will not join); another pair of refittable pieces is 48E, IV, 6 and 48F, IV, 6. I presume that Toth may have identified other cases.

For Torralba, my information is relatively complete, since we had the lithics in Chicago for study (and replication) for an extended period. The series includes a relatively small number of conjoinable stone artifacts. Of the 626 classifiable artifacts (excluding congelifragments), 29 are conjoinable fragments. We were quite aware of the potential information to be gained from such pieces, and most of them were detected during the course of excavations. The field identifications were all verified in Chicago. There were only two cases (totaling 6 flakes), where the conjoinability of pieces was first recognized in Chicago. It is possible of course that the collections still contain one or more conjoinable pieces that I missed, but I would not expect their number to be large. Nor would I expect there to be many such pieces in the smaller 1961 collection that I have not examined as closely. The following list does not include the several cases discovered of artifacts that are probably attributable to the same core or chunk of raw material, but could not actually be physically conjoined.

The 29 conjoinable artifacts found in 1962–1963 are from 12 occurrences in 7 levels at Torralba. Their provenience and separation are shown in Table 6.1.

The data in Table 6.1 are remarkable. Virtually all the conjoinable materials identified in the Torralba collections are pieces that were found with very little lateral separation between them or none at all (the four pieces level-bagged from Occupation 2 were found very close together and placed in a matchbox, but the markers indicating find positions were accidentally disturbed before they could be mapped). The unusually small lateral distance between the pieces would seem to imply that neither during deposition nor afterwards were they affected by any appreciable lateral transport. The separations noted are in fact small in comparison with average distances separating conjoinable finds in other situations where there is no possible question of fluvial transport, where distributions are universally agreed to be “human-made,” and have always been interpreted as such. That would seem to be a datum to bear in mind in evaluating the possibility that long-distance water transport and rolling have altered bone surfaces or materially affected the original distribution of recovered materials at Torralba.

■ TABLE 6.1. *Conjoinable pieces from Torralba*

<i>Occ.</i>	<i>Feature</i>	<i>Material</i>	<i>Description</i>	<i>Separation</i>
1	L9, 37	Chal. flint	Tr. s/scr w/2 flakes	0 (touch)
	L18, 1	Chal. flint	2 flakes	6 cm
1d	D6, 58	Quartzite	1 fl. atop 1 core	0 (touch)
2	I24, Lev	Chal. flint	4 tiny flake frags	< 10 cm
2b	D9, 69	Chal. flint	2 flakes together	2 cm
4	I21, 1	Chal. flint	2 util flake frags	0 (touch)
	I18, 26	Chal. flint	2 flakes	< 10 cm
	H18, 8	Chal. flint	1 burin, 1 ret. fl.	< 10 cm
	I21, 1	Quartzite	2 complete s/scr	4.2 m
	&			
	J24, 36			
5	F9, 39	Quartzite	2 lg. fl atop 2 small	10 cm
7	G15, 21	Chal. flint	2 flakes	0 (touch)
	H15, 17	Chal. flint	2 complete s/s	4.5 m
	&			
	G12, 44			

In two cases only at Torralba, fragments of the same original piece were found separated by 4.2–4.5 meters. But those cases are unique. Each involves a pair of complete sidescrapers made on two refittable pieces of a single large flake. In both cases, the flake was broken before the final shaping of the individual sidescraper edges took place. With such data, human agency seems the most likely explanation for the separation of the find spots.

Faunal Samples and MNIs

The Torralba fauna—its makeup, condition, significance, and abundance—has been the subject of some debate, partly because of differences of opinion about identifications and individual estimates provided by the two principal faunal analysts, Emiliano Aguirre and Richard Klein. I believe that a significant part of the disagreement between them can be resolved at this time. A certain amount of disagreement will remain unexplained, particularly where a single feature seems to have been attributed to two different taxa. Even in that case, part of the difference is due to the assignment of a single feature number to two (rarely three or four) pieces found together in a level, in intimate contact.

Sometimes, curation procedures that are beyond the control of Howell or the excavators were the cause of later analytical problems. Materials once excavated were removed (after plaster jacketing, where necessary) for shipment to the Museo Nacional de Ciencias Naturales by workers under Aguirre's direction. Some faunal materials—and this is particularly true for the shafts of ribs—were discarded by that

team as “requiring excessive museum space for their limited scientific interest.” All such items were identified and thoroughly examined by Aguirre beforehand. While I have no reason to question his identifications, such pieces will of course not have been available to Klein for his later study. There is some reason to believe that among the bones so treated were some that bore possible marks of human modification.

After arrival at the museum, several of the bigger and more impressive bones—particularly elephant bones—were selected for display. Those pieces were repaired—sometimes separately found fragments of the same bone were rejoined—and their surfaces smoothed where necessary and coated with preservative. Pieces so treated often or usually lost their identifying labels in the process. And, the surface treatment they received obliterated what I had identified as cutmarks in some cases, or made it impossible for Klein to differentiate modern damage from ancient modification. While the number of pieces so affected is not large, most of the information that they might have provided is forever lost. A larger number of plaster-jacketed pieces—some tusks, skulls, mandibles, pelvis, and scapula fragments, as well as the bigger and more complete limb bones—were stored in their jackets and remain in them. Consequently, Klein was unable to examine and identify them, and any information they provide about human agency or carnivore action is for the time being inaccessible. A still more important problem has been that most of the pieces have been relocated and relabeled on several occasions during periodic museum reorganization, and an even larger number of (usually smaller) items has become detached from its labels, misplaced, confused with other materials, or outright lost in the process. Last, the 1960s collections are reported to have been partly dispersed due to overlap in function between museums on an intra- (should these remains be regarded as primarily paleontological with tools, or primarily archeological, with bones?) or interregional (do they belong in Madrid or in Soria?) scale.

In cases where finds can no longer be identified, we have no recourse except to accept Aguirre’s faunal identifications and his, Howell’s, and my observations recorded in our field and laboratory notes.

The discrepancy between Aguirre’s counts of taxa and Klein’s can be partly explained on this basis. Klein, after all, saw only 1,521 (71 percent) of the 2,141 bone fragments recovered, and among the bones he could not examine were a substantial part of the largest, most readily identifiable skeletal elements.

That by itself will probably not account for most of the discrepancy. For the 1962–1963 Torralba excavations, Aguirre calculates an estimated minimum of 116 individual animals (112 mammals) for all levels, of which 37 are elephants, 23 equids, 21 red deer, 15 aurochs, 7 *Dama*, 5 rhinoceros, 2 lions, 2 small carnivores, and 4 Aves. (Azzaroli *in litt.* identifies one of the cervid mandibles as *Megaceros* sp.) Klein, in contrast, estimates only 64 individual mammals: 15 horses, 14 elephants, 10 red deer, 10 aurochs, 8 *Dama*, 4 rhinos, 2 lions, and 1 lagomorph. Klein then has 48 fewer individuals (excluding the birds) than Aguirre. Another factor helps resolve most of this difference.

Klein’s MNI calculations were derived on the basis of counting repetitions of the best represented body part for each taxon in each “level”—surely accepted practice,

and the most conservative, justifiable way to proceed. However, when Klein calculated MNIs, he combined remains from the sublevels or spreads discussed earlier with the major horizon with which they were associated: all sublevels of Level 1 were united in his Level 1, and so on. When levels are combined, the MNI count invariably drops, as Klein himself illustrates in his chapter in the forthcoming monograph: uniting all Torralba levels drops his total MNI by almost 50 percent—from 64 to 34! Combining sublevels as he did by itself eliminates from Klein's level-by-level counts 42 animals that would be called different individuals were the subhorizons differentiated, reducing the overall discrepancy between Klein and Aguirre to 7 animals. Since Klein only saw 70 percent of the bones, a difference of this order of magnitude is scarcely cause for alarm. Some unexplainable differences still remain: Klein's list, though shorter, has one more *Dama* than Aguirre's, and a lagomorph (which may be one of the otherwise missing "small carnivores" in Aguirre's list).

Klein originally characterized the mortality profiles for Torralba elephants as catastrophic (*in litt.*) but has later stated that the sample size was probably too small for reliable estimation, suggesting that "if the Torralba and Ambrona 'Lower' samples are combined, the case for attritional mortality is especially strong" (Klein 1987: 29). However, if combining remains from different sublevels is likely to be misleading, combining remains from different sites is much more perilous. In fact, when the remains of all bones (not just teeth) from the larger sample of ageable "individuals" obtained from the separated sublevels are examined, the Torralba mortality profiles once more become catastrophic rather than attritional. If that is a correct diagnosis, the observation made by Santonja and Villa (1990: 61), that "the mortality profiles . . . cannot be reconciled with Freeman's and Howell's view of the sites," is wrong. (I believe that it is best to reserve judgment about the shape of the age distribution at Ambrona until the final level distinctions have been established, and the occupation contents correlated across the site.)

At various times Klein has suggested that even catastrophic profiles might be explained by nonhuman agency, suggesting the drying of water holes or flash-flooding as likely alternatives. However, there is not the least geological or paleoenvironmental evidence for either phenomenon at either site. In the prehistoric environmental settings as they are now understood, truly catastrophic age profiles would almost certainly imply human agency.

Birds

Bird remains from Torralba and Ambrona have been identified by Antonio Sanchez and E. Aguirre (Sanchez and Aguirre *in litt.*). At Torralba, the four specimens recovered are all water birds: a "wishbone" of *Tadorna ferruginea*, the ruddy shelduck; a scapula of *Mergus serrator*, the red-breasted merganser; a humerus of *Porphyrio porphyrio*, the purple swamphen; and a coracoid from an unidentified anatid. There is no reason to believe that these creatures were captured by humans—such small, light remains may have been dropped nearby by kites or other predators and washed into the site deposits, and none is cut or otherwise altered. At the right season, all

could have been found nesting in the reedy edges of lakes or slow-moving streams at Torralba—the merganser would normally be found near more northerly seacoasts, far from Torralba, at other seasons, and the swamphen, a partial migrant, though occasionally reported as far from its southerly range as Norway, would not ordinarily be found in as cold conditions as those at either Torralba or Ambrona during the winter season (Vaurie 1965: 138–39, 357–58).

Twelve bird bones were recovered from Ambrona; the provenience label is missing from one of them. In addition to the swamphen and the merganser represented at Torralba, the provenienced items are bones of *Anser anser* (graylag), *Anas acuta* (pintail), *Fulica cf. atra* (coot), and *Vanellus vanellus* (lapwing). All but the lapwing are waterfowl, and it too inhabits the banks of ponds and shores as frequently as moist meadows. The coot prefers large, open bodies of water. Like the merganser, the pintail is tolerant of brackish water (Vaurie 1965: 116–17, 359–360, 389). Again, there is no evidence that these bones are related to any human activity at the site.

The avifauna tells us something about local environments, but the species list is chronologically uninformative. It is interesting that most bird remains were detected in the course of excavation, even at Ambrona; few specimens were recovered by washing.

Small Fauna

The Torralba deposits did not yield much in the way of small fauna, aside from the often intact remains of tiny freshwater snails, some specifically pond dwellers, dominated by *Hydrobia* sp., denizens of streams, ponds, marshes, and backwaters. It is notable that this genus is a recent invader of fresh water and is salt tolerant. They and the birds confirm the presence of bodies of water near the site but are otherwise climatically uninformative. My notes also indicate a 1960s identification of a pelobatid (spadefoot) toad from the site, but it is unclear and the material is not mentioned in later references.

At Ambrona, where the 1980s sediments were washed, samples of small animals were recovered in some abundance. They were identified by Drs. C. Sese, B. Sanchiz, and I. Doadrio in Madrid. Sese recognized the insectivore *Crocidura* sp., the rodents *Arvicola aff. sapidus*, *Microtus brecciensis*, and *Apodemus aff. sylvaticus*, as well as the leporid *Oryctolagus* (Sese *in litt.*). (*Lepus* was said by Aguirre to be represented in the 1960s material.) *Arvicola*, the water vole, is a strong swimmer that prefers to live in cool, humid ground near bodies of water—I would be surprised if *A. sapidus* can be differentiated from the more northern form *A. amphibius* from the material recovered. This surprisingly impoverished fauna suggests a post-Biharian age for the site but is not otherwise very informative.

Sanchiz identified anurids including *Discoglossus pictus*, *Pelobates cultripes*, *Pelodytes punctatus*, *Bufo bufo*, *Bufo calamita*, a *Hyla* (*H. arborea* or *H. meridionalis*), and *Rana perezi*, as well as the water snake *cf. Natrix* (Sanchiz *in litt.*). *Discoglossus* is usually found in bodies of water or their damp grassy banks. *Pelobates*, the spadefoot

“toad,” lives in dry, sandy ground close to bodies of water (Salvador 1974), excavating galleries in which it can survive long dry or cold periods.

Fish remains were found in considerable numbers, but all may probably represent a single species: *Rutilus arcasii*—its first documented fossil occurrence; less precisely identifiable remains were all attributable to *Rutilus/Chondrostoma* sp. or to indeterminate cyprinids (Doadrio *in litt.*), all of which may very well be from the same species. That in itself is interesting since *R. arcasii* has been found as the exclusive fish colonizing some interior drainage lakes in Spain (Doadrio *in litt.*). The species prefers to live in and near the reedy shallows of sluggish or tranquil waters and is absent from turbulent streams or very cold water. The waters of lakes deep enough not to freeze solid may be warm enough for them to survive year-round even in cold climates.

For the number of remains that were recovered by washing, the poverty of small mammal, reptile, and fish taxa is noteworthy. These creatures were all most probably resident at the site during its formation. The species found coincide in showing that the site environment was characterized by lakes, ponds, and marshy ground. As far as refinements of dating are concerned, they are unfortunately banal. There is no indication that any of them were used by people at Ambrona.

Carnivores as Agents of Bone Accumulation

Binford and others have suggested that the accumulations of animal remains at Torralba and Ambrona may be due to natural causes having nothing at all to do with the human presence seemingly attested by the stone tools. The excavators (and later, Shipman) detected traces of animal gnawing on a few bones. Discussions by Klein have reinforced the impression that carnivore remains or coprolites are quite abundant at the sites. Klein characterizes coprolites as “numerous—although artifacts are more numerous than coprolites” (1987: 18), thus giving the unfortunate impression that there must be many hundreds of large carnivore coprolites at Torralba and Ambrona, when in fact that has never been demonstrated. These observations have suggested to some that animals may be the major agents involved in the bone accumulations.

To the contrary, carnivore remains—bones as well as coprolites—while present, are rare at both sites. Even where present, specimens that are apparently coprolites must be further analyzed before their meaning is clear. Most of the fragments considered to be coprolites are not well-formed scats, but fragments of clayey sediment containing small bits of bone. At some mid-Pleistocene sites near Madrid, I have seen small clumps of clay filled with crushed or whole remains of the bones of small mammals and reptiles that are probably fossil pellets of raptorial birds. In the case of true coprolites, only detailed analysis of their contents can determine which carnivore is responsible: even some amount of decayed “bone-meal” (which may be present in scat of foxes and smaller carnivores) is no guarantee that hyenas are responsible. Furthermore, the feces of several small carnivores contains bone fragments. Klein has certainly identified coprolites at Ambrona. I have seen some

of them myself but I don't think that analysis of the specimens has been thorough enough to show that all the bits of bone-rich clayey sediment from the site were produced by large carnivores, or in particular, hyenas.

Bones of carnivores large enough to have killed the animals represented at either site or to have gotten their jaws around the bones of the larger ungulates to gnaw them are very few indeed, and marks of gnawing at Torralba have been said to be as rare as cutmarks apparently due to human modification. There are just two lion bones at Torralba: one in Occupation 1c and one in Occupation 4 (Klein lists the latter in Level 3). No wolves, no bears, no hyenas—in fact, no other large carnivores at all—are represented at that site. There are, of course, possibly two small (mustelid-sized) carnivores in Aguirre's list, one from Level 4b and one from Level 10 but even if both are carnivores, they are certainly not the bone accumulators at Torralba. Even the Torralba lion, a respectably large cat, could not have dealt with a healthy adult elephant the size of those at Torralba—with shoulder heights verging on 11 to 12 feet—though lions could certainly have killed some of the other animals, and they might very well have—probably did—scavenge from carcasses of animals dead from other causes. How any of the carnivores represented could have managed to remove the appendicular bones of the large elephants, as Klein (1987: 25) suggests to explain their rareness compared to the abundance of innominates, is quite unclear; the imbalance must be due to some other agency, and among the alternative possibilities human activity seems the strongest.

At Ambrona, in the Lower Unit, both hyena and lynx are represented by but a single individual each, while indications of carnivore activity are not abundant at Ambrona, and Klein and Cruz-Urbe identified just three bones as bearing marks of carnivore chewing (Klein 1987). Such figures as these do attest a carnivore presence but are scarcely convincing evidence of a major carnivore role in the accumulation or alteration of the mammal remains from either site.

One might object that marks of carnivore activity could have been obliterated by natural alterations of the bone surfaces during or after their deposition. But if that is the case, as many marks of human alteration could have been obliterated at the same time. Arguments that postulate that a mechanism that is inherently non-selective is responsible for selective destruction of particular kinds of data are inherently fallacious.

Implications of the Surface Condition of Bones

Emiliano Aguirre, in his original study of the faunal remains from Torralba (*in litt.*), said:

The preservation of the vertebrate remains at Torralba varies from good, even sometimes excellent, to specimens having been altered in various ways, some prior to the process of fossilization and others, clearly subsequent to that process. In respect to the latter situations it is worth noting that there is relatively little breakage attributable to processes—such as mechanical deformation due to tectonic events or other such causes—within the sedimentary body itself. . . . On the other

hand, in not a few instances, there are clear evidences of modification to faunal elements as a consequence of post-depositional chemical or biological processes, which hamper the identification of features of interest on a number of pieces.

Superficial decay or degradation of the bone and dendritic patterns produced by invertebrates and roots occur with some frequency, indicating interruptions in the process of sedimentation, deflation, and even periods of atmospheric exposure. He noted that exceptionally, bones were seen to exhibit a uniform polish all over, or all over one flat surface, but observed that “relatively few bones exhibit erosive traces over the entire surface, such as might result from water washing over a fossiliferous horizon, and leading to smoothing of protruding body parts through transport and rolling, or more rarely, aeolian processes” (Aguirre *in litt.*). He goes on to say “the great majority of modifications of bony elements fall into regular patterns,” particularly patterns of breakage, incision, and percussion, “that can be attributed to cultural activities.” Aguirre thus suggested that the bone was in good enough condition, despite surface alteration, so that traces of deliberate cultural modification could still be recognized on some—perhaps many—bones, and in this I concur. From the outset, Aguirre and all other analysts have recognized that surface abrasion exists on a number of specimens from Torralba (and Ambrona). However, Aguirre’s assessment of the general state of the bones is much more positive than the later diagnosis by Richard Klein.

Klein (1987: 19–21) states that at both Torralba and Ambrona

intense post-depositional leaching . . . has corroded bone surfaces. . . . The alterations introduced by leaching and corrosion are compounded by the massive fragmentation that occurred during and after burial at both Torralba and Ambrona. . . . It is notable that one-third of the 1779 bones at Torralba and one-sixth of the 4326 bones from Ambrona “Lower” exhibit edge-rounding that Butzer (pers. comm.) suggests occurred during limited fluvial transport on seasonally activated valley-margins or during net transport of sandy alluvium that partially buried the bones. Many bones that are not conspicuously rounded show a distinctive polish or luster and probably would exhibit abrasion or edge rounding under magnification. . . . Using a hand-held glass on a sample of lustrous Torralba and Ambrona bones, Butzer (pers. comm.) found parallel microstriations from abrasion by sand-sized particles on every one. (1987: 20)

He notes that Shipman and Rose also found “rounding” (under greater magnification) on nearly every specimen they examined from the two sites and goes on to say that “excepting abrasion and corrosion, Cruz-Urbe and I found little other damage on the Torralba and Ambrona bones” (1987: 21). The total number of carnivore-chewed pieces they detected was 14 from Torralba and 3 from Ambrona Lower, while the number of possible stone tool cutmarks was 22 at Torralba and none from Ambrona Lower. (Klein recognizes, of course, that surface corrosion may have obliterated other traces of both kinds.)

Butzer’s observations on this subject are recorded in an appendix to his final faunal chapter in the Torralba monograph. It is worth quoting *in extenso*. He reports:

The conspicuous concentration of archeological materials in such coarser-grained, intermediate energy horizons cautions strongly against diagnosing these as intact, primary associations. Instead, it is probable not only that there has been a measure of pre-depositional dispersal, but that at least some of the archeological micro-horizons are telescoped lag levels. This is strongly supported by my 1981 examination of the Torralba bone in the Madrid museum. Every specimen selected at random under low-power magnification showed systematic, very fine, longitudinal and parallel striations and had a “sandpapered” feel. This systematic striation was noted on all sides of each bone and was strongest on the most-exposed ends. It can only be explained by sand transport below, above and around the bone, resulting from energy conditions adequate to transport sand but mainly inefficient to move large bone; repeated burial and exposure is therefore probable. This is not incompatible with my conclusion that the archeological occurrences may retain their basic associations, i.e. between bone and bone, or bone and artifact, despite some horizontal displacement and changes in orientation. But the problem of telescoping bone and artifacts into “pseudo-floor” lags is more serious than I had anticipated. Trampling and sinking of heavy objects in wet clayey sediments is less problematical than at Ambrona, although it bedevils interpretation of those archeological materials at Torralba that are found in or at the base of clayey deposits. In effect, like all other Paleolithic open-air sites that I have examined since 1961–1963, the best associations at Torralba are in sediment taphonomic terms, semi-primary. (See Butzer 1982: 120–22)

Some more or less significant differences in these three observations call for comment. Sometimes they are quite subtle, but the differences have such important consequences for interpretation that it is essential to be quite careful about language. Aguirre’s description makes the Torralba fauna sound relatively intact, and relatively informative about cultural behavior. Klein in contrast talks of the “intense pre- and post-depositional destruction that affected the Ambrona bones” (1987: 27) (a description that can only be fairly applied to the Ambrona Upper series, where intense leaching has removed most bone).

Butzer’s description does not make it clear whether his sample was chosen from all bones or all visibly polished bones, as Klein suggests, but that is of less consequence than the conclusions he derives. His term “semi-primary” implies limited dispersal of cultural materials prior to burial, after which the buried deposits are subject to some disturbance (Butzer 1982: 121). In the depositional unit at Torralba bearing most of the archeological materials, though its sediments deposited under cold conditions in valley-bottom deposits, there is little cryoturbation and transport distances must have been quite short, Surface abrasion of bone could be ascribed to sediments passing around the bones, rather than to lateral movement of the bones in the sediments. The lack of preferential orientations or size-sorting would seem to support this possibility. Archeological associations, as Butzer points out, could survive this degree of disturbance and still be recognizable. Only his conclusion that the depositional environment is one in which different archeological levels might have been telescoped into “pseudo-floor lags” poses any substantial theoretical problem to cultural interpretation.

Butzer's conclusions are borne out by the archeological field observations. The local merging of elsewhere discrete levels shows that even the thinnest, apparently most pristine level might contain materials originally deposited in several separate episodes. But lag deposits have a geo-archeological signature. Ordinarily lag deposits built up over any length of time may be expected to be heterogeneous in content, and different lag deposits should differ in random ways. That is because, as a rule, the depositional conditions were different for each of the discrete "moments" that later telescope to form a single apparent "floor." Ordinarily, the materials in one lag deposit don't differ from those in another in patterned ways, unless the landscape and the conditions of deposition have remained so constant that the local depositional environment has repeatedly caused accumulations of materials of the same size and shape to be dropped in essentially "the same spot." Only then should telescoping of formerly disparate levels produce a horizon (or horizons) whose contents are both internally homogeneous in their characteristics and different from others in regularly repeated and predictable ways. Such cases are by no means geologically exceptional; nevertheless, careful examination should reveal the essentially "geological" nature of the accumulation (due to similar behavior of items whose sizes or shapes are analogous when waterborne or moved by gravity, etc.). What is more, in the archeological case, the original cultural behavior that produced the residues forming the lag would of course have had to be essentially similar during each episode of accumulation, implying the repeated performance of the same set of activities in the same part of the changing prehistoric landscape (whether this is the actual area excavated or other areas which served as sources to the lag). The evidence of the accumulations called "occupations" at Torralba and Ambrona runs contrary to such an interpretation.

Another problematic situation he mentions is that of the "sinking of heavy objects in wet, clayey sediments." At Ambrona, there are some situations in clayey sediments in which skeletal remains of several animals were found lying one above another in layer-cake fashion, and in the absence of other evidence, it would be a mistake to interpret these as single cultural accumulations. At Torralba, this is less a potential problem than at Ambrona, since the major accumulation at the base of clays (the clay facies in the north half of Occupation 7) consists principally of the bones from one side of one individual animal, in a somewhat rearranged "near-anatomical" position. Since that individual died but once, the question of whether or not it sank, and at what rate, is immaterial. The large stones in the same horizon that are interpreted as part of this accumulation were pretty evidently positioned in relation to the bones: again, sinking provides no objection to previous interpretation. I see no reason to believe that all else is a culturally meaningful association, while the stone tools in intimate juxtaposition to the bones are extraneous.

The concentration of accumulations within or at the base of clayey deposits certainly does impose peculiar restraints on interpretation—in some cases it may even rule out explanations in purely geological terms. It is hard to account for differences in the distributions of materials deposited in still-water or marshy sediments, particularly the sorting of large, dense items such as elephant bone, in terms

of geological agency. If the accumulations are found at some distance from the edge of a prehistoric lake or bog, and there are no nearby channels in which flow would have sorted them as they were swept along, the discovery of sorting by body part or bone size or shape may well have cultural rather than simply geological significance.

Figure 6.2 shows an example of this sort from Ambrona. In 1980, we found a group of five elephant tusks of different sizes, lying in close proximity in clays (not an isolated example—other tusks were found grouped together not far away in the same deposits). One of the tusks was near vertical in the clayey sediment. There are no faults or other disturbances in the deposits that could account for its attitude. It must have been buried that way, fast enough so that it was not weathered to pieces. Its position may very likely be due to a heavier tusk having sunk more rapidly, trapping the point of the smaller one and pulling it down into the angle it maintained at discovery. While the attitude of this single find may be purely a result of depositional processes, I do not see how any natural agent other than human activity can explain the spatial segregation of the tusks from other bones in these deposits.

The sediments are still-water beds, not stream deposits, and assortment by channeled flow is out of the question. No geological force as far from a contemporary channel would have separated these five tusks from other relatively same-shaped body parts and dumped them all together.

Non-human biotic agencies are also improbable agents. The tusks are uninteresting to carnivores, who in any case would scarcely have dragged them all into a separate pile in muck or standing water. As Villa notes, elephants today often pick up and carry about bones of their dead congeners, and anyone who has seen filmed behavior of this sort must admit that it is remarkable. However, they do not sort the bones and dispose of them in piles segregated by body part. Rather, they seem to carry or drag the bones about for a bit, then toss them away apparently at random. Peter Beard (1977) has published scores of photographs of dead elephants, including some astonishing natural accumulations of bone, but in the few cases he shows where bones are segregated by body part (or arranged into tidy localized piles) the hands of humans were responsible.

Butzer's concern about the problematic effects of sinking in clayey deposits is doubtless well placed. On the other hand, such sediments may, in special cases such as the ones just described, constrain geological interpretation in directions that pave the way toward an understanding of cultural phenomena.

Marks of Human Alteration on Bone

On many of the bones from Torralba and Ambrona, there are marks that I do not believe could have been made by any non-hominid agency. The marks are gross enough in most cases so that surface alteration has not obliterated them or rendered them unrecognizable. Of course, those marks will never pass muster as evidence for hominid alteration if one insists that the bone surface topography must be essentially fresh for the markings to be studied at all. That requirement has been both one



■ FIGURE 6.2. Group of elephant tusks—one vertical—in Ambrona Lower Unit (1980)

of the strengths and, at the same time, one of the weaknesses of a recent study of some Torralba and Ambrona bones undertaken by Shipman and Rose (1983).

They subjected replicas of surfaces of some of the smaller Torralba bone fragments to reexamination for microscopic evidence of cutting and gnawing, using the scanning electron microscope. Shipman's criteria for identifying cutmarks are very exacting, and her procedure rigorous. Therefore, there is very little doubt that a bone she identifies as cut actually bears marks that most would find convincing evidence of such alteration. She found such marks on some of the Torralba bone, and I suppose that I should be pleased that there is some support for my belief that hominids altered some of the Torralba bones. While it may seem contrary of me, I have several reservations about the Shipman and Rose study.

Most important, I believe that their description of procedures as published is unreliable, and the estimate of the proportion of sliced bones in the collection they offer is therefore unusable. I don't mean that the marks identified do not exist or were wrongly counted, but that other statements about the study and the size of samples examined incorporate serious errors.

For one thing, Shipman claims to have examined all the Torralba bones. However, there is no way she could have examined the whole collection, since the plaster-jacketed bones Klein was unable to see are still in the same jackets. (Many of the marks I find most convincing are found on those larger bones—major parts of elephant pelvis, whole tusks, mandibles, or elephant and bovid crania—under the plaster jackets.) Second, she claims to have found convincing marks of hominid alteration on a total of 12—some 1.2 percent—of the replicas examined from Torralba. The figure cannot possibly be correct.

After Klein's reclassification of the Torralba bone, the museum collections were reorganized, replacing the finds in the shelved lots by square, level, and feature number, rather than by species and body part. (The only exception is a lot of 22 bones Klein suspected might be cut and had shelved separately for future study.) Any thorough examination of all bone finds would first require opening every box, locating the label (square and feature number) on each piece, and then identifying it from Klein's inventory of taxa and body parts, a process that by itself would necessitate several days' work. Then the surface of each bone would have had to be completely examined under proper lighting, even on occasion under low magnification. Next, suspect bone surfaces would need replication, in itself a time-consuming process. To examine this bulk of material carefully and replicate the specimens that seemed altered would require a minimum of several weeks' time. This estimate may be approximately doubled because of the shortness of the museum's hours—ordinarily only 4 to 5 hours of access to its warehoused collections are permitted each day.

Shipman spent in all several hours with the collections, not several weeks. In such limited time it is not possible that she could have had time to examine, let alone replicate, more than a few bones from the Torralba collection. For 12 to be 1.2 percent of the replicas made, Shipman would have had to make a thousand of them. In the short time available, this is an unrealizably high number, even if several replicas were made of any single bone.

It seems to me most probable that, given the time restrictions of her study, Shipman must in fact have spent almost all her time on the two dozen bones Klein had set aside, looking at others only as (or if) time permitted. Perhaps this is actually what Shipman and Rose intended to say. Whatever the explanation, the account they give of sample size and procedures is inconsistent with the nature and size of the collections.

If what Shipman and Rose really examined was just the collection Klein thought might be worked, their sample was doubly constrained by any preconceptions he may have had at the time about the nature of bone working, and by his ability under the less-than-ideal conditions in the museum to distinguish marks on bone. Results of the Shipman and Rose study would then be unintentionally biased, no matter what their remaining procedures.

The Shipman-Rose study provides some information of qualified interest. They did find 12 convincing marks of human alteration on 4 bones of *Paleoloxodon*, 3 of *Equus*, and 1 of *Cervus*, as well as on 1 bone of indeterminate species. In a clear misunderstanding of the evidence, Santonja and Villa state that “the rarity of cutmarks on the bones . . . cannot be reconciled with Freeman’s and Howell’s view of the sites as places where herds of elephants were killed and butchered” (1990: 61). But Shipman and Rose actually said that their study offers “only limited” support, not “no support” for the fact that Torralba (and Ambrona) were butchery sites. There is a real difference. And, there is no reason to suppose that butchering marks need to be abundant even in a culturally modified faunal collection. Visible cutmarks may be very rare—even virtually absent—in more recent butchery sites, such as some “buffalo jumps” in the United States, where humans are known to be the principal or only agent of bone accumulation and/or alteration. As mentioned previously, Shipman and Rose also detected carnivore tooth scratches in comparable frequency. They were characterized as less abundant than might be expected of assemblages where carnivores were the primary agents of bone alteration. Shipman and Rose’s conclusions do not correspond to Santonja and Villa’s summary.

In sum, despite its problematic nature, the work of Shipman and Rose is nonetheless interesting insofar as it provides some direct evidence of apparent human intervention in the alteration of the Torralba bones. However, theirs is far from the “last word” on the subject. There are other kinds of apparently cultural marks on bones from these two sites that were not considered in their study. By far the most abundant marks that were earlier interpreted as signs of hominid alteration are grosser traces than the fine slicing studied by Shipman. They consist of large-scale scars of gross damage—marks of battering; chopping with a large, sharp, wedge-shaped edge; scraping, or abrasion with a smooth, blunter stone edge; and deep slicing, gouging, or grooving. Though they occur on bones whose surfaces have also been altered by natural postdepositional phenomena, they have resisted obliteration. Quite comparable coarse marks of hacking are identified as butchering traces at the Casper site (Frison 1974: 36–37) and elsewhere and have been interpreted as important evidence about butchering techniques at those sites. Shipman’s methods simply ignore all such evidence, which to me seems as obvious and as convincingly indica-

tive of human handiwork as the pristine fine slicemarks she studies. The coarser topography of such marks was the basis for my own field counts of worked bones, in the majority of cases.

In collections from sites where bone surfaces have undergone more than minimal postdepositional alteration, as at Torralba, macroscopic butchery marks may be the only ones that can survive. Most bone under such conditions cannot preserve the diagnostic microtopographic features, the fresh traces of fine slicing, that Shipman's microscopic study relies on. Marks of gross damage certainly merit further investigation, instead of summary dismissal.

I examined the bones from Torralba while they were being excavated, while the surfaces were "fresh," unvarnished, and still unjacketed. It was then still easy to tell fresh excavation damage from ancient alteration. Workers alerted me as they recognized apparent human modification, so I watched many of the surfaces as they were cleaned and excavated not a few myself. In the field, I identified four types of modifications that seemed to be cultural: slicing, hacking with a wedge-shaped edge, scraping or abrasion, and battering or repeated percussion. My notes show 56 sliced surfaces, 6 cases of hackmarks, 1 abraded bone, and 4 battered specimens. There were in addition a number of charred bones, 2 so heavily burnt that I thought it unlikely that grassfire could be responsible. There were also some large bones that had apparently been deliberately flaked while "green" in such ways that carnivore gnawing as responsible agency was out of the question. Those were not counted; we relied on Aguirre to study them (which he did, in a chapter in the forthcoming monograph). Klein saw the collections only after they were jacketed/warehoused/preserved, when it was much harder or impossible to differentiate fresh damage from ancient modification, and so he quite properly excluded several by then "dubious" cases from his accounts. Nonetheless, he recorded 22 bones as potentially cut, 4 charred (possibly naturally), and 10 from which flakes had apparently been struck in the "green" state. In fact, the disagreement between Klein's figures and mine is really not serious, considering what had happened to the collections between the excavation and the time he saw them.

In the 1980 excavation in the Ambrona Lower Unit, I found that about 50 percent of the larger bones bore marks suggestive of cultural alteration. A selection of pieces from both sites is illustrated. Figure 6.3 shows an immense elephant left innominate with subparallel grooves attesting extensive scraping. Figure 6.4 shows hacking and slicing on the premaxilla of an elephant skull. In Figure 6.5, an elephant mandible whose ascending ramus was removed, by repeated chopping with a wedge-shaped edge, is shown. Details of the hacking are illustrated in Figure 6.6. The remainder of the ramus was found just behind the mandible (it can be seen in the first photograph), and bore matching scars (Fig. 6.7). Despite the evident surface corrosion on these pieces, the marks are still easily identifiable, and in no case do they seem explicable by carnivore activity. None of these pieces would have been replicated by Shipman: their surfaces are too corroded and the marks they bear are not the sort she studies. Three apparently sliced specimens from Torralba are shown in Figures 6.8 to 6.10 and a hacked bone from the same site in Figure 6.11. Only space limits



■ FIGURE 6.3. *Large elephant innominate, showing marks of scraping (Amb 80)*

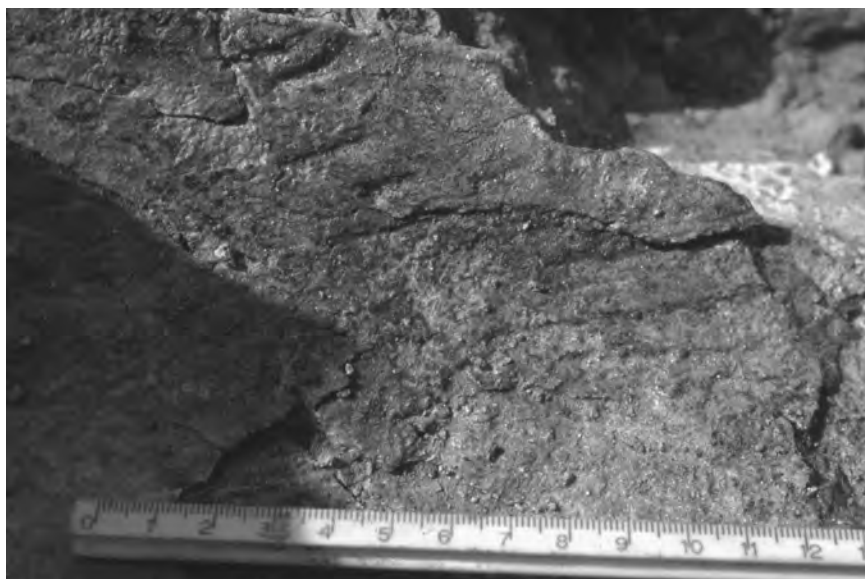
keep me from illustrating a score of other altered bones, including skulls, scapulae, ribs, innominates, and longbones.

Spatial Associations

The discovery of items in close juxtaposition in an archeological level has traditionally been seen as evidence that there is a real relationship between them. While some apparent spatial associations that are detected by eye are misleading, at least in the absence of statistical demonstration that the associations are unlikely to have arisen by chance, other visual associations are quite valid. No one would believe that an association of the bones of the skeleton of a single individual (such as the focal association in the part of Occupation 7 at Torralba) needs statistical validation. Nor is that an isolated instance.

The separate accumulations of elephant tusks in the Ambrona Lower Unit are statistically significant associations. So are the repeated concentrations of bovine horncores and bifaces in squares G15, G12, H12, and I3 in Torralba's Occupation 3 (Fig. 6.12).

Still other associations seem so unlikely that even though their probability cannot be directly determined because each is almost unique, their nature and number still persuasively suggest a direct relationship between the animal bones and the implements found at these two sites. In Torralba Occupation 1 we found one particularly striking case: square J12 held an elephant pelvis with a convergent denticulate tucked inside the acetabulum; a limestone battered polyhedron lay just outside the



■ FIGURE 6.4. *Elephant skull, hacked on premaxilla (Amb 80)*



■ FIGURE 6.5. *Elephant mandible with marks of hacking (to remove ramus?) (Amb 80)*

socket (Fig. 6.13). In I9 in the same level, a flint utilized flake lay atop an elephant right pyramidal. In Level 7 a small flint biface lay next to an elephant radio-ulna in square M12. At Ambrona, in the Lower Unit we repeatedly found bifaces right beside



■ FIGURE 6.6. *Details of hacking, visible despite corrosion, on mandible (Amb 80)*

tusks (Square G99: Fig. 6.14) or elephant vertebrae (STE 4: 2 vertebrae with two handaxes; Fig. 6.15). Other cases are too numerous to mention. The sheer number of such finds and their coherence with results of the statistical study of frequency relationships (see later discussion) cannot fail to impress a reasonable analyst.

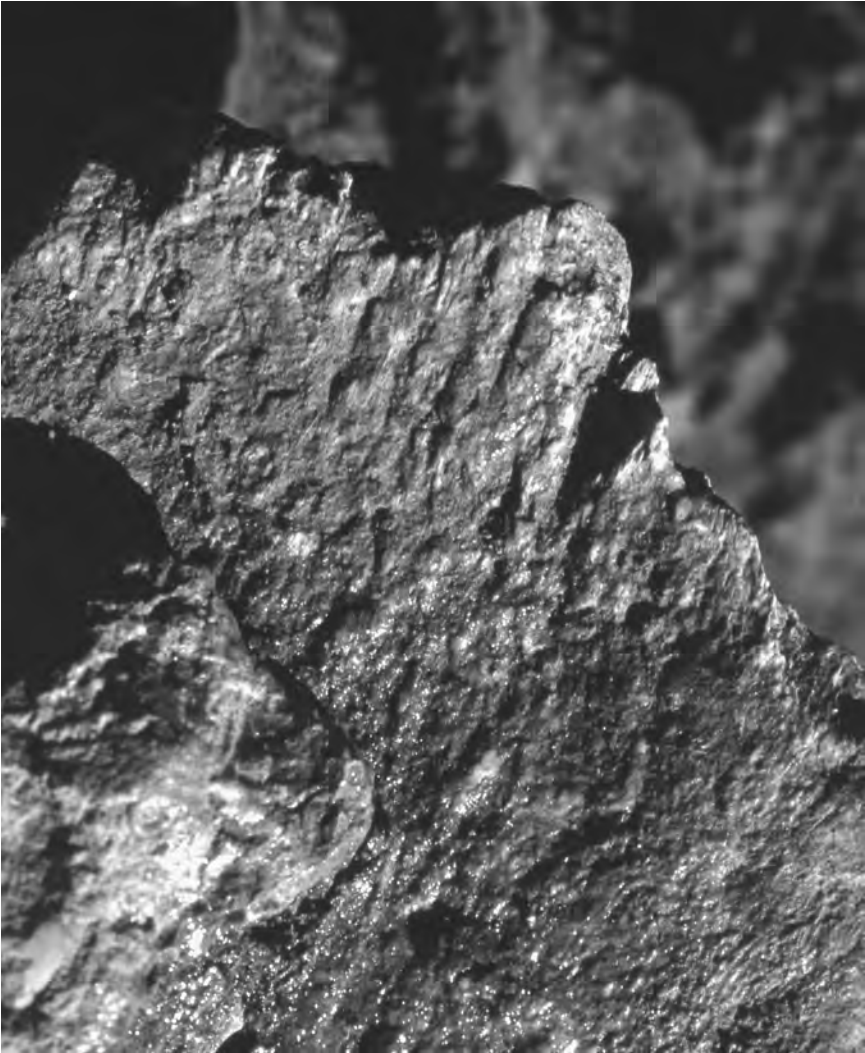
Statistical Analyses

If this were not enough, there is more abundant—and, to my mind, more convincing—evidence that there is a meaningful, culturally mediated relationship between the remains of large animals found at these sites and the artifacts left there by humans. That is the evidence provided by multivariate statistical analyses of relationships between these different kinds of data, analyses that have been carried furthest (and criticized most) at Torralba.

There is only one problem with the results of statistical testing. Most people still really don't understand the tests or their results, and so they will either reject the whole process as less meaningful than the solid, tangible "real" data an excavator digs up, or—even worse—will uncritically accept any and all statistical manipulations as valid, only to reject each in turn in favor of the latest test claimed to have produced contradictory results. It is an unfortunate problem, but one that eventually must vanish with education. In fact, used properly and evaluated critically, statistical tests are just so many more among the tools—knives, brushes, and so on—that excavators use to gather data, and their results are just as real and meaningful as any finds made with those tools. Just as one can pick the wrong tool for excavation, using shovels where trowels are called for, so one can use an inappropriate statistical procedure. Not all statistical procedures are equally justifiable. Just as one can excavate badly, producing erroneous information, one can also use statistics inappropriately to produce wrong or misleading information. Not all statistical results are equally reliable.

The use of any statistical test requires that the data to be analyzed be error free, that if the data must be transformed it be done in a justifiable and appropriate way that will neither invalidate the calculations nor hinder interpretation, and that the measures chosen be suited to the kind of data being studied. The tests chosen here produce measures of bivariate relationship—a matrix of correlation coefficients, in this case—and then use those measures as a basis for further computation. Any measure of the strength of a relationship between two variables should remain the same whether or not a third variable is present; that is not the case for some measures, but it is for the coefficients used here. Some variables are unrepresented in some samples. The problem of zeros in the data was handled by treating them as missing values and deleting any pairwise comparison where a zero occurred. Sometimes statistical software packages perform a multivariate test in different ways, producing different solutions from the same data. Obviously, that is undesirable: it ought to be the case that any analyst, using the same data and the same tests, should get the same results. The tests we used are fully replicable.

Table 6.2 lists the more abundant artifact types and MNIs for the major species represented in the Torralba occupations. Since use of edge counts in a previous work



■ FIGURE 6.7. *Ascending ramus of same mandible with matching hack marks*

(Freeman 1978) drew criticism and, more important, caused confusion, the artifact counts used here are of *whole tools* tabulated by level.

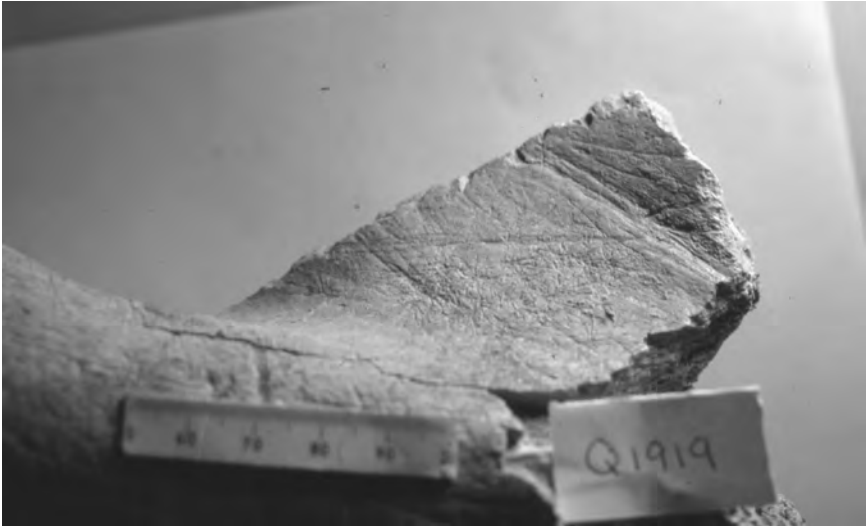
Whether edge counts or whole tool counts are used, significant patterned relationships appear in the data. The solutions are not identical. Multiple tools often combine different kinds of edges, but where the particular combination is not abundant enough to be considered a significant “new” type, they are placed into the type of the best-made edge. There are inevitably differences between solutions based on edge counts (which I still consider more meaningful) and those based on whole tool



■ FIGURE 6.8. *Torralba bone showing marks of slicing*

counts, but the differences are less important than the fact that significant patterning is detected no matter which data are used.

The counts were ranked and used to calculate the rank-order correlation coefficient, Spearman's rho. Rho works with ranks of frequencies, rather than the raw frequencies themselves, and ranking is far and away the most mathematically defensible transformation for these data, where it is inappropriate to make assumptions

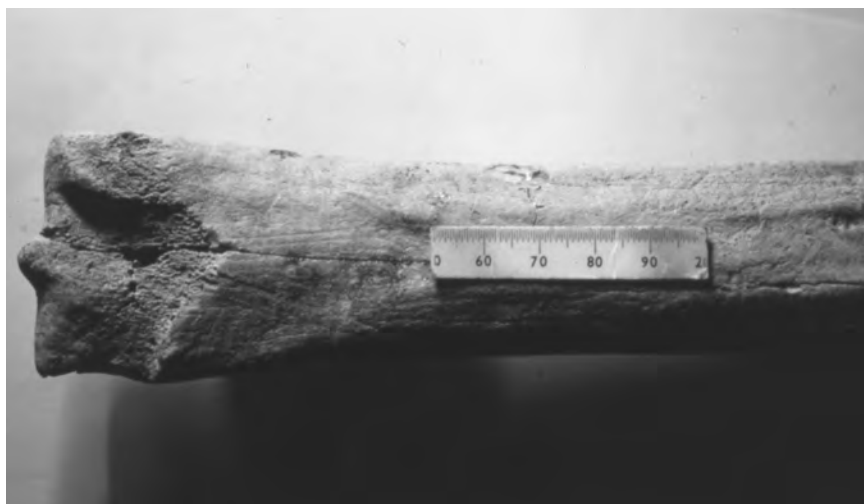


■ FIGURE 6.9. *Torralba* bone showing apparent slicing

about the underlying shape of the data distributions. In the past, I have transformed frequencies to square roots and used the more common bivariate correlation coefficient, Pearson's r (with larger data sets the transformation had essentially no effect on results); the results were slightly different from those presented here, but both tests coincide in showing relationship between similar sets of variables. There are no significant patterns of replacement or inverse relationship in the data.

The rank-order correlation coefficients in Table 6.3 were used as measures of nearness in a cluster analysis (Fig. 6.16). The structure of variability was simple enough so as to be discernable in most of its details on visual inspection of the coefficient matrix, but the dendrogram, based on a single-linkage procedure, shows its characteristics more clearly. Two data categories are really unlike the rest: congelifragments and bovid MNIs. Since the number of cattle is nearly invariant, this is as one would expect. The fact that congelifragments are unlike other data categories is reassuring. The animal species remaining are in fact related to each other, and also to particular stone tool types. Notches and denticulates stand apart from the rest of the tools, but choppers are related in frequency to equids and cervids, while bifaces, end- and sidescrapers, and cores relate more closely to elephant counts. That is not to say that elephants, cervids, and equids are unrelated to each other—all, and the other tools, form an interrelated group at a more distant level.

This simple test indicates beyond any doubt that there are meaningful relationships between the abundance of particular stone tools and the abundance of particular animal species. That simply would not be the case if it were true as some allege that the human presence at Torralba was essentially unrelated to the presence of animals at the site. But the tests are not in every respect satisfactory explorations of the



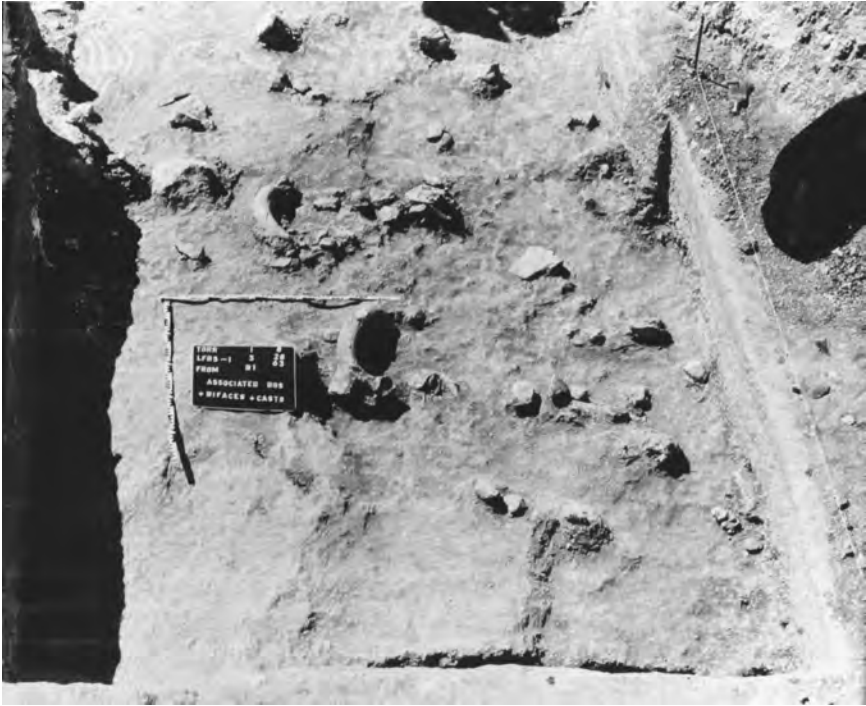
■ FIGURE 6.10. *Torralba bone showing apparent slicing and battering*



■ FIGURE 6.11. *Fragment of “chopped” elephant bone from Torralba*

data. Bovid MNIs, as noted, are small and nearly invariant. There are fewer cervids and equids than one would ideally prefer, and there are a lot of tied ranks. In these respects, the correlation matrix and cluster procedure, though certainly conclusive, leave something to be desired.

When body part counts are used rather than counts of MNIs both counts and variability in the faunal categories increase. The picture of relationships is both strengthened and clarified as details are added. At the same time, new dimensions of variability appear that are not adequately depicted in the essentially two-dimensional



■ FIGURE 6.12. Association of bifaces and horncores, Occ. 3, Torralba

cluster analysis; fortunately, the related but more elegant principal components analysis can show these relationships quite well.

In the following test I have used the stone artifact frequencies from the previous table, dropped the MNIs, and added the body part counts shown in Table 6.4. Differences in frequency are not so great that raw frequencies and Pearson's r could not have been used, and it might very well have been appropriate to do so, but since nothing is known of the nature of the underlying distribution of these data, to be safe the same nonparametric measure of correlation, Spearman's rho, was chosen.

The matrix of bivariate rank-order correlations is given in Table 6.5. Binford (1987) claims his analyses show that the Torralba deposits show a palimpsest of two major patterns: one in which bovid, equid, and cervid remains were deposited with tools while elephant remains were deposited in unrelated fashion; the other in which elephant bones were deposited in association with stone tools, but in which bones were broken into unidentifiable bits by forces other than human agency (1987: 66). More detailed examination of frequency relationships including body parts leads him to identify one pattern as potentially due to hominids, only to reject that possibility in the following terms: "No matter how we interpret the patterning, the case for 'activity areas' is very hard to sustain. The elephant carcass material is inversely related to remains of other species, making it difficult to argue that the differences in



■ FIGURE 6.13. *Elephant pelvis, with denticulate inside acetabulum; polyhedron nearby, Torralba*

tools represent tools appropriate to sequential processing steps in the butchering of a single animal” (Binford 1987: 90). In his detailed “analysis,” in fact, he claims to find in the Torralba data an inverse relationship between frequencies of a kind of pseudo “Mousterian of Acheulean Tradition” tool set, including especially bifaces, notches, and denticulates, on the one hand, and those of waste on the other (1987: 55); an association of scrapers and choppers—which he also wrongly calls “corescrapers or core axes” (1987: 77); elephants varying inversely with other animals (1987: 83, 85, 91); and an association between bifaces, sidescrapers, and elephant remains that he explains away as partly related to the paucity of those tool types (sidescrapers are, on the contrary, the next most abundant flake tool category—whether whole tools or edges are counted—in the collection and only five fewer in number than denticulates), and partly due to the fact that the sidescrapper counts are elevated because they are “compound edged tools” (1987: 89). While it is true that counts of working edges were used instead of whole tool counts for most flake tool types in the study (Freeman 1978) that was the source of data reanalyzed by Binford, patterned relationships between sidescrapers, other tools, and bones appear just as clearly when whole tool counts are used, as the present study shows. One could go on to contest other “results” of Binford’s “analysis” in detail, but it is pointless. No matter what

one feels about the logical coherence of his explanations of patterning (there, too, I find much that is questionable), the statistical results on which the arguments are based are worthless, since he used erroneous data, unjustified and unnecessarily convoluted data transformations, and inappropriate analytical procedures—no one whose hand was not guided by Binford could repeat his test and obtain the same results.

In fact, a simple inspection of the correlation matrices in this chapter is enough to show that Binford's claims are wrong. Aside from the association of notches and denticulates, which is only part of a more heterogeneous group he defines, not a single one of his claimed relationships has any validity—a very unfortunate state of affairs, since his results have been uncritically accepted at face value by Villa (1990, 1991—though she interprets them differently) among others.

True, pairs of items that cluster in Binford's solution sometimes also cluster in mine, but not in the ways or for the reasons he specifies, and there is no evidence of any substantial "inverse relationship" between variables. The numerous inverse relationships that so preoccupy Binford are in fact mathematical fictions. They occur neither in a correctly calculated matrix of correlation coefficients—product-moment or rank-order—nor are they at all numerous in an appropriate matrix of component loadings. Any real inverse relationship between variables has to be reflected in one increasing as the other decreases—producing at least a partial inversion of their numbers or rank orders—and that must result in a significant negative correlation. This simply is very unusual in the Torralba data: only one of the small number of negative coefficients (notches vs. elephant feet) reaches significance at the .05 level. It doesn't even happen when the erroneously copied data Binford presents are analyzed correctly.

I can only explain the large number of negative loadings in Binford's tables by assuming that either his "chi-square" transforms were inappropriately calculated from percentages (I suspect this may be the case, since Binford has been so fond of percentages in the past), or that he has presented an incomplete solution, which, had he allowed the test to continue to iterate until it reached a unique solution, would have eliminated the negative loadings. (There may be other mathematical explanations for his results, but no one could isolate them from Binford's almost deliberately obtuse procedural description.) Whatever the case, the statistical procedure is—has to be—invalid, as one can determine just by inspection of his data tables.

Our table of rotated factor loadings (Table 6.6) shows that seven factors or components are adequate to account for over 92 percent of the variance in the matrix of correlation coefficients. As is my usual practice, I rotated one more component (as a possible "error component") than the number with eigenvalues of 1.0 or greater. The last component does not principally determine variation in any variable; that is as one would hope. The seventh component only loads highly on geologically crushed pieces. That is also an encouraging sign.

Such tests as these are most justifiable when applied to data about whose structure there are some prior expectations. We had some idea beforehand what the statistical results at Torralba might show. Field observations of spatial associations sug-



■ FIGURE 6.14. Associated *biface and elephant tusk, Ambrona*



■ FIGURE 6.15. Association of vertebrae and handaxes, Ambrona

gested that cores and scrapers should each be related to elephant tusk, ribs, limbs, vertebrae, scapula, and pelvis (these two were combined in the statistical test); that bifaces and perforators should be related to elephant skull; that denticulates and notches were related (a pattern also incidentally found by Binford); and that both bifaces and denticulates were related to bovid skull (but bovid skull was too infrequent to be used alone in the test). Cervid metapodials and bovid and elephant foot bones were suspected to be related to waste and minimally utilized pieces, but waste flakes often occurred near skull fragments. (Note that expectations would be different for Ambrona, where other spatial associations were observed with greater frequency.) In my previously published analysis based on Pearson's r , several of these associations were confirmed statistically.

In this test, using a less powerful but more justifiable measure of association, and a slightly different set of data, with fewer collapsed categories, fewer correspondences occur, but the general picture remains the same.

The first and largest tendency for variation is associated very strongly with sidescrapers, elephant teeth, tusks, limbs, ribs, feet and vertebrae, scapula/pelvis, and equid teeth and feet, and less strongly with cores, elephant skull, equid skull, and bovid limbs. The second is still less strongly associated with variance in bifaces and endscrapers but strongly determines variation in cervid antler. The third is highly associated with cervid limbs, less strongly with cervid skull, and less still with equid limbs, while equid skull and endscrapers show moderate negative loadings on this factor—that suggests simply that we may be sampling different aspects of the “cultural landscape” in each level, and that the places of discovery of these latter items

■ TABLE 6.2. *Torralba major data categories by level (lithics are whole pieces; taxa are Aguirre's MNIs)*

	<i>Bifaces</i>	<i>Choppers</i>	<i>Cores</i>	<i>Waste</i>	<i>Sidescr</i>
OCC 1	3	5	7	27	13
OCC 1C	1	2	0	2	0
OCC 1D	2	1	2	9	0
OCC 2	2	3	1	28	2
OCC 2A	2	0	0	3	0
OCC 3	10	3	5	27	7
OCC 3A	1	0	1	10	0
OCC 4	0	2	2	25	3
OCC 5	2	0	1	10	1
OCC 7	8	2	4	32	8
OCC 8	3	2	3	93	3
	<i>Endscr</i>	<i>Perf</i>	<i>Notch</i>	<i>Dentic</i>	<i>Congel</i>
OCC 1	4	2	3	6	6
OCC 1C	0	0	0	2	0
OCC 1D	1	2	3	4	1
OCC 2	2	0	4	3	1
OCC 2A	0	0	0	1	2
OCC 3	2	2	4	10	10
OCC 3A	1	0	1	4	3
OCC 4	0	2	0	5	3
OCC 5	0	1	1	2	5
OCC 7	3	7	2	3	7
OCC 8	1	3	1	2	1
	<i>Elephas</i>	<i>Equus</i>	<i>Bos</i>	<i>Cervus</i>	
OCC 1	5	3	1	3	
OCC 1C	1	1	1	1	
OCC 1D	2	1	1	1	
OCC 2	2	1	1	1	
OCC 2A	1	1	1	0	
OCC 3	3	2	2	1	
OCC 3A	1	1	0	0	
OCC 4	3	2	1	1	
OCC 5	1	1	1	1	
OCC 7	6	1	1	2	
OCC 8	2	2	1	2	

are different from those of the former (a fact that has no evident geological explanation). Perforators, “waste,” and elephant skull fragments are strongly associated with Factor 4. The fifth tendency for variation strongly determines variation in choppers and horse scapula/pelvis, “explaining” to a smaller degree variation in bovid limbs. Notches and denticulates are found alone to be determined by Factor 6.

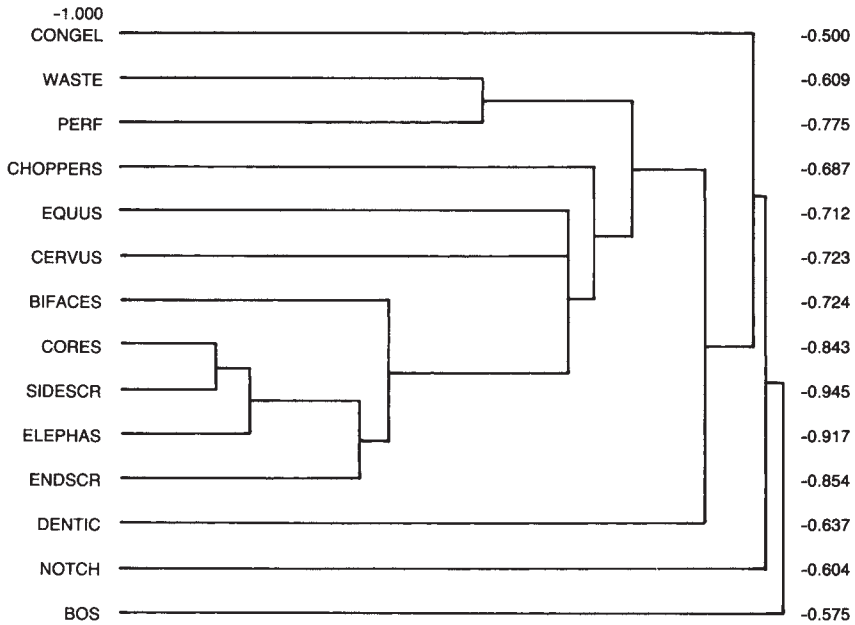
Using Pearson’s *r* (results not shown), the number of meaningful factors isolated was 7. The associations detected remained essentially similar, but one factor

■ TABLE 6.3. *Torralba artifacts and species matrix of Spearman Rank Correlation Coefficients (RHO) (zeros treated as missing data; pairs with any zero member eliminated)*

	<i>Bifaces</i>	<i>Choppers</i>	<i>Cores</i>	<i>Waste</i>	<i>Sidescr</i>
<i>Bifaces</i>	1.000				
<i>Choppers</i>	0.333	1.000			
<i>Cores</i>	0.843	0.472	1.000		
<i>Waste</i>	0.687	0.276	0.429	1.000	
<i>Sidescr</i>	0.677	0.313	0.945	0.309	1.000
<i>Endscr</i>	0.543	0.712	0.670	0.255	0.821
<i>Perf</i>	0.532	-0.216	0.437	0.775	0.462
<i>Notch</i>	0.363	0.485	0.342	0.025	0.265
<i>Dentic</i>	0.375	0.452	0.513	0.257	0.633
<i>Congel</i>	0.578	0.408	0.561	0.133	0.609
<i>Elephas</i>	0.842	0.356	0.825	0.679	0.917
<i>Equus</i>	0.642	0.539	0.723	0.460	0.584
<i>Bos</i>	0.575	0.354	0.417	0.116	0.206
<i>Cervus</i>	0.489	0.310	0.682	0.615	0.724
	<i>Endscr</i>	<i>Perf</i>	<i>Notch</i>	<i>Dentic</i>	<i>Congel</i>
<i>Endscr</i>	1.000				
<i>Perf</i>	0.000	1.000			
<i>Notch</i>	0.423	-0.094	1.000		
<i>Dentic</i>	0.295	-0.139	0.604	1.000	
<i>Congel</i>	0.602	0.030	0.177	0.475	1.000
<i>Elephas</i>	0.854	0.572	0.497	0.637	0.491
<i>Equus</i>	0.376	0.021	0.193	0.573	0.292
<i>Bos</i>	0.000	-0.113	0.525	0.530	0.557
<i>Cervus</i>	0.556	0.560	-0.338	0.051	0.211
	<i>Elephas</i>	<i>Equus</i>	<i>Bos</i>	<i>Cervus</i>	
<i>Elephas</i>	1.000				
<i>Equus</i>	0.585	1.000			
<i>Bos</i>	0.239	0.332	1.000		
<i>Cervus</i>	0.582	0.490	-0.245	1.000	

determined most variation in both waste and equid limbs, and elephant foot bones and cervid limbs were found related to another.

The results of the principal components analysis indicate that there are in fact patterned relationships between stone artifact types and particular animal body parts, for all the major species represented at Torralba. They are nontrivial: a trivial association would be, for example, a single tendency that determined variation in all the variables, which would indicate that sample size was the only operative variable. They do not bear out Villa’s (1990: 304) claim that at Torralba people butchered “elephant carcass leftovers” rather than whole carcasses—all major elephant body parts are involved in these patterned relationships. The statistical tests demonstrate that the human presence and the animal remains really cannot be independent of each other. In some cases, at least, they correspond to spatial associations viewed during



■ FIGURE 6.16. Dendrogram of relationships between artifact types and animal species, Torralba; single lineage method; distance measure = Spearman's rho

the course of excavation, and their contents could not have been acted on similarly by natural depositional agencies other than humans, so that no simple explanation of site formation processes that excludes human agency can adequately account for them.

When the results of the level-by-level statistical analysis are evaluated in light of all other evidence from Torralba, the most economical way to account for the presence of the different components, the distinctive clusters of variables associated with each, and the fact that different clusters were frequently found in different areas is to ascribe them largely to the organization of human activities. That is not to say that all materials from Torralba reflect human behavior, for there are many kinds of data that were not included in the tests, and some that were not adequately explained in terms of the factors isolated. Nor is it to claim that there has been no natural disturbance of the original patterns in the residues. Despite these processes, however, a picture of human activity emerges among the other pictures reflected in the Torralba finds.

Binford's dubious statistical procedures and errors and his mistranscriptions—perhaps better, “manipulations”—of artifact and bone counts from the site have misled readers about the nature and composition of the Torralba assemblages, and about the relationships between data categories. As Howell noted in a review in the *Journal of Human Evolution* (1989), 14.3 percent (10) of the 70 cells in the matrix

■ TABLE 6.4. *Torralba body parts count by level*

	<i>Elth</i>	<i>Eltsk</i>	<i>Elskl</i>	<i>Elrib</i>	<i>Ellmb</i>	<i>Elfet</i>	<i>Elscpl</i>	<i>Elvrt</i>	<i>Eqth</i>	<i>Eqskl</i>
OCC 1	16	30	4	29	42	11	17	33	32	5
OCC 1C	0	0	2	0	1	1	0	0	1	0
OCC 1D	0	3	0	0	1	0	0	0	0	0
OCC 2	2	1	0	2	1	1	1	1	5	0
OCC 2A	5	6	2	12	9	4	2	2	9	3
OCC 3	0	0	0	1	0	1	0	0	2	1
OCC 3A	8	14	10	17	24	11	3	11	21	2
OCC 4	4	4	4	5	4	3	1	6	19	3
OCC 5	0	5	1	3	4	0	1	3	2	1
OCC 7	10	18	12	27	23	18	10	22	23	4
OCC 8	5	6	0	4	3	0	2	6	22	2

	<i>Eqlmb</i>	<i>Eqfet</i>	<i>Eqscpl</i>	<i>Bolmb</i>	<i>Crvnt</i>	<i>Crskl</i>	<i>Crmb</i>
OCC 1	14	7	11	9	15	2	2
OCC 1C	0	0	0	0	0	0	0
OCC 1D	1	1	1	1	0	0	2
OCC 2	0	1	0	0	0	0	1
OCC 2A	6	4	8	0	4	1	0
OCC 3	0	0	0	2	0	0	0
OCC 3A	13	6	9	5	2	2	3
OCC 4	4	2	4	0	3	1	1
OCC 5	7	1	1	2	5	1	0
OCC 7	7	6	2	3	5	1	1
OCC 8	18	4	7	6	8	2	4

Binford supposedly compiled from my earlier published figures are wrong: even had he used identical tests, he would therefore have obtained different results from mine. The situation is aggravated by questionable transformations of the data—over-interpretation of mathematical results that are not statistically significant to begin with, and the use of analytical procedures that I defy anyone (other than Binford) to understand or replicate. There are perfectly appropriate ways of transforming the data for his purposes—simply ranking the raw counts and using a rank-order correlation procedure as has been done here is the simplest and probably the best, while square root or log transformations of all the data and the use of Pearson’s *r* are probably also defensible in this case—and when error-free data, transformed appropriately, are used as input to ordinary principal components analysis and rotation (or related multivariate tests whose results are free of operator bias and equally insensitive to the order of data entry), the results obtained are the ones I have published here and elsewhere, not those Binford presents.

■ CONCLUSIONS

I hope that I have presented enough information regarding Howell’s work at Torralba and Ambrona in the 1960s and later to indicate the significance of those investiga-

■ TABLE 6.5. Matrix of Spearman correlation coefficients—ranked data

	<i>Bifaces</i>	<i>Choppers</i>	<i>Cores</i>	<i>Waste</i>	<i>Sidescr</i>
Bifaces	1.000				
Choppers	0.333	1.000			
Cores	0.843	0.472	1.000		
Waste	0.687	0.276	0.429	1.000	
Sidescr	0.677	0.313	0.945	0.309	1.000
Endscr	0.543	0.712	0.670	0.255	0.821
Perf	0.532	-0.216	0.437	0.775	0.462
Notch	0.363	0.485	0.342	0.025	0.265
Dentic	0.375	0.452	0.513	0.257	0.633
Congel	0.578	0.408	0.561	0.133	0.609
Elteeth	0.448	0.224	0.754	0.036	0.975
Eltusk	0.462	0.334	0.626	0.210	0.783
Elskull	0.358	0.000	0.263	0.679	0.632
Elribs	-0.074	-0.000	0.317	-0.084	0.595
Ellimbs	0.322	0.429	0.373	0.194	0.721
Elfeet	0.143	-0.098	0.179	0.253	0.718
Elscpel	0.467	0.344	0.731	0.185	0.893
Elverts	0.505	0.229	0.767	0.277	0.941
Equteeth	0.378	0.179	0.479	0.563	0.709
Equskull	0.056	0.224	0.459	0.178	0.735
Equlimbs	0.321	0.671	0.339	0.657	0.263
Equf feet	0.384	0.462	0.646	0.262	0.971
Equsc pel	-0.019	0.894	0.385	0.139	0.667
Boslimbs	0.064	0.667	0.373	0.606	0.616
Cervantl	0.702	0.775	0.750	0.609	0.526
Cerskull	-0.101	0.577	0.198	0.364	0.444
Cerlimbs	-0.258	-0.131	0.048	-0.019	0.287
	<i>Endscr</i>	<i>Perf</i>	<i>Notch</i>	<i>Dentic</i>	<i>Congel</i>
Endscr	1.000				
Perf	0.000	1.000			
Notch	0.423	-0.094	1.000		
Dentic	0.295	-0.139	0.604	1.000	
Congel	0.602	0.030	0.177	0.475	1.000
Elteeth	0.667	0.105	-0.051	0.327	0.761
Eltusk	0.577	0.395	-0.281	0.140	0.701
Elskull	-0.500	1.000	0.211	0.578	0.426
Elribs	0.500	0.395	-0.356	0.025	0.168
Ellimbs	0.462	0.092	-0.330	0.326	0.794
Elfeet	0.379	0.775	-0.811	0.000	0.266
Elscpel	0.667	0.500	0.154	0.329	0.528
Elverts	0.667	0.500	0.030	0.636	0.715
Equteeth	0.500	0.585	-0.104	0.253	0.017
Equskull	0.684	0.462	0.156	0.061	0.012
Equlimbs	0.112	0.308	-0.344	-0.030	0.206
Equf feet	0.563	0.585	-0.137	0.290	0.572
Equsc pel	0.447	0.339	0.047	0.267	0.103

continued on next page

■ TABLE 6.5—continued

	<i>Endscr</i>	<i>Perf</i>	<i>Notch</i>	<i>Dentic</i>	<i>Congel</i>
Cervantl	0.632	0.105	0.574	0.045	0.236
Cerskull	-0.275	0.148	-0.000	0.364	-0.218
Cerlimbs	-0.625	-0.059	-0.727	-0.162	-0.314
	<i>Elteeth</i>	<i>Eltusk</i>	<i>Elskull</i>	<i>Elribs</i>	<i>Ellimbs</i>
Elteeth	1.000				
Eltusk	1.000	1.000			
Elskull	0.410	0.522	1.000		
Elribs	0.937	0.910	0.667	1.000	
Ellimbs	0.901	0.903	0.615	0.934	1.000
Elfeet	0.899	0.899	0.821	0.927	0.881
Elscpel	0.991	0.976	0.603	0.908	0.827
Elverts	0.918	0.849	0.696	0.850	0.777
Equeteeth	0.883	0.826	0.782	0.854	0.714
Equskull	0.567	0.606	0.456	0.885	0.596
Equlimbs	0.493	0.711	0.265	0.162	0.428
Equfeet	0.991	0.944	0.647	0.946	0.879
Equscpel	0.406	0.687	0.232	0.607	0.717
Boslimbs	0.200	0.771	0.200	0.754	0.657
Cervantl	0.493	0.418	-0.176	0.126	-0.009
Cerskull	0.396	0.510	0.315	0.289	0.291
Cerlimbs	0.273	0.243	-0.056	0.030	0.160
	<i>Elfeet</i>	<i>Elscpel</i>	<i>Elverts</i>	<i>Equeteeth</i>	<i>Equskull</i>
Elfeet	1.000				
Elscpel	0.882	1.000			
Elverts	0.841	0.821	1.000		
Equeteeth	0.908	0.872	0.886	1.000	
Equskull	0.647	0.676	0.606	0.793	1.000
Equlimbs	0.667	0.523	0.473	0.595	-0.073
Equfeet	0.868	0.975	0.860	0.897	0.676
Equscpel	-0.051	0.600	0.378	0.464	0.400
Boslimbs-	0.316	0.600	0.600	0.824	0.806
Cervantl	0.410	0.376	0.327	0.523	0.312
Cerskull	0.296	0.514	0.510	0.577	0.000
Cerlimbs	0.287	0.277	0.154	0.334	-0.632
	<i>Equlimbs</i>	<i>Equfeet</i>	<i>Equscpel</i>	<i>Boslimbs</i>	<i>Cervantl</i>
Equlimbs	1.000				
Equfeet	0.634	1.000			
Equscpel	0.572	0.805	1.000		
Boslimbs	0.928	0.794	0.928	1.000	
Cervantl	0.600	0.266	0.054	0.667	1.000
Cerskull	0.874	0.588	0.722	0.866	0.291
Cerlimbs	0.647	0.276	0.441	0.410	0.154
	<i>Cerskull</i>	<i>Cerlimbs</i>			
Cerskull	1.000				
Cerlimbs	0.889	1.000			

■ TABLE 6.6. *PC analysis with rotation Torralba artifacts and body parts*

I) LATENT ROOTS (EIGEN-VALUES)

1	2	3	4	5	6	7	8
13.576	4.308	3.484	2.718	2.158	1.973	1.295	0.893
9	10	11	12	13	14	15	16
0.589	0.327	0.289	0.209	0.079	0.047	0.010	0.000

II) ROTATED LOADINGS

	1	2	3	4	5	6	7	8
Bifaces	0.337	0.571	-0.133	0.459	-0.102	0.222	0.476	-0.272
Choppers	0.244	0.437	-0.018	-0.093	0.844	0.325	0.195	0.157
Cores	0.607	0.501	-0.025	0.145	0.050	0.301	0.190	-0.489
Waste	0.185	0.423	0.070	0.864	0.199	0.060	0.072	0.156
Sidescr	0.909	0.195	0.022	0.081	0.040	0.323	0.049	-0.357
Endscr	0.587	0.561	-0.602	-0.284	0.236	0.062	0.146	-0.132
Perf	0.430	-0.063	-0.067	0.920	-0.085	-0.152	-0.148	-0.256
Notch	-0.147	0.343	-0.375	0.013	0.130	0.886	-0.083	-0.174
Dentic	0.308	-0.043	0.049	0.035	0.052	0.913	0.222	0.079
Congel	0.519	0.065	-0.221	-0.048	-0.003	0.219	0.860	0.020
Elteeth	1.005	0.172	0.096	-0.244	-0.181	0.074	0.191	-0.022
Eltusk	0.960	0.085	0.085	0.029	0.175	-0.181	0.193	0.007
Elskull	0.605	-0.420	0.055	0.765	-0.160	0.366	0.018	0.174
Elribs	0.965	-0.270	-0.164	-0.083	0.042	-0.198	-0.287	0.076
Ellimbs	0.902	-0.244	-0.042	-0.050	0.249	-0.066	0.333	0.211
Elfeet	0.922	-0.002	0.102	0.233	-0.355	-0.377	-0.027	0.327
Elscpel	0.969	0.095	0.055	0.032	0.057	0.101	-0.006	-0.128
Elverts	0.940	0.078	0.048	0.115	-0.068	0.248	0.102	0.027
Equeteeth	0.890	0.179	0.123	0.287	0.005	0.009	-0.347	0.170
Equskull	0.728	0.027	-0.613	0.098	0.153	-0.014	-0.493	0.030
Equlimbs	0.445	0.387	0.591	0.286	0.436	-0.242	0.165	0.282
Equfeet	0.981	-0.040	0.090	0.119	0.251	-0.014	0.052	-0.111
Equscpel	0.537	-0.168	0.181	-0.020	0.875	0.074	-0.074	-0.281
Boslimbs	0.612	0.216	0.188	0.220	0.733	-0.221	-0.340	0.093
Cervantl	0.325	1.006	0.046	0.081	0.138	0.086	-0.082	0.053
Cerskull	0.403	0.051	0.797	0.093	0.450	0.228	-0.281	0.135
Cerlimbs	0.183	-0.030	1.054	-0.106	0.037	-0.245	-0.089	-0.133

tions once and for all, and to lay the less well-founded criticisms of our work to rest. I do not imply that our interpretations—specifically, my own—have always been impeccable and infallible. They have certainly not. In my earlier work, I seriously misjudged the extent of cultural elaboration expectable in a mid-Pleistocene site and underestimated the difficulties in unraveling what cultural information there is from the overlay of other processes—geological, mechanical, chemical, and biological—that may embed and hide it. Nor have I always expressed myself as well as I could have done. Excavations in 1963, conducted under my guidance, while good enough, could nonetheless have been better; I paid too little attention in the 1960s to marks of gnawing or to marks of butchery; it is probably my own fault that no one

knows that screens were used at Torralba; I should undoubtedly have indicated more clearly that we knew that carnivores were present at the sites, or that we had taken geological processes such as slopewash and channel flow into consideration; I never stressed enough that our statistical tests only included some of the Torralba data, or that some of the tested variables were not adequately explained. Of course there are animal remains at Torralba and Ambrona that were not manipulated by humans, or even evident to them. That should also have been made clearer.

Questions about the causes of patterning in these residues are not simple black-and-white issues. It is irresponsible and nonscientific to decide that either all the patterning detected must result from human cultural agency or none of it can. There is patterning due to nonhuman agency at the two sites. At the same time, a substantial basis for cultural interpretation can still be recognized at both. The archeological record of hominid activity at Torralba and Ambrona is not pristine and free from pre- and postdepositional distortions. However, even if these obscure the message, they do not obliterate it entirely, and enough remains to tell at least part of a story of hominid-animal interactions at Torralba and Ambrona.

No single kind of evidence tells the whole story. The sediments and fauna pose questions that must be answered with conjoinable stone tools, wear-polishes, skeletal dispersal, and patterned regularities discerned statistically. No single line of evidence—sinking in clayey deposits, MNIs, age distributions, tooth-marks, stone tools—tells its own story unambiguously. To decipher what Binford has called the “palimpsest” of Torralba requires assembling and comparing all these multitudinous kinds of information and trying to reconcile each with the rest. But when that is done, the outline of a message about human adaptation appears, behind other messages, it is true, but nevertheless still legible.

I hope that these observations will help in some small way to clarify the importance and potential of Torralba and Ambrona and to secure for them the recognition they deserve. It is unfortunately true that we still know all too little about hominid adaptations of the mid-Pleistocene. That is so in spite of the number of new mid-Pleistocene sites that have been discovered and carefully excavated since the 1960s. Despite the high quality of excavations at sites like Aridos and Isernia, much more information will be needed before any satisfactory idea of the nature of mid-Pleistocene adaptations in any region can be derived. Each site we now know is like an irreplaceable piece of a huge and variable picture puzzle most of whose pieces are missing. Each site we know so far has proven to be unique in scale, in scope, and in quality of information; it would be absolutely senseless to discard or ignore any of the pieces we have so laboriously assembled, assuming that it is replicated by any other. The pattern on each and every piece is damaged or obscure—every other European site from this time range presents at least as many problems of interpretation as do Torralba and Ambrona. To progress, we must try to understand every piece in its own terms, and to see how each relates to all the rest.

In this interpretive process, Torralba and Ambrona will continue to play a large part. Few European mid-Pleistocene sites are nearly as informative as they. In the last analysis, that we owe to the vision, care, and scholarship of F. Clark Howell.

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Were There Scavengers at Torralba?

Healthy debate about the hunting capacity of Lower and Middle Paleolithic foraging peoples continues as strongly now as it did more than two decades ago (González Echegaray and Freeman 1998). The multilevel sister sites of Torralba (Fig. 7.1) and Ambrona (Fig. 7.2) in the province of Soria on the high Spanish Meseta, excavated since the 1960s under the direction of F. Clark Howell (the second more recently re-investigated by M. Santonja), have been prominent among European Acheulean foci of this discussion, and are probably familiar to most readers because their abundant faunas contain several individual elephants of a very large mid-Pleistocene species, *Elephas (Paleoloxodon) antiquus*, although other large herbivores such as horses, rhinos, and wild oxen are also present, and in the case of the horse, are as numerous as elephants in some levels (see Howell et al. 1992; Freeman 1994; González Echegaray and Freeman 1998 for recent reviews of these sites and additional bibliography).

The evidence that the human presence at Torralba and at Ambrona is related to the presence of the animals, and that humans actually manipulated the animal remains at both sites, is quite convincing. Preservation is excellent for a mid-Pleistocene site, and several kinds of evidence converge to support that conclusion. Occupations—or, if you prefer, episodes of utilization of the site and deposition of tools and bones—are multiple, rapid, short-term accumulations, sometimes subject



■ FIGURE 7.1. Overview (from north) of the Torralba excavations (1962); checkerboard of three-meter square excavation units visible in right foreground

to some disturbance or reworking; but usually that disturbance has reoriented materials without destroying their associations. These are not simply lag deposits: that transport of bones and artifacts was apparently not extensive for most levels is shown by the lack of spatial separation of refit lithics and the near-articulated positions of some skeletal elements. While the deposits suggest relatively rapid though sporadic accumulation of the archeological materials, each of the levels distinguished contains the bones of several individual animals of different species, whose carcasses were apparently all in utilizable condition at essentially “the same time”—that is, they could have all been processed at once, and sometimes at least the evidence suggests that to have been the case (body parts of different animals are intermingled, tools or flakes apparently from the same core are found with different animals). Where they can be determined for single levels, age profiles are characteristic of catastrophic accumulations, and the only appropriate catastrophe, given all else that we know of the environment, was pretty certainly human-related. Stone tools (and shaped wooden ones, recovered mostly as plaster casts incorporating patches of charred wood) occur right among the animal remains, and in several cases are intimately juxtaposed to bones in tight physical association. The body parts of some species, including the elephants, have been rearranged selectively in ways that cannot be explained by geological processes or the behavior of the animals themselves (Fig. 7.3). Many of the bones bear gross macroscopic traces of deliberate flaking (Figs. 7.4, 7.5), hacking (Fig. 7.6), and abrasion; some show microscopic traces that have been interpreted as



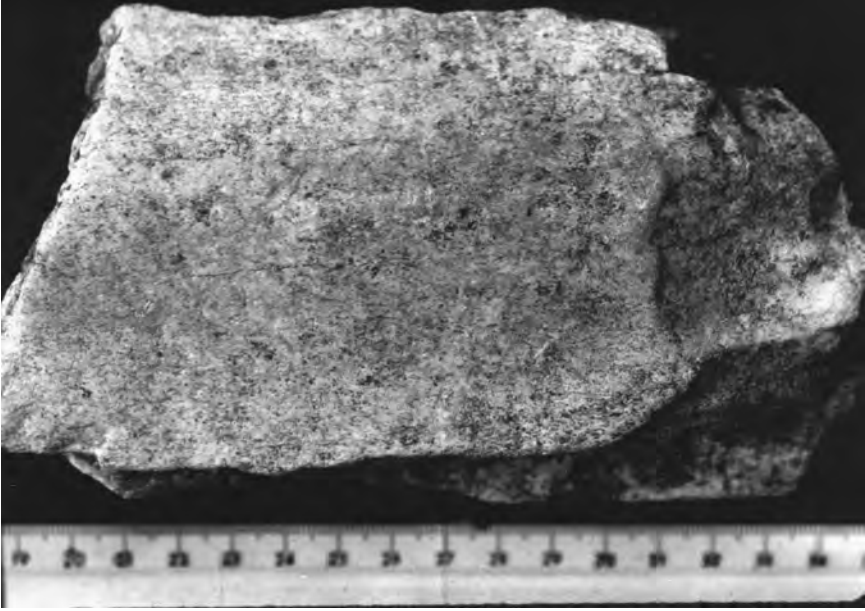
■ FIGURE 7.2. *Ambrona, overview (from north) of area opened in 1981; standing walls separate excavated three-meter squares; on-site museum at right*

characteristic of slicing with sharp-edged implements; and others are partly charred. The nature of the artifacts supports the suggestion of hunting and/or butchering: cleavers, handaxes, scraping tools, and sharp-edged slicing implements are present among the stone tools, and the wooden implements include one meter-long shaft with a sharp wooden point, that in any other Acheulean site would be interpreted unquestioningly as a spear (Fig. 7.7). Statistical tests provide evidence of consistent relationships between artifacts and all the major animal species represented, and certain kinds of tools and specific body parts are regularly related in abundance in ways that cannot be explained except as the result of cultural choice.

Despite this accumulation of evidence, some scholars, principal among them L. R. Binford, have denied that the animals and the stone tools at these two sites have anything to do with each other. To these critics, the faunal remains are the result of long periods of accumulation of the bones of individuals that died natural deaths from age, disease, carnivore predation, or local disaster, and the artifacts attest the ephemeral presence of hominids passing through the landscape for unrelated purposes. On occasion, of course, the critics admit that these hominids made use of the meat they could scavenge from animals recently dead of natural causes, but they assert that ability to hunt, immobilize, and kill these huge beasts was beyond their limited cultural (technological and organizational) capacity. In fact (although this caricatures their positions) the advocates of a “scavenging phase” of hominid food procurement seem to take the oversimple position that all dietary needs for meat protein were satisfied by scavenging, while those on the other side in the debate



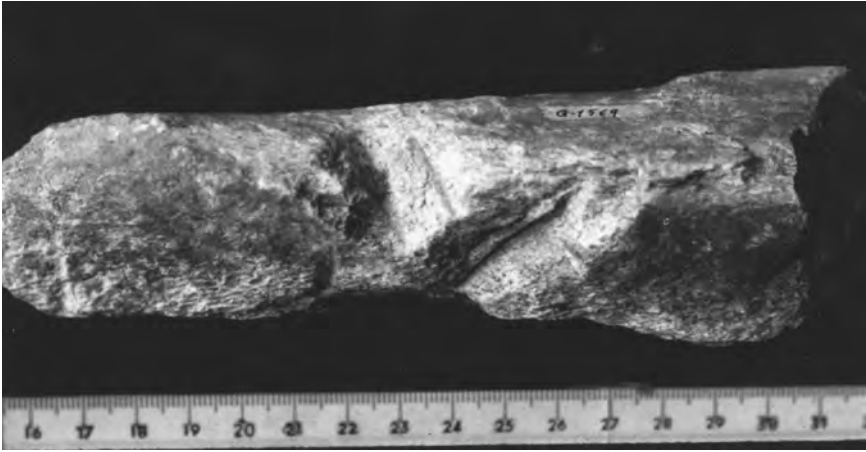
■ FIGURE 7.3. Bone alignment in Ambrona Lower Unit, Area I (1963); elephant femora and tusk, with perpendicular radio-ulna



■ FIGURE 7.4. *Flaked elephant bone from Torralba*



■ FIGURE 7.5. *Flaked juvenile tusk tip from Ambrona (possible point)*

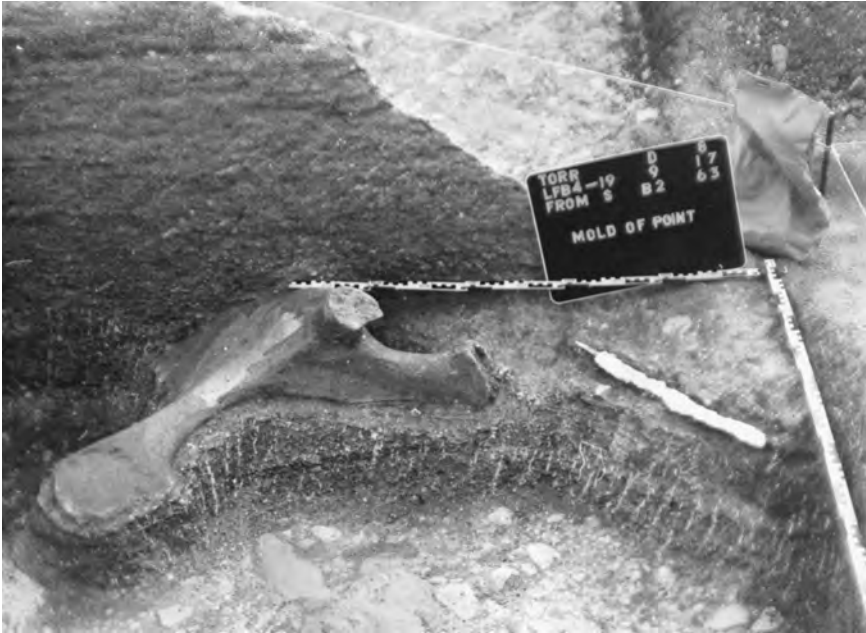


■ FIGURE 7.6. *Hacked elephant long bone from Torralba*

seem to take the equally unrealistic stand that no meat was ever scavenged from naturally dead carcasses.

I find both polarized assertions untenable. In this chapter I continue to insist that the patterns of evidence from Torralba and Ambrona convincingly document animal butchery. But I will go further to assert that while Lower and Middle Paleolithic hominid foragers in mid-latitude Europe may have scavenged (and probably did so) from freshly dead carcasses whenever the opportunity presented itself, scavenging alone could not have provided the regular supplies of digestible animal food that were absolutely required for survival. Consequently, from the time of the earliest hominid presence in such latitudes, foragers had to actively seek, catch or immobilize, and dispatch the prey on which they fed. I have become convinced of this position after listening to decades of debate, and I insist that though it is moderate it is not really a middle ground: the idea of a purely “scavenger phase” of hominid foraging for Lower and Middle Paleolithic peoples in mid-latitude Europe is, I firmly believe, simply untenable. Of course, I could be wrong. But my reading of the literature (though I certainly do not claim to know it all) does not convince me that I am. Please understand that I make no claim to present anything like a comprehensive review of the relevant literature, nor do I pretend to firsthand familiarity with the relevant African analogues; I hope to learn a great deal in that respect from the presentations of colleagues such as Profs. Fisher and Frison, who have that personal experience.

The difficulty of demonstrating the “hunting” position is manifest. None of the evidence from Torralba or Ambrona that I have cited above—or from any other mid-Pleistocene site I know—can be used to *prove* hunting, unless perhaps the spearpoint is thought to do so. Some other European Acheulean sites have yielded wooden spears: Lehringen and Schöningen (Thieme 1996, 1997) are among the sites where such pieces were recovered. Another kind of evidence, perhaps the best for hunting



■ FIGURE 7.7. Plaster cast of wooden point, with associated elephant pelvis, from Torralba

to date, comes from Boxgrove, where Mark Roberts reports finding a spear wound in a horse scapula, in a context that includes other evidence suggestive of human hunting (Roberts 1996). Even that case, which seems quite compelling, might conceivably be challenged, claiming it resulted from the use of a sharpened lever to pry muscles and bone apart during the butchering process, rather than from weapon use. But conclusive evidence of hunting is difficult to obtain throughout prehistory, even in sites where no one has ever doubted that bone accumulations resulted from deliberate hunts. Most hunted animals die of poison, infection, internal bleeding, or destruction of vital organs. Only rarely does a weapon point become embedded in bone. Due to the decay of soft body parts (and at least the shafts of most hunting weapons), it is very difficult to prove that any butchered carcass from a Paleolithic site was actually killed by humans. The evidence for hunting remains mostly indirect. Such factors as an accumulation of carcasses of animals of several species, with individuals from all active age ranges, in approximately their expectable proportions in living herds, might under certain circumstances provide evidence of hunting. (Such “catastrophic” distributions can, of course, also be produced by natural die-offs, whose causes would first have to be excluded.)

In short, it is hard to imagine that *direct* evidence of hunting as a deliberate and consistent cultural pattern, of the sort that would silence all objection, will ever be obtained from any mid-Pleistocene site. Much of the debate about hunting or scavenging must continue to be based on indirect evidence, on theoretical considerations,

and on the study of living creatures. However, not everything is possible. There is evidence in the anatomy and physiology of modern humans, the skeletal anatomy of our earlier ancestors, the behavior of our close primate relatives, and the nature of past environments that constrains the possible food-procurement alternatives that must have been available to mid-Pleistocene foragers.

■ HOMINID DIETARY REQUIREMENTS: THE EVIDENCE OF ANATOMY AND PHYSIOLOGY

Our anatomy (and that of our hominid ancestors, as far as the skeletal evidence goes) reflects a dietary pattern that includes the ingestion of meat. In the anatomy of their dentition and digestive apparatus, humans today are adapted to be omnivores—to digest meat as well as certain kinds of plant foods. To judge from the fossil record, hominids have been omnivores for the last two and a half to three million years at least. While we and our ancestors lack the anatomical equipment of specialized carnivores, we have long been facultative carnivores—able to use meat as a dietary staple, supplemented by other foods, whenever it became available.

Protein from meat has many dietary advantages: it builds muscle rapidly, and is a high-quality source of energy; weight for weight, it takes less meat to provide a given amount of usable protein than it does nuts, seeds, or the other good vegetal protein sources. If prey stomach contents and organ meats are eaten, a hominid can survive completely without the need for vegetal foods, in most environments. A strict vegetarian must spend much more time and energy foraging to satisfy dietary requirements than does an omnivore who balances vegetal intake with meat protein. In these respects, a totally meatless diet even if practical would be an inferior diet for hominids. Since the 1950s many paleoanthropologists have recognized that the consumption of meat was an important part of the adaptive niche developed and occupied by the ancestors of modern *Homo sapiens sapiens*.

In fact, a strict vegetarian diet is not simply impractical, but impossible, for foraging groups of *Homo sapiens*. Without modern pharmaceuticals, modern humans (and there is no reason to believe that their close mid-Pleistocene ancestors and relatives were significantly different in this regard) must ingest regular quantities of animal food in order to survive. We require regular supplies of trace quantities of cobalamine (vitamin B-12) which is unavailable (at least in adequate quantity) from any wild vegetable source (see Berkow 1992—it may, however, be possible to get needed quantities from some cultivated legumes). Unless this substance is regularly ingested, anemia and relatively rapid death ensue. Modern strict vegetarians, even those who use legumes as a dietary base, must acquire cobalamine (in vitamin supplements, for example) or they develop what medicine knows as “vegan anemia.”

Ingesting carrion is a risky way to meet this dietary need. It is no accident that most specialist scavengers have digestive systems that tolerate toxins produced during the decay of meat. Bacteria present in the ground or in the guts and on the skins of animals spread rapidly as a carcass decays, and the decay process is faster in warmer climates. Bacterial metabolism may be associated with the production of

toxic chemical by-products. Hominids lack the scavenger's physiological defenses against many of these spoilage microorganisms. Consequently, meat that has decayed beyond the initial stages is not food but poison to us, and there is no reason to believe that such was not the case for our earlier hominid relatives as well. Our ability to acquire resistance to such meat-spoilage bacteria or carcass contaminants as *Streptococcus*, *Staphylococcus*, *Salmonella*, *Shigella*, *Escherichia*, and *Clostridium botulinum* (which can be ingested from contaminated carrion) is limited and in some cases nonexistent (see Burrows 1963). Once ingested, some of these bacteria, or their by-products, remain in the system for weeks, and contact even with resistant individuals will infect others. We also know that ingestion of some animal parts, such as brains or offal, risks exposure to viral infections like *kuru* or such sicknesses as mad cow disease (a fact that may be less important to earlier hominid evolution, since death or debilitation from the resulting disease may take quite a long time).

How did early hominids obtain animal food, once its consumption entered their survival strategies? Aside from trade with or raiding of their neighbors for food, or outright cannibalism, before domestication there were only three ways. One, of course, is the acquisition of meat by scavenging from carcasses abandoned by predators, who had either fed to satiation or been driven away from their kills. The second is the deliberate hunting of mobile animals. These views are often polarized, their proponents suggesting either that all meat protein consumed was obtained by hunting or that, at least for the first millions of years of hominid history, none of it was. This polarization is unnecessary and unreasonable. In the last analysis all would agree that hominids obtained some meat protein in yet a third way: by catching and eating small, relatively immobile animal resources such as insects and larvae, eggs and fledging birds, snails, tortoises, newborn mammals, and in general any creature weak enough or slow enough that it could not escape. The debate about hunting really centers on the hominid ability to take larger, fleetier prey. L. R. Binford (1981, 1983, 1984, 1985, 1987) has been a vocal exponent of the viewpoint that well into the Upper Pleistocene hominids were unable to hunt large game. However, his seems an extreme position.

■ BEHAVIORAL EVIDENCE FROM LIVING PRIMATES

Primate ethology suggests that facultative hunting, even of relatively large animals, is not beyond the range of behaviors we might expect from an early hominid. There are relevant data from the behavior of living, free-ranging primates, who require no language, no very complex organizational skills, nor any sophisticated technology to capture and kill other animals. By now, everyone knows that our closest primate ancestors, the chimpanzees, are facultative hunters who by cooperative action are able to capture and devour such quick-moving creatures as monkeys or young antelope. Among chimps and baboons, both of whom are facultative hunters and apparently enjoy meat protein, the extent of scavenging is apparently small.

According to an early study (on baboons, certainly less capable than chimpanzees) by S. W. Washburn:

all of the information that we have on primate hunting . . . suggests that these animals will take eggs, young birds, and other small, living creatures, but that they do not scavenge. The evidence shows that hunting small, easily captured prey is far simpler and more nearly universal than is scavenging. Besides, scavenging from large carcasses when carnivores are nearby can be exceedingly difficult. . . . It is said that it is easy to drive a lion from its kill, but this is only true in areas where lions are accustomed to being hunted and are trained to stay away from man.

Finally, scavenged meat is a rare occurrence by comparison with meat that is easy to hunt. In an area in Africa in which there are many lions, there are still hundreds of baboon per lion; so that if the baboons were to try to live on scavenged meat, they would have a hard time getting an ounce of meat per day per baboon. The most minimal hunting of easily killed animals is more rewarding than this. (Washburn 1968: 342)

Carrion in edible condition is a rare commodity, as anyone who does much hiking in the wilderness will realize. Even road kill, much more abundant along U.S. highways than carnivore kills, would not provide enough accessible food to support a small group of human foragers. Barring a cataclysm, live animals are always more abundant than recent kills.

Craig Stanford (1995: 261) observed that chimps in the Gombe reserve show little interest in scavenging. Geza Teleki also studied chimpanzee hunting there, and with the exception of stealing bushbuck carcasses from baboons, seconds or at most minutes after they were captured—the chimps had often observed (and been excited by) the hunt in progress—he noted no examples of true scavenging. In fact, in experiments, chimps would not treat carcasses of species they normally preyed on, offered them by the experimenter, as food. He comments: “in view of these observations I am puzzled that the original notion of a scavenger phase in hominid evolution has recently regained popularity among scholars concerned with the hominid fossil record. . . . Evidence to the contrary has been appearing regularly over the same span of years . . . but with little apparent effect” (Teleki 1981: 329).

■ BEHAVIOR OF NON-PRIMATE SCAVENGERS

Other observations of relevance come from the study of non-primate mammals who are known as efficient scavengers. The spotted hyena is probably Africa’s best-known (and best-adapted) mammalian scavenger. Hans Kruuk spent three and a half years studying their behavior in Serengeti National Park and the Ngorongoro Crater. He found that even in the Serengeti, where the proportion of scavenging was relatively high, 68 percent of animals eaten by hyenas were hunted and killed by hyenas. The reason that the importance of hyena hunting had not previously been observed is that hyenas are nocturnal hunters (Kruuk 1972: 111). Hyenas lurking about lions feeding from a carcass were in fact often not scavengers awaiting their chance at the meat, as would popularly be thought, but hunters themselves that the lions had driven from their own kills. Jackals and wild dogs, too, kill much of the meat they feed on, though each will also scavenge, as will lions. In fact, in Africa each of the

major carnivores kills or scavenges depending on opportunity (Schaller and Lowther 1969: 325).

Most dedicated scavengers have developed special abilities that facilitate the utilization of kills made by other animals. They locate carrion using highly developed senses. The eyesight of buzzards and other flying carrion-eaters is extremely acute. Terrestrial scavengers often rely on especially sharp olfaction. All specialized scavengers move rapidly about a great deal of territory in their search for food. In contrast, hominids are not particularly mobile, and neither they nor any of their closest relatives possess the visual or olfactory acuity of a competent scavenger.

Hominids are, and to judge from our relatives, the apes, probably always were, more intelligent and their behavior more flexible than the social carnivores. It is relatively easy for higher primates—as for social carnivores—to hunt and kill small animals, or the young or incapacitated individuals of medium-sized species. It is much harder and more dangerous for them to drive large predators away from their kills. While today it is said not to be particularly difficult for hunters to drive large predators from their kills, *Homo sapiens sapiens* is considerably better equipped in intelligence, foresight, weaponry, social communication, and cooperation than were our earlier ancestors. Early hominid scavenging would almost certainly have had to be passive scavenging—waiting until a carcass was abandoned by its predators, rather than driving them off.

■ THE AVAILABILITY OF CARRION: THE SERENGETI

One must consider other lines of evidence before deciding on the relative likelihood of a scavenging adaptation. Studies of the availability of edible meat from scavengable carcasses have produced other data of importance to this question.

Schaller and Lowther (1969: 325–30) reported the results of a brief study undertaken in the Serengeti, during the dry season. In two transects, one across the plains, the other in woodlands, they found the remains of several carcasses, some neonates, and two sick or crippled animals. They concluded that while meat-eaters might survive over the short term by killing and eating disabled beasts as well as feeding on carrion from predator kills, scavenging alone would not provide a sufficiently regular and predictable food supply for survival over the long run. For long-term survival, a carnivorous hominid group would have had to combine scavenging with the killing of incapacitated or very young animals.

The suggestive study of carcass availability (in the same general region) undertaken by R. Blumenschine (1987) extends these results. He found that the potential of the Serengeti for scavenging from predator kills depended on the nature, size, and density of herbivore species; the nature of locally abundant carnivores; whether the area is riparian forest (where water is generally available, lions abound, and spotted hyenas are rare) or open grassland (with dry-season water holes); the time of year; and the extent of competition among predators for meat.

One of the principal predators in his study, the spotted hyena (*Crocuta crocuta*), can crush open and chew long bones and skulls of middle-sized animals, digesting

the bones as well as their contents, and leaving little for the scavenger. All carnivores except the spotted hyena soon stripped the flesh from kills but left long bones and skulls intact. Where there were few spotted hyenas, or whenever long bones and skulls were abandoned whole, a scavenger who could smash the bones would find a good source of protein therein. As one might expect, the carcasses of larger animals by and large provide more potentially usable food for scavengers than do smaller animals. In the southern plains of the study area, where large, migratory herds were present in the dry season, seasonal scarcity of water forced herbivores to congregate around waterholes. This could result in a surplus of food for predators: even spotted hyenas, who usually leave nothing for other scavengers, under those circumstances would sometimes satiate themselves and abandon carcasses that still had meat on them. Usable carcasses of medium-sized animals (the size of adult wildebeests or zebras) would have been relatively more abundant at those times; according to Blumenschine, even if most meat had been stripped from carcasses, intact marrow bones and skull contents would nevertheless be more available for a scavenger in such conditions.

In sites such as Olduvai, on the Plio-Pleistocene boundary, Blumenschine estimates that large herbivore biomass should have been greater than it is today. Speculating that saber-tooth cats, supposed to be less complete consumers of large carcasses, might have been relatively common during the Early Paleolithic in riparian woodlands (where hyenas are comparatively rare) led him to suggest that marrow bones and meat on large carcasses would probably have been even more readily available at such times and places. So Blumenschine sees considerable potential in that environmental setting for an early hominid opting to get animal protein by scavenging.

In a later paper (1989), Cavallo and Blumenschine report that tree-stored leopard kills would add to the meat available for hominid exploitation, though only prey smaller than antelope-sized would persist for more than about an hour (a pattern unlike that of carcasses on the ground). While he tells us that carcasses of larger animals on the ground persisted for several hours or up to as many as four days, small animals usually being devoured in minutes or hours, Blumenschine provides no information on the rapidity of putrefaction, or the toxicity of tissues at various periods after death. These are essential questions about the time carcasses would provide tissues in digestible condition. Nor are absolute quantities of usable tissue reported. Just how much reliance could be placed on scavenged meat protein as a dietary mainstay in the Serengeti situation remains unclear. And, useful as Blumenschine's paper is for modeling hominid behavior in one sub-Saharan African region at the Plio-Pleistocene boundary, none of his observations is directly applicable to the Spanish case.

Selvaggio (1998a) reports that spotted hyenas cache parts of the carcasses of animals they have obtained in the shallows of Lake Macat (in the Ngorongoro crater) and may abandon them for as much as one or two days; she observed that the meat appeared, at least on superficial observation, to remain fresh during that time, and would have been available to scavenging hominids. She rightly suggests that hominid

scavenging of meat or bone from such caches could account for the accumulations of cutmarked bones at some African Plio-Pleistocene sites. However, because of the size discrepancy of the animals from Selvaggio's caches and those from Torralba/Ambrona, hyena caching does not seem to be a viable explanation for the bulk of those Spanish accumulations.

■ CARRION AVAILABILITY IN MID-LATITUDE EUROPE

The Spanish Meseta, where most Acheulean sites (such as Torralba and Ambrona) are found, is a mosaic of ecosystems having little in common with the Serengeti (or the Ngorongoro crater). There are today some 30 species of ungulates in the Serengeti, including many antelopes, while there were no more than a third this total number in mid-Pleistocene Spain. Large herbivores in both areas include(d) elephants and rhinos. Authors differ in their estimates of animal populations, but all counts agree that animals are extremely abundant in the Serengeti. There, in moist savanna, large herbivore biomass may run from 8,000 to 10,000 kilograms per square kilometer (Delaney and Happold 1979: table 11.14)—biomass for all herbivores is substantially greater—and in woodlands runs perhaps a fifth as high. Again, when all herbivores are considered, the biomass is larger, rising to some 5,000 kilograms per square kilometer (Hendrichs 1970). (As one might expect, these average values fluctuate with the seasons, and there are longer-term fluctuations as well.) When migratory ungulates are present on the Serengeti plains, there may be 220 of them per square kilometer, while in woodlands, the density is less than a tenth of that (Houston 1979: 268). There are about a half dozen middle-sized to large carnivores. Their numbers, according to the best survey, are relatively high—there may be 7,000 of them in the reserve as a whole, and biomass for the five largest predators is 14–16 kilograms per square kilometer (Schaller 1972: 454). (Schaller, incidentally, notes that adult rhino and elephant are too large to be manageable prey for lions.) The number of available carcasses for use by scavengers has been estimated at one per 33 square kilometers in plains areas in the wet season—this drops to one per 300 square kilometers in the dry season—and one per 412 square kilometers in the woodlands (Houston 1979: 268). Although these averages do not take into account dry-season aggregation around waterholes, they do not suggest that life as an exclusively scavenging hominid with dietary reliance on a regular supply of meat would have been particularly easy.

In Europe, in contrast, ungulate biomass was always substantially smaller. Though precise estimates are impossible due to human interference with ecosystems, figures on the order of 500 to 1,000 kilograms per square kilometer for mixed woodlands and 3,500–5,000 kilograms per square kilometer for mid-latitude grasslands seem as large as is reasonable (see, e.g., Bourlière 1964). In the European case, there were at any period about as many species of large carnivores as in the Serengeti; among them lions, wolves, bears, and hyenas (and earlier, saber-teeth) were the principal figures. But their numbers were a small fraction of those in the African area. By the time sites like Torralba and Aridos were occupied, saber-teeth were either

very rare or completely absent from the Spanish landscape. No contemporary (mid-Pleistocene) carnivore in Spain was large enough to attack adult elephants of the size of the *E. antiquus* or the rhinos from Torralba and Ambrona (nor is hyenid water-caching of body parts of these large animals a serious possibility).

Seasonal scarcity of water (or moisture in the form of snow) was not a limiting factor for large mammals. The lean season would have been the winter, as it is today. Winter cold, deep snow, scarcity of edible vegetation, and difficulty traveling imply reduced carrying capacity for herbivores, particularly during glacial phases. Climate was extremely severe, with much colder winters than at present, when Torralba and Ambrona were utilized. These critical factors limited the size of animal populations and the internal variety of communities. There were never as many large herbivores as there were in the Serengeti, nor as many different kinds of them. Limiting herbivore populations restricts the number of local predators they can support. Consequently, fresh carcasses of animals killed by carnivores must always have been rarer, harder to find, and further between than in the African case. If kills were scattered more sparsely over the landscape, hominids would have had to travel considerably farther to find carcasses than in the Serengeti case. The density of usable carcasses could not have been more than a tenth to a fifth as great in Spain. It is highly unlikely that specialized scavenging could have been a viable adaptive strategy in these environmental conditions.

The winter scarcity of plant food would have affected hominids as much or more than it did true vegetarians—hominids cannot digest grasses, whether fresh or dried as hay, as can a specialized herbivore. Since hominids do not hibernate, eating was a yearlong necessity. Food storage is one possible way out of this dilemma, but there is absolutely no evidence that storage of any kind was practiced until much later in the Spanish Paleolithic. There are no potential storage facilities in any Spanish Acheulean site. While pits have been found in rare Mousterian excavations, there is not the slightest evidence that they were used for long-term food storage (or that they were used to store anything edible other than meat). Under the circumstances, meat protein would have been a dietary necessity, and if it could not regularly be obtained by scavenging it must have been procured by hunting. If there ever was a scavenging phase of human subsistence, it seems probable that it had long vanished before the colonization of mid-Pleistocene Europe.

In short, while the simple answer to the question posed by the title to this chapter is yes, there were almost certainly scavengers at Torralba (and Ambrona), I suggest that the animals found in these two sites must almost inevitably include many that were deliberately killed by mid-Pleistocene human hunters.

POSSIBLE HUNTING METHODS

At one time it was very generally accepted that early hominids, camped in places like Olduvai Gorge, obtained some animal food by killing tortoises and other small animals including neonates and young of larger mammalian species. In fact, even though there has more recently been a tendency to explain the remains of these

animals as the result of death by natural causes, kills by hominids remain quite plausible, and well within the behavioral repertoire of our living higher primate relatives. On the other hand, the hunting of adults of such huge creatures as the Torralba/Ambroña elephants seems an impossibly daunting challenge for hominids with such rudimentary technologies as those indicated by the Acheulean evidence. Hunting elephants is not without danger to the modern gunman armed with a large-bore rifle; by what possible means could such large and powerful creatures have been immobilized and killed by our primitive ancestors? One suspects that such ethnocentric and *a priori* ruminations underlie much of the argument for a long-enduring scavenging phase of hominid evolution. To the contrary, however, the literature of exploration and reports of observations of African hunting methods suggest that big game was until recently being hunted successfully with very rudimentary equipment.

Techniques observed in use to capture game by skillful elephant-hunting groups without firearms included dropping heavily weighted spears from trees, spearing them repeatedly with small-diameter spears, stabbing them from below with large metal spears (in all these cases, the weapon point could be smeared with poison), hamstringing them with swords from horseback, cutting off their trunks, catching them in footsnare or pitfalls, and net hunting. We know of no pitfalls earlier than the Upper Paleolithic, however, and no technique requiring a metal point or blade was then available. Of this list, only the use of wooden spears, footsnare, and net-hunting remain as possibilities. But there is one other method that is widely reported and that would have been both possible, suitable, and devastating: the use of fire either to surround and burn animals or to drive them into situations, such as the mucky shallows of lakes, from which they could not readily escape. Sir Samuel Baker observed this technique before 1890, seeing large numbers of elephants and other animals driven into a narrowing ring of fire, which left them half suffocated by smoke, badly burnt, and often blinded. The technique was so effective in taking whole herds that Dr. G. Schweinfurth, who saw it employed by the Azande (1873), feared that its repeated use would lead to the extermination of the species (Cloudsley-Thompson 1967; R. Carrington 1962). Henry Stanley (1890: 339) reported seeing vast heaps of bones of slaughtered game in a circle some 300 yards in diameter on the shores of Lake Albert. The bones included remains of animals of many kinds, from elephants to bushbuck. He attributed this accumulation to the familiar practice of ringing the animals with fire. The use of fire drives has long seemed to me to be the most probable technique for trapping and immobilizing elephants along the shores of the Ambroña lagoon or the riverbanks of the Spanish Meseta. It would account for the simultaneous occurrence of bones of several other species in the accumulations better than any alternative I can envision.

■ DIRECTIONS FOR FUTURE RESEARCH

While I am as sure of my ground as I may be given the current state of knowledge, I am under no illusion that this essay or this symposium will resolve the hunting/scavenging debate to everyone's satisfaction. I hope that papers by other participants,

particularly Fisher, Frison, and West, will help clarify the issues. But I anticipate that much further research will be needed to arrive at any resolution. I believe that we can now see some of the directions that this research must take.

It is in my opinion fruitless to hope to find convincing evidence of hominid hunting in marks left on bones. If every bone recovered bore convincing butchery marks, that would still not indicate that humans had hunted the butchered animals. Even finding an apparent spear point embedded in bone would probably only result in arguments over the function of the artifact type; as we all know, most stone artifacts that could be used as spear points could equally well have been used as knives. The best associations between stone tools and animal remains will remain unsatisfactory as evidence for human hunting.

Before the study of Paleolithic butchery methods can progress, we must also clarify our thought about the study of butchering traces or any other kind of tool marks on mid-Pleistocene bone. Ancient bone isn't fresh bone; pristine and unaltered tool marks or other traces of working or utilization on mid-Pleistocene bone are not to be expected, and when they do occur are likely to be both rare and debatable.

We must also abandon the assumption, held by many Paleolithic prehistorians, that any indication whatever of carnivore involvement in altering a bone assemblage completely rules out any human agency. Wolves will scavenge carcasses hunted by humans today, and did so in the recent past. Carnivores fed on and gnawed remains of carcasses left behind after humans removed what they wanted from hunted game. Why then do some prehistorians/paleoanthropologists find it so hard to conceive that carnivores could have behaved in like fashion earlier in the Pleistocene? More subtly, we must recognize that while residues of carnivore behavior may sometimes mimic apparent results of human behavior so closely as to confuse or mislead us, it is equally true that patterned human behavior can mimic carnivore activity: for example, technology may limit the hunter, or cultural choice dictate that only very young or incapacitated animals be taken as prey.

The study of modern analogues remains the most productive single line of approach to a resolution of the hunting/scavenging debate, but in future, data gathering must be more systematic and more precisely controlled, and comparisons subjected to reasonable constraints. We must learn more about the abundance of large mammals, about the ways they may be hunted, about the behavior of predators and scavengers, and about what they leave behind in places like the African grasslands and forests where one can still observe elephants and other large mammals in interaction under largely naturalistic conditions. It is essential to complement observations of carcass availability with chemical and bacteriological analyses, to show that the apparently available carcasses would have been digestible and not harmful to hominids. We must also learn to avoid the Bushman pitfall: the fallacy of assuming that all elephants, environments, scavengers, or hunters must be identical to the one case we are familiar with or the one that has most recently been popularized. African analogues are never going to be a perfect fit to the European data, but as long as the differences are recognized and their effects correctly evaluated, the comparisons and contrasts we find will be increasingly enlightening.

Advancing our knowledge of the range, limits, and development of early hominid subsistence and environmental utilization will require the convergence of several different lines of evidence. Better paleoenvironmental data, refined calibration of the duration of accumulations, an increasingly scrupulous examination of artifacts and associations, more attention to details of site context, more careful calculation of minimum individual estimates and mortality profiles, and tighter control in the study and comparison of modern analogues to ensure their relevance are all needed. More research on the staging of hominid and carnivore alteration of bones, where both are present, is sorely needed. Recent work of this sort by Blumenschine and Selvaggio (1991; Selvaggio 1994, 1998b) is an excellent beginning, but only a beginning. None of these investigations will resolve the hunting/scavenging discussion, and some will only bear on it indirectly, but if all are taken together they will certainly provide a more accurate understanding of individual cases, and well-analyzed cases will lead in the aggregate to more realistic and reliable reconstructions of the socio-economic behavior of our early ancestors.

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P A R T

IV

THE MIDDLE PALEOLITHIC

The first of these three chapters treats the origins of the Mousterian and shows that well-excavated assemblages can and do intergrade. For that reason and others, the interpretation of the Mousterian facies as non-overlapping, mutually exclusive sets of related industries can no longer be maintained, nor can the idea that they were the stylistically distinctive products of separate, identity-conscious socio-cultural groups. In Chapter 9 I present some of the evidence suggesting that there are differences between Middle and Upper Paleolithic adaptations and speculate about their causes. Chapter 10 attempts to summarize still more evidence about those differences and to indicate some of the research errors we have committed in the past.

Among the techniques used to reach these conclusions was a pair of multivariate tests: the Kolmogorov-Smirnov two-sample test and a principal components analysis based on rank-order correlations, whose solution was subjected to Varimax rotation. My use of these techniques has been criticized for reasons suggesting that the critics lacked mathematical sophistication. One objected that the Kolmogorov-Smirnov test needed to “take account of sample size,” but that is something that is built into its formulas.

The idea that discrepancies in sample size can overdetermine correlation has led others to use a variety of means to eliminate these discrepancies, such as data

transformations, when the best such means has always been the use of rank-order correlation. I have used Spearman's r rather than the Pearson's r statistic for correlation, since the two coefficients are closely related and Spearman's coefficient is part of every major statistical package for home computers. As it happens, the concern is more theoretical than real, and results using discrepant sample sizes and Pearson's r are not that different from mine. (Having in fact used both techniques, I am sure of my ground.) Last, some people still fail to understand that rotation does not really alter the Principal Components solution in any way; it just spins its axes to a position that makes the solution easier to grasp without considerable effort.

Kaleidoscope or Tarnished Mirror?

THIRTY YEARS OF MOUSTERIAN INVESTIGATIONS IN CANTABRIA

Thirty years ago, I undertook my first independent Paleolithic research, on the nature of the Cantabrian Mousterian. Motivated by a desire to extend the “new systematics” of artifact and assemblage classification developed by the late François Bordes to an area outside France, I sought to determine whether or not the distinctive and seemingly nontemporal constellations of similar Mousterian assemblages or “facies” he recognized could be identified outside their type area, and to find causes or correlates of their variation. It seemed logical to select, for this kind of study, an area not too distant from southwest France, where the sequence of major environmental changes during the last Glacial might be expectably related in understandable ways to what had happened in France at comparable times, and where one might even hope for some continuities in populations and traditions on either side of the Pyrenees. Cantabria is an attractive theater for this kind of study. Its Paleolithic record rivaled that of southwest France, and large, well-curated Mousterian collections from sites like Castillo, el Pendo, and Cueva Morín provided a rich field for reanalysis.

Those first investigations led over the course of time to a complete reappraisal of the Cantabrian Mousterian and a much different understanding of the Mousterian in general. That reappraisal is to some extent reflected in excellent recent publications

by Cabrera (1983, 1989) and Cabrera and Bernaldo de Quirós (1992). Typological contributions have been made by Benito del Rey (1972–73, 1976), Cabrera (1989), and Santamaría (1984). Cantabrian Mousterian research has paralleled or stimulated work elsewhere in Iberia and the Pyrenees: recently, I. Baldeón (1974), Barandiarán (1973, 1979), Chauchat (1985), Villaverde (1984), Moure and Delibes (1972) have examined particular sites and collections, and Altuna (1989), and Butzer (1981) have studied environmental contexts from sediments, pollen, and/or faunal remains. I will not attempt to synthesize their work here, important though it is: Straus (1992) provides a recent review of northern Spanish developments, and Vega Toscano (1983) attempts a brief overview of the Mousterian in Spain as a whole, that may serve that purpose. Nor can I discuss evolving Mousterian adaptations to the changing environmental settings of the Late Pleistocene here. I aim only to present, for the first time, a very personalized historical narrative describing the course of our research on Cantabrian stone tool assemblages, and the successive stages of our interpretations from their beginnings to their present state.

A discussion of the development of Cantabrian Mousterian research and its results is most appropriate in a *Festschrift* dedicated to Joaquín González Echegaray, who has been from the outset a major participant in the investigations. Our conclusions have undergone successive modifications, that have unfortunately not always been appreciated by a new generation of students who lack firsthand familiarity with the Mousterian. Non-specialists often want opinions once crystallized to remain forever invariant.

Fortunately, our field inevitably evolves, and new and better understandings require that older interpretations be modified or abandoned in the course of time. My first involvement in these Mousterian investigations was a dissertation study of old museum collections, with all the defects of mixture and selection that such materials always entail, complicated by a tyro's naïveté. Conclusions based on old collections—particularly those involving facies attribution—have had to be altered as data from modern, well-controlled excavations have become available for study, new assemblages from Morín and el Pendo replacing their older unreliable counterparts. While our joint research was under way, Henry de Lumley's (1969–1971) work on Mousterian assemblages from the French Midi forced revision of the Typical Mousterian facies, and that too has had to be considered. Dibble and others (e.g., Dibble and Rolland 1992) have challenged traditional artifact typology. Each infusion of fresh data required reevaluation of the overall picture of Cantabrian Mousterian facies, only to lead, at last, to the rejection of the facies concept in its original form. As a result, the very nature of Mousterian research has itself changed. The study is not finished. Careful excavation of the Castillo Mousterian will certainly yield much new information, some of it surprising, in the not-too-distant future. All of these factors have made Cantabrian Mousterian research rather like a kaleidoscope of evolving interpretations.

When I began dissertation research, I hoped to identify the Bordes facies where they were present, define new ones as necessary, and try to learn the reasons for their existence: were facies distinctions the result of stylistic distinctions between different

synchronous local groups, as Bordes suggested, or were their differences mostly due to their economic and technological uses? Was some facies difference due to stylistic change over time, and was variation related to other entirely different causes? I hoped to learn what I could about the local antecedents of Mousterian industries, and to clarify the nature of the Mousterian–Upper Paleolithic transition. These last two goals were secondary to the major thrust of research, however: the identification and analysis of Mousterian facies.

The field was very exciting when I began research in Cantabria. In 1962, scholarship seemed on the brink of resolving the mysteries of the Mousterian worldwide. Bordes had systematically defined its numerous artifact types, and had decided that Mousterian collections in France fell into one or another of four broad, distinctive, apparently nontemporal groups he called “facies.” Bordes himself had classified some collections from other countries, including Italy and Spain (he identified what seemed to be the Charentian facies in Castillo Mousterian Beta, and defined a new complex he called the “Vasconian” for Castillo Mousterian Alpha). It nonetheless remained to be demonstrated that his facies classification was suitable and sufficient for the categorization of the Mousterian complex beyond southwest France.

Throughout, I was primarily interested in seeing how Cantabrian Mousterian materials would compare to the better-defined French sequence. That “Francocentric” orientation was normal: the French had developed paleolithic studies earlier and carried them further by the 1960s than had others, and French sequences and ideas were touchstones all Paleolithic prehistorians used. My training was partially French—I had studied Mousterian artifact classification under Bordes in his laboratory in Talence. Bordes was until his death the world’s leading authority on the Mousterian, who had virtually singlehandedly systematized the previously chaotic field of Mousterian studies. When I began “independent” research it was with the partial collections from Castillo then stored in the Institut de Paléontologie Humaine in Paris, and my classification at the IPH was guided at every step by Bordes and his colleague, Jacques Tixier.

My investigations were planned to carry on work in the Bordes tradition; they were a controlled application of his ideas and methods to a new area. But they also added something new. Jacques de Heinzelin (1960) had shown that descriptive statistics and graphic representation of multidimensional relationships could be used to refine artifact classification, providing a more objective and probabilistic basis for type definitions stated subjectively by Bordes. Without powerful computers, his work could provide no more than a few examples to show the potential of his approach; computationally costly procedures such as discriminant function analysis of large samples for several variables were beyond his reach (1960: 37, 55). With the electronic computers available in the 1960s, I hoped to go considerably further, introducing powerful statistical methods to the study, to evaluate the contribution of chance to assemblage differences, and provide a means for the objective demonstration of relationships that had previously been postulated subjectively on vaguely stated or ill-defined grounds. Such tests were absolutely essential to detect relationships between types and to search for the correlates of difference between the facies.

■ GONZÁLEZ ECHEGARAY AND THE CANTABRIAN MOUSTERIAN

I began searching out and classifying relevant collections in the Provincial Museum of Prehistory and Archeology in Santander in late 1962. I had been introduced to the complexities of the Spanish Mousterian by Francisco Jordá Cerdá, who, in a fortunate moment, presented me to Joaquín González EcheGARAY, then its vice-director. Had it not been for Joaquín's guidance, stimulation, and support, my career in Cantabrian prehistory would have been unproductive, boring, possibly frustrating, and certainly short. Thanks to him, Cantabria has ever since been for me an inexhaustible treasure trove of challenging evidence and the city of Santander has become my second home.

Joaquín and the Museum's new director, Dr. M. A. García Guinea, placed its rich collections, its extraordinary library, and its valuable archive at my disposal. No other research environment was then remotely comparable to Santander. The museum was then world-class. It was known for the quality of its library and its collections, for its unselfish openness to all scholars, whatever their nationality, and for the stature of its directors. The international reputation of the young Joaquín González EcheGARAY was already well established, and his authority in Spanish Paleolithic studies universally acknowledged.

Of the greatest benefit to my work was the fact that Joaquín proved to be both extremely interested in, and thoroughly informed about, the current state of Mousterian research. He knew the Cantabrian Mousterian at first hand, having participated in the el Pendo excavations in the 1950s, and investigated the curious Cave of la Mora (González EcheGARAY 1957). He was one of the very few Spanish professionals who made habitual use of the Bordes classification. He took a personal interest in my research from the first. The most I could have expected from a busy museum director was disinterested facilitation of access to collections and documents. Instead, Joaquín spent hours discussing fine points of lithic typology, the Bordes system, Mousterian problems, and the aims and potential of prehistoric research with me. Our relationship led to the thirty-year program of collaborative research whose results are outlined here—research and conclusions now as much his as mine.

■ PRIOR RESEARCH

Prior to our work, most Spanish prehistorians, even some of the very best ones, still classified Mousterian collections in rather haphazard fashion. Despite the early efforts of the Comisión de Investigaciones Paleontológicas y Prehistóricas (1916) to formalize a series of mutually exclusive definitions of tool types, lithic classification in practice remained unsystematic; no single classificatory system was in general use, and even the best fieldworkers often used type definitions that overlapped.

Consideration of the nature of the whole collection was the exception rather than the rule; the classifier's attention was instead focused on a few supposedly diagnostic "guide" types. Assemblages that contained crude large tools such as handaxes

were arbitrarily assigned to a supposedly “early” Mousterian, while collections lacking such pieces were attributed to a “late” Mousterian, called that or “the Mousterian of small types.” Classifiers generally assumed that any collection containing good proportions of large, crudely made tools must be Mousterian or earlier. Some collections of Upper Paleolithic tools from quarry/workshop sites in low terraces of the Manzanares and Jarama basins were misdiagnosed as Mousterian or Acheulean because of their rough, unfinished appearance, and pick-rich, post-Paleolithic assemblages on the coasts of Spain and Portugal were often wrongly classified as Lower Paleolithic—even Oldowan—a problem that persists today.

The best syntheses of the Mousterian in Spain were those of François Bordes and Francisco Jordá Cerdá. The differences between their diagnoses were largely terminological. Bordes had recognized in the collections from Castillo both a manifestation of the Charentian Mousterian (Mousterian Beta) and, in Mousterian Alpha, a “very specialized Mousterian facies . . . characterized by the presence of flake-cleavers, or Olha flakes, a frequent form in Africa . . .” (1953: 463–64), and proposed to call this collection a new “Vasconian” facies, one that he thought might represent a “passing infusion of Levallois technique and African typology” into an industry that is otherwise basically “Quina in nature” (Bordes 1953: 464). Jordá, comparing other Spanish collections to Castillo, concluded that the lower Castillo Mousterian was an Upper Mousterian (meaning a Mousterian like that at La Quina), while the old Morín collection and Castillo Alpha were an “Upper Mousterian of Acheulean Tradition,” implying the addition of bifaces (cleaver flakes) to a Quina-like flake tool series (Jordá 1957: 158). Though these opinions evidently influenced my work, I thought at the time that I had arrived at the best possible classification of the collections quite independently.

■ LOCAL ROOTS OF THE MOUSTERIAN: THE ACHEULEAN AT CASTILLO

Bordes’s claim to see African influence in the collection from Castillo Alpha raised questions about the origins and relationships of the Mousterian in Cantabria. The supposedly “African” types at Castillo, the characteristic “Vasconian” cleaver flakes, were known to Africanists as components of “Late Acheulean” assemblages from the Maghreb. Little was known about local pre-Mousterian industries. Such industries, containing cleaver-flakes, were claimed primarily on the basis of (mixed?) surface collections devoid of stratigraphic context. They were found partly rolled in superficial beach deposits, atop terraces, or atop *rasas* or other land surfaces in the vicinity of sites such as Altamira, el Pendo, and Cueva Morín. Such evidence was inconclusive.

There were bones and a very few nondescript flakes (but no cleaver-flakes) from “pre-Mousterian” strata at el Pendo, in undatable contexts. The only substantial *in situ* collections of apparently pre-Mousterian artifacts in Cantabria were (and remain) the materials from the supposedly Acheulean levels at Castillo. When I began research, these tools had not been reclassified nor their stratigraphic context verified

since their excavation in the early years of the century. Whether or not they are truly Acheulean, as Bordes believed, was not clear.

In February 1963, assisted by Henry Irwin as recorder, I cleaned the standing section at Castillo, and identified and measured the visible strata. The cleaned section showed intact levels from the Upper Paleolithic at top, down through the Mousterian Alpha level (where cleaning produced a cleaver-flake), and on through what was tentatively identified as Mousterian Beta. Both Mousterian “levels” proved to be stratigraphic composites of multiple layers of sediment, with Mousterian Alpha (Level 20 in Cabrera 1984) being at least two levels, together measuring 1 meter in thickness, and Mousterian Beta (Cabrera’s Level 22) consisting of a block of about six levels totaling 1.25 meters in thickness. The two were separated by a “sterile” orange clay, about 60 centimeters thick (Cabrera’s Level 21). However, tools were not uniformly dispersed through these deposits, but seemed instead to occur in much thinner seams; the number of rich cultural horizons encompassed by each Mousterian bed identified by Breuil and Obermaier may possibly be quite small, and the so-called Mousterian Beta deposit may not contain Mousterian tools all the way to its base.

Later that year, González Echegaray and I cut a 2 m × 2 m trench in the southeast corner of the old excavation, and found the supposedly Acheulean basal deposits intact. Atop the sterile “cave clay” was a 30-centimeter layer of whitish clay with much broken bone, including several identifiable fragments of cave bear. In Cabrera’s revised stratigraphy, levels earlier than Mousterian Beta are numbered 23–26 from youngest to oldest. Our cave bear level has the characteristics of her Level 26. There were a very few flakes in its uppermost part. The bear layer was overlain by 60 centimeters of reddish clay with dispersed stones, and another 40 centimeters of chocolate-colored clay, containing bone fragments, with numerous stones in its lower half.

Some flakes but no identifiable retouched tools were recovered from either layer, and the two together probably equate with Obermaier’s culturally poor levels “below the Acheulean” (Cabrera’s Level 25). Immediately above was a 10-centimeter-deep “floor” of flakes, choppers, and small retouched tools, among which were both scrapers and denticulates. This should be Cabrera’s Level 24, at first called a “Moustérien fruste” (Mousterian Gamma), and later Acheulean, by earlier excavators. Above these deposits came some 70 centimeters of orange-brown travertinous deposits, with a dark, organic band some 20 centimeters above its base. This is thought to be Cabrera’s Level 23, a deep, sterile deposit separating Mousterian Beta from the “Acheulean.” Bischoff obtained a U-series date of 89,000 ± 11 ka / –10 ka BP on basal Level 23 (Bischoff, García, and Straus 1991), but I cannot ascertain its exact correlation with the deposit as revealed in my test. Though it is certainly later than the “Acheulean” and earlier than Mousterian Beta, there is no justification for assuming that it is any kind of a terminus for either the local Acheulean, which may have ended very much earlier, or the Mousterian, which may have begun locally either earlier or later than Level 23. Similar questions apply to a date of 92,800 BP for the Castillo “Acheulean” (Level 24?) obtained by Rainer Grün and reported by Cabrera and Bernaldo de Quirós (1992: 106).

Parts of the so-called Acheulean collections from Castillo were warehoused in Santander and parts in Madrid. It was only in 1972 that I had an opportunity to classify the Santander collections. Cabrera, in her invaluable monograph on Castillo (1984), also revised the “Acheulean” from that site, evidently on the basis of collections in Madrid. While our classifications should be completely complementary, some of the artifacts illustrated in her thesis are in fact pieces warehoused in Santander.¹

The collections Cabrera saw apparently had more trimmed pieces and a different proportional representation of types than the collections I saw. Since collections from any single level were very small, I have combined pieces from all so-called Acheulean levels (see Fig. 8.1). There are 6 bifaces (1 irregular ophite biface, 3 partial bifaces of which 2 are cordiform, and 2 broken biface tips). The bifaces, though ill-made, are unlike bifaces in any Mousterian collection I know from Cantabria, and Cabrera figures a large amygdaloidal biface with a strikingly Acheulean allure. There are 106 pieces in my “essential flake tool” series, containing about 36 percent denticulates and 25 percent sidescrapers, but the proportional indices vary from sublevel to sublevel, with sidescrapers more abundant in the upper level (Cabrera’s 24) and denticulates more frequent lower in the sequence. Choppers and chopping tools are far more numerous than in any ordinary Mousterian assemblage from Cantabria, amounting to almost 15 percent of the essential flake tool series (Fig. 8.1). Indices of sidescrapers and denticulates in the partial collections classified by Cabrera were variable, but she also found that in Level 24, sidescrapers outnumbered denticulates substantially. However, her collections contained very few chopping tools, and more Levallois types than I know for any collection from Cantabria.

None of the flake tools would be out of place in a Mousterian assemblage. Except for their high proportions of choppers and chopping tools, the flake tool series from these levels could be called Mousterian. Nevertheless, I am reluctant to do that. The flake tools in some classic Micoquian assemblages look just as Mousterian, and the Micoquian is nevertheless called Acheulean by everyone. The shapes and technical characteristics of the bifaces, and the extraordinary proportions of choppers and chopping tools in these collections, are characteristics that are out of the range of variability for other Cantabrian Mousterian. That does not imply that I see a clear break between the Acheulean and Mousterian in Cantabria or elsewhere—in fact, I believe that continuity between latest Acheulean and earliest Mousterian is the rule, not disjunction. But I see no artifactual grounds for excluding the Castillo collections from the Acheulean at present.

Cleaver flakes were not found in the collections I saw. There is one somewhat irregular cleaver flake in the series Cabrera classified: that one piece, which cannot be intrusive from a very much higher level, suggests that local antecedents of the “African” type in fact do exist. There may very well be a long, continuous cleaver-making tradition in Cantabria, as there is elsewhere in Spain (I found good proportions of such pieces in the Tahivilla Acheulean), whatever their original relationships to African assemblages. We need not postulate a sudden later Mousterian importation of foreign techniques and types to account for Mousterian Alpha.

■ THE PASSAGE FROM MOUSTERIAN TO UPPER PALEOLITHIC IN CANTABRIA

In 1963, the question of the nature of the local industrial transition from the Mousterian to Upper Paleolithic loomed large, as it still does. In France, the earliest Upper Paleolithic complex known seemed to be the Chatelperronian; some thought that it marked a real break with the Mousterian, while others, including Bordes, saw continuities between the Chatelperronian and one Mousterian facies. A minority held that wherever the complex had been found, levels were heavily cryoturbated or mixed. Cantabria seemed to offer the alternative possibility of regional variability: at the sites of el Conde in Oviedo and Cueva Morín in Santander, early excavators claimed to have found transitional Mousterian/Upper Paleolithic horizons, the so-called Aurignaco-Mousterian, or “Proto-Aurignacian,” with characteristics quite different from the Chatelperronian, which itself was still unknown from well-excavated contexts in the Iberian Peninsula.

Our research soon laid that complex to rest (González Echegaray and Freeman 1971, 1973). Almost all “transitional” collections, we discovered, were just misidentified. The two principal collections on which claims were based proved to be mixed. My test excavations at el Conde eliminated the supposed transitional level there: it was in fact a mixture of a Mousterian level and an Upper Paleolithic level (Freeman 1977). At Morín, too, the transitional level resulted from inadequate excavation: our predecessors had dug several levels, from the uppermost Mousterian deposits through the lower Aurignacian horizons, together as a unit.

Right atop the last Mousterian occupation at Morín, we found a perfectly characteristic Chatelperronian horizon: the first convincing level of its kind in Spain. Later, in examining the well-excavated assemblages from el Pendo, we found another interesting Chatelperronian industry in Level 8. As sometimes in France, the Chatelperronian from el Pendo overlies a horizon of Early Aurignacian materials. (An apparently Chatelperronian horizon has since been reported from the cave of Ekain as well.)

The early Upper Paleolithic in Cantabria—and elsewhere in Spain—is respectably old. One date of about 35,000 BP was obtained for the Morín Chatelperronian, but seems unreliable. Accelerator mass spectrometer radiocarbon dates more recently reported by Cabrera and Bischoff (1989) for the earlier Aurignacian at Castillo averaged 38,700 BP ± 1900. Were dates obtained by the same procedures available for the Chatelperronian and Early Aurignacian at Morín and el Pendo, they would probably indicate comparable antiquity.

■ MOUSTERIAN FACIES: EARLY GLIMPSES OF A PROBLEM

My first investigations of the Cantabrian Mousterian proper, as I have said, were principally based on collections made by earlier excavators: collections excavated at Morín and el Pendo by Father Carballo; from the site of el Conde or el Forno in Asturias, made by the Conde de la Vega del Sella (who also excavated at Morín); from

Castillo, a site that was still our most important source of data about the Paleolithic industrial sequence in Spain, made by the Abbé Breuil and Hugo Obermaier; from la Flecha (Freeman and González Echegaray 1968) and la Pasiega in the Castillo hill, excavated by Dr. Garcia Lorenzo and others; and some levels that proved not to be Mousterian or were too small for diagnosis, from sites such as la Chora, Otero, la Busta, and la Cueva. I also classified materials from five levels in the Passemard excavations (Passemard 1936) at Abri Olha in the French Pyrenees, some of which contained cleaver flakes. In addition, I examined part of the collections from the well-controlled excavations conducted from 1953 to 1957 by an international group under the direction of J. Martínez Santa-Olalla at the site of el Pendo; a thorough study of the Mousterian levels would have been central to the thesis research, but Santa-Olalla had not decided their disposition, so I was not permitted to undertake a complete classification of tools from any level or to report my impressions of them. A small amount of information, mostly in the form of clarification of stratigraphic questions, was provided by very limited test excavations I conducted for the Santander Provincial Museum at Castillo and Cueva Morín in 1963, while my test for the Provincial Archeological Museum of Oviedo at the Cueva del Conde (Freeman 1977) added small uncontaminated assemblages from Paleolithic levels that had escaped clandestine excavation in a small cul-de-sac at the back of the vestibule.

The apparently simple task of reclassifying these older collections was complicated by the fact that they were dispersed, and information about the whereabouts of the different portions of each assemblage was incomplete. Locating the various parts of the collections and traveling to the several museums in different countries that housed them proved to be quite time-consuming.

The location of the Castillo collections is a good illustration of this difficulty. The flake tools from the major Mousterian collections were dispersed as shown in Table 8.1.

In addition, there were seven nondescript pieces from Mousterian Beta and six from Mousterian Alpha (as well as some 50 Mousterian tools from Cueva Morín) in the Nels C. Nelson collections of the American Museum of Natural History in New York, acquired in 1913.²

■ CANTABRIAN FACIES I: THE CASTILLO COLLECTIONS (FIGS. 8.1, 8.2)

The first stage of research was the classification of tools in each collection, the calculation of percentages for tool type and of the characteristic indices. (A definitive list of cumulative percentages for the most reliable Mousterian collections known from Cantabria at this writing is given in Figure 8.1.) Graphs of the cumulative percentages of “essential” tool types were drawn. They and the indices were the data used to assign collections to facies. I knew, of course, that some of the collections might prove mixed or misleading, making facies recognition difficult or impossible.

The first collection classified was, however, not at all problematic. The huge collection (4,303 stone artifacts, 3,147 “essential” flake tools) from Mousterian Beta

■ TABLE 8.1. *Dispersal of Castillo Mousterian*

<i>Locale</i>	<i>Flake tools (Moust. Beta)</i>	<i>Flake tools (Moust. Alpha)</i>
Museo Arqueológico		
Provincial, Oviedo	53	0
IPH (Paris)	796	876
Museo Municipal, Madrid	46	27
Museo Provincial, Santander	2,493	2,004
Total	3,388	2,907

(Level 22 in Cabrera's system) had only eight bifaces, almost no Levallois technique or Levallois tools, few denticulates, more than 65 percent sidescrapers in the "essential" flake tool series, and more than 30 percent of "Charentian" types. These characteristics, coupled with good numbers of Quina scrapers, made assignment to the Quina Charentian subfacies obvious. (Though I suspected that the Castillo Beta level included assemblages from more than one occupation, the levels confused must have been overwhelmingly Quina in content to produce the Mousterian Beta percentages.)

The Mousterian Alpha collection (Level 20 of Cabrera) was also immense. It provided 4,382 stone artifacts of which 2,530 were "essential" flake tools, and another 334, or 11 percent of the combined flake tool + biface series, were bifaces, including 303 cleaver flakes. At the time, the proportion of sidescrapers it contained, over 43 percent, seemed somewhat high for Typical Mousterian, and there were few of the Mousterian points that are so often found in that facies. Denticulates had risen to 31 percent of the collection. Levallois technique was more abundant, but still involved only 12 percent of flakes, and there were less than 1 percent Levallois types. While the Charentian index had dropped, there were still many Quina pieces. There were notable similarities between the graphs of the Mousterian Alpha and Mousterian Beta flake tool series, the principal difference between them being the increase of denticulates in Mousterian Alpha. Bordes, too, was more impressed by the similarity of the two graphs than by their difference. I finally convinced myself that Bordes had been right to consider Mousterian Alpha as basically Quina with an infusion of cleaver flakes and an anomalously high proportion of denticulate tools. (I now think Mousterian Alpha may actually be a mixture of Denticulate Mousterian, Typical Mousterian, and Charentian assemblages, but at the time it seemed appropriate to treat it as a valid collection with peculiar characteristics.)

The classification of the Castillo collections skewed facies assignment for the remaining Cantabrian collections. Applying Bordes's facies definitions to the graphs and indices of his "essential" flake tool series, and what I thought I had learned at Castillo, I thought I could assign several of the other collections to one of two facies. The Quina variant of the Charentian Mousterian, recognized in Castillo Beta, was also present at Cueva Morín: the curve of the old, cleaver flake-rich collection from Morín was virtually indistinguishable from that of the Castillo level. The collection from Hornos de la Peña, though evidently somewhat selected, seemed most like them. The Charentian Mousterian was also certainly documented for the French

Pyrenees, in three levels at Abri Olha, Foyers inférieurs 2, 3, and 4, the first apparently the Ferrassie variant, and the latter two more probably Quina with cleaver flakes, as I then thought. Olha F. i. 2 actually had just too few sidescrapers to be attributed to Quina, but too many sidescrapers and Charentian types to have been assigned to the Typical Mousterian as then defined. (I considered it somewhat selected.)

The collection (Fig. 8.6) from la Flecha (Freeman and González Echegaray 1968) as well as the assemblage I excavated in Level 6 at the Cueva del Conde (Freeman 1977) were readily recognized as Denticulate Mousterian. Abri Olha F. i. 1, which had a single cleaver flake (seemingly in situ), and Olha Foyer moyen were also Denticulate collections.

Other collections, principally that from el Pendo, would have been much harder to assign to any facies, if I hadn't used the model of Castillo Alpha. Though the collection from Pendo was short (only 38 "essential" flake tools) its graph was nearly identical to that of Castillo Alpha. That apparent similarity led me to conclude that other similar collections were probably Charentian too. If they fell short of the threshold value for sidescraper proportions for that attribution (IRes = 55 percent), it was either because some sidescrapers had been discarded, while other tools were selectively saved, or because the threshold value was set too high. I tentatively proposed lowering the required sidescraper index to 40 percent. Once that was done, all the anomalous collections could be accommodated in an expanded Charentian.

There was some contradictory evidence, though I couldn't see it at the time. Had I placed my faith in the small assemblage excavated from el Conde 8/9, I should have seen that expansion of the Charentian was not what was called for. But that assemblage came from a narrow, limited cul-de-sac, contained a large number of geologically crushed (conglifract) pieces, and was small: there were only 65 "essential" flake tools.

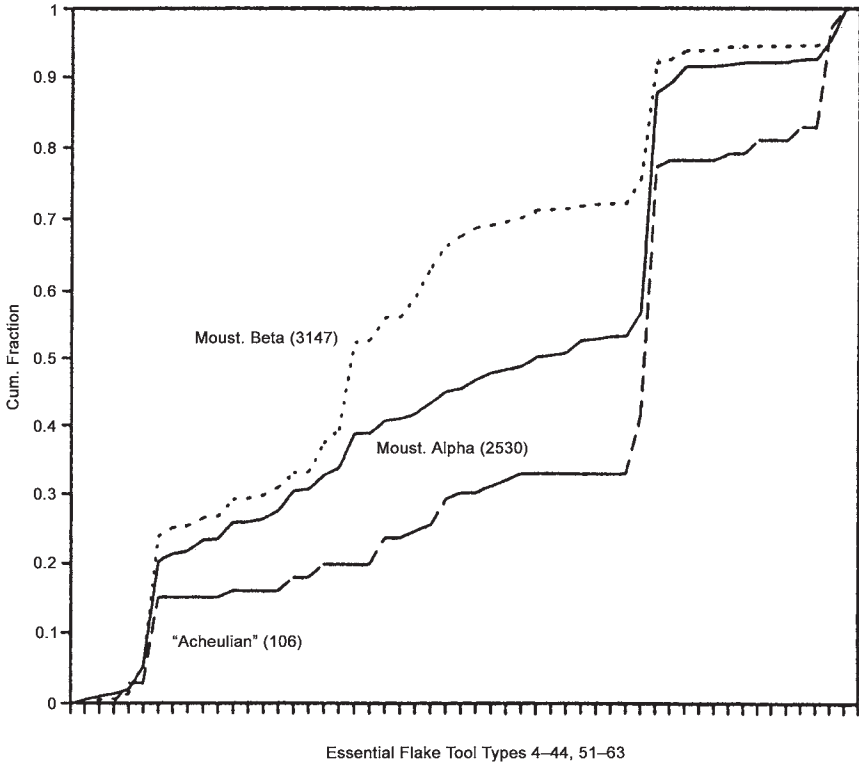
Despite its faults, this early research made several positive and lasting contributions to our understanding of Cantabrian Mousterian facies. The first was recognition that Levallois tools and Levallois technique were virtually not represented in Cantabria: the technique was mostly found on ophite and exceptionally large quartzite pieces, both much larger than the usual raw materials in the region; it was almost never found among the ordinary quartzite and flint pieces, the vast majority of which were made on cobbles of small sizes and poor quality. Second, Cantabrian Denticulate Mousterian proved to be unusually rich in denticulate tools. Last, the collections from Cantabria and the Pyrenees proved that cleaver flake-bearing collections were otherwise heterogeneous; that did away with the so-called Vasconian as a viable industrial facies. Bordes somewhat reluctantly agreed that the Vasconian was untenable as a facies, suggesting that the designation should in future only be applied to the distinctive Cantabrian cleaver flakes themselves.

The thesis research taught me—the hard way—that the reliability of older collections was very irregular. Some provided useful, even invaluable information. Others proved to be less than ideal for many purposes, among those being facies attribution, though I was not fully aware of this at the time I wrote the thesis. While conclusions derived from the statistical tests described later proved robust in general

Type	4	Cast "Ach"	CasMustA	CasMustB	Morin 11	Morin 12	Mor 13 14	Morin 15	Mor 16NW	Mor16Sub
		0	0	0	0.00000	0	0	0.00000	0.003257	0
5	0	0.004743	0.000318	0.03743	0.022901	0.022222	0.00000	0.016287		0
6	0	0.010277	0.005084	0.03743	0.022901	0.022222	0.01010	0.032573	0.013514	
7	0	0.013439	0.006037	0.03743	0.022901	0.022222	0.01010	0.032573	0.018018	
8	0.028302	0.019763	0.013664	0.04278	0.022901	0.022222	0.01010	0.035831	0.022523	
9	0.028302	0.051383	0.05402	0.04813	0.022901	0.033333	0.07071	0.071661	0.054054	
10	0.150943	0.201581	0.238322	0.09626	0.057252	0.211111	0.21212	0.237785	0.135135	
11	0.150943	0.213043	0.250397	0.10160	0.057252	0.211111	0.21212	0.2443	0.13964	
12	0.150943	0.216996	0.253257	0.10160	0.057252	0.211111	0.21212	0.2443	0.144144	
13	0.150943	0.233202	0.264697	0.10160	0.057252	0.211111	0.22222	0.247557	0.166667	
14	0.150943	0.233992	0.267239	0.10160	0.057252	0.211111	0.22222	0.247557	0.166667	
15	0.160377	0.258893	0.292024	0.10160	0.057252	0.233333	0.22222	0.257329	0.175676	
16	0.160377	0.259289	0.293295	0.10160	0.057252	0.233333	0.22222	0.257329	0.175676	
17	0.160377	0.263241	0.297744	0.10160	0.057252	0.233333	0.22222	0.257329	0.175676	
18	0.160377	0.275099	0.309183	0.10160	0.057252	0.233333	0.22222	0.257329	0.175676	
19	0.179245	0.303953	0.331109	0.10160	0.057252	0.244444	0.23232	0.267101	0.175676	
20	0.179245	0.306719	0.331745	0.10160	0.057252	0.244444	0.23232	0.267101	0.175676	
21	0.198113	0.327273	0.37496	0.11230	0.057252	0.255556	0.27273	0.29316	0.175676	
22	0.198113	0.337945	0.394026	0.11765	0.057252	0.255556	0.28283	0.29316	0.184685	
23	0.198113	0.388142	0.523356	0.16043	0.064885	0.288889	0.39394	0.32899	0.193694	
24	0.198113	0.388933	0.527169	0.16043	0.064885	0.288889	0.39394	0.32899	0.193694	
25	0.235849	0.407115	0.560534	0.17647	0.072519	0.288889	0.42424	0.351792	0.198198	
26	0.235849	0.409881	0.561805	0.17647	0.072519	0.288889	0.42424	0.355049	0.202703	
27	0.245283	0.417391	0.592628	0.17647	0.072519	0.288889	0.44444	0.364821	0.202703	
28	0.254717	0.433202	0.636161	0.18182	0.072519	0.311111	0.44444	0.371336	0.202703	
29	0.292453	0.449012	0.662854	0.19251	0.072519	0.322222	0.46465	0.394137	0.211712	
30	0.301887	0.45415	0.675882	0.19251	0.072519	0.322222	0.46465	0.416938	0.216216	
31	0.301887	0.467589	0.687957	0.20321	0.091603	0.333333	0.48485	0.442997	0.22973	
32	0.311321	0.47747	0.691452	0.24064	0.099237	0.333333	0.48485	0.449511	0.22973	
33	0.320755	0.483004	0.695901	0.25668	0.110687	0.366667	0.49495	0.469055	0.261261	
34	0.330189	0.487352	0.701938	0.27807	0.129771	0.366667	0.50505	0.488599	0.265766	
35	0.330189	0.501581	0.712107	0.35829	0.240458	0.388889	0.57576	0.543974	0.369369	
36	0.330189	0.503953	0.712425	0.35829	0.240458	0.388889	0.57576	0.543974	0.369369	
37	0.330189	0.50751	0.713696	0.35829	0.259542	0.422222	0.57576	0.547231	0.369369	
38	0.330189	0.526877	0.718462	0.35829	0.270992	0.433333	0.59596	0.563518	0.391892	
39	0.330189	0.528854	0.720369	0.36364	0.274809	0.433333	0.59596	0.570033	0.391892	
40	0.330189	0.532806	0.72164	0.39037	0.28626	0.455556	0.63636	0.579805	0.391892	
41	0.330189	0.533597	0.722911	0.39037	0.28626	0.455556	0.63636	0.579805	0.391892	
42	0.415094	0.568379	0.757547	0.49733	0.438931	0.555556	0.72727	0.700326	0.518018	
43	0.773585	0.87747	0.919924	0.84492	0.847328	0.966667	0.95960	0.905537	0.900901	
44	0.783019	0.8917	0.925326	0.89840	0.874046	0.988889	0.97980	0.928339	0.918919	
51	0.783019	0.91502	0.937718	0.89840	0.881679	0.988889	0.97980	0.931596	0.981982	
52	0.783019	0.915415	0.938036	0.90909	0.900763	0.988889	0.97980	0.938111	0.981982	
53	0.783019	0.915415	0.938036	0.90909	0.90458	0.988889	0.97980	0.938111	0.981982	
54	0.792453	0.917787	0.943756	0.93048	0.927481	0.988889	0.97980	0.954397	0.981982	
55	0.792453	0.920553	0.944391	0.93048	0.927481	0.988889	0.97980	0.954397	0.981982	
56	0.811321	0.921344	0.945027	0.93048	0.927481	0.988889	0.97980	0.954397	0.981982	
57	0.811321	0.921344	0.945027	0.93048	0.927481	0.988889	0.97980	0.954397	0.981982	
58	0.811321	0.921739	0.945027	0.93048	0.927481	0.988889	0.97980	0.954397	0.981982	
59	0.830189	0.925296	0.94598	0.93048	0.931298	0.988889	0.97980	0.967427	0.981982	
60	0.830189	0.925692	0.94598	0.93048	0.931298	0.988889	0.97980	0.967427	0.981982	
61	0.971698	0.95336	0.951382	0.93583	0.931298	0.988889	0.98990	0.973941	0.981982	
62	1	0.998419	1	1.00000	1	1	1.00000	1	1	
63	1	0.999209	1	1.00000	1	1	1.00000	1	1	
Sum		106	2530	3147	187	262	90	99	307	222

■ FIGURE 8.1. Cumulative percentages of "essential" flake tools in nineteen Cantabrian assemblages

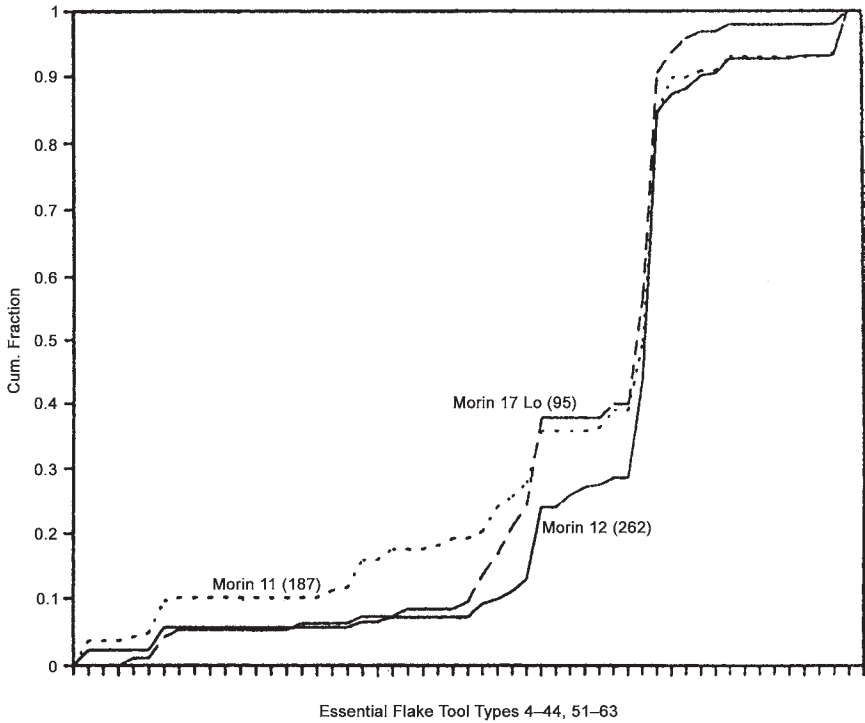
Cumulative	Ess. %									
Mor up 17	Mor lo 17	Pendo8D	Pendo12	Pendo 13	Pendo 14	Pendo 16	Conde0	Conde0-0	Fecha	
0	0	0	0	0	0	0	0	0	0	0
0.010724	0	0	0.010101	0	0	0.005747	0.02649	0	0.005291	
0.013405	0	0	0.010101	0	0	0.005747	0.02649	0	0.005291	
0.016086	0	0	0.010101	0	0	0.005747	0.02649	0	0.005291	
0.016086	0.010526	0	0.010101	0	0	0.005747	0.02649	0	0.013228	
0.037534	0.010526	0	0.020202	0	0.040816	0.005747	0.039735	0.09375	0.034392	
0.134048	0.042105	0.12069	0.040404	0.306122	0.285714	0.166667	0.07947	0.296875	0.063492	
0.142091	0.052632	0.12069	0.050505	0.306122	0.326531	0.172414	0.07947	0.296875	0.071429	
0.142091	0.052632	0.12069	0.050505	0.306122	0.326531	0.172414	0.07947	0.296875	0.071429	
0.144772	0.052632	0.155172	0.050505	0.306122	0.346939	0.178161	0.07947	0.296875	0.071429	
0.144772	0.052632	0.155172	0.050505	0.306122	0.346939	0.178161	0.086093	0.3125	0.074074	
0.163539	0.052632	0.155172	0.050505	0.346939	0.408163	0.206897	0.086093	0.328125	0.074074	
0.163539	0.052632	0.155172	0.050505	0.346939	0.408163	0.206897	0.092715	0.328125	0.074074	
0.163539	0.052632	0.155172	0.050505	0.346939	0.408163	0.206897	0.093338	0.328125	0.074074	
0.163539	0.052632	0.155172	0.050505	0.346939	0.408163	0.206897	0.10596	0.34375	0.074074	
0.174263	0.063158	0.155172	0.050505	0.346939	0.428571	0.224138	0.10596	0.34375	0.074074	
0.174263	0.063158	0.155172	0.050505	0.346939	0.428571	0.224138	0.10596	0.34375	0.074074	
0.187668	0.063158	0.155172	0.070707	0.346939	0.44898	0.229885	0.10596	0.34375	0.076722	
0.190349	0.063158	0.172414	0.070707	0.346939	0.44898	0.229885	0.10596	0.34375	0.076722	
0.217158	0.073684	0.189655	0.090909	0.387755	0.489796	0.247126	0.112583	0.390625	0.092593	
0.217158	0.073684	0.189655	0.090909	0.387755	0.489796	0.247126	0.119205	0.390625	0.092593	
0.230563	0.073684	0.206897	0.121212	0.428571	0.510204	0.252874	0.125828	0.4375	0.103175	
0.233244	0.084211	0.206897	0.121212	0.428571	0.510204	0.252874	0.125828	0.4375	0.10582	
0.233244	0.084211	0.206897	0.121212	0.428571	0.510204	0.258621	0.125828	0.4375	0.113757	
0.235925	0.084211	0.206897	0.141414	0.428571	0.530612	0.258621	0.125828	0.4375	0.116402	
0.24933	0.084211	0.206897	0.151515	0.510204	0.571429	0.270115	0.13245	0.46875	0.121693	
0.262735	0.094737	0.224138	0.161616	0.510204	0.591837	0.287356	0.145695	0.5	0.132275	
0.308311	0.136842	0.275862	0.232323	0.571429	0.632653	0.304598	0.192053	0.578125	0.156085	
0.321716	0.168421	0.293103	0.242424	0.571429	0.673469	0.33908	0.192053	0.578125	0.156085	
0.351206	0.210526	0.293103	0.242424	0.591837	0.714286	0.362069	0.192053	0.59375	0.161376	
0.378016	0.242105	0.293103	0.262626	0.591837	0.714286	0.373563	0.198675	0.609375	0.164021	
0.44504	0.378947	0.344828	0.323232	0.612245	0.734694	0.396552	0.211921	0.609375	0.18254	
0.44504	0.378947	0.344828	0.323232	0.612245	0.734694	0.396552	0.211921	0.609375	0.18254	
0.453083	0.378947	0.344828	0.323232	0.612245	0.734694	0.396552	0.225166	0.609375	0.185185	
0.469169	0.378947	0.344828	0.343434	0.653061	0.77551	0.402299	0.264901	0.625	0.21164	
0.469169	0.378947	0.344828	0.353535	0.653061	0.77551	0.402299	0.291391	0.625	0.219577	
0.474531	0.4	0.362069	0.353535	0.653061	0.77551	0.41954	0.291391	0.625	0.219577	
0.474531	0.4	0.362069	0.363636	0.653061	0.77551	0.41954	0.291391	0.625	0.219577	
0.597855	0.568421	0.517241	0.454545	0.77551	0.836735	0.465517	0.357616	0.71875	0.293651	
0.882038	0.905263	0.896552	0.969697	0.938776	0.959184	0.948276	0.980132	0.890625	0.867725	
0.906166	0.936842	0.965517	0.979798	0.959184	0.979592	0.965517	0.980132	0.90625	0.89418	
0.911528	0.957895	0.982759	1	0.979592	1	0.988506	0.980132	0.90625	0.899471	
0.91689	0.968421	0.982759	1	0.979592	1	0.988506	0.980132	0.90625	0.899471	
0.91689	0.968421	0.982759	1	0.979592	1	0.988506	0.980132	0.90625	0.899471	
0.941019	0.978947	1	1	0.979592	1	0.994253	0.986755	0.9375	0.912698	
0.941019	0.978947	1	1	0.979592	1	0.994253	0.986755	0.9375	0.912698	
0.941019	0.978947	1	1	0.979592	1	0.994253	0.986755	0.9375	0.912698	
0.941019	0.978947	1	1	0.979592	1	0.994253	0.986755	0.9375	0.912698	
0.941019	0.978947	1	1	0.979592	1	0.994253	0.986755	0.9375	0.912698	
0.946381	0.978947	1	1	0.979592	1	0.994253	0.986755	0.9375	0.915344	
0.946381	0.978947	1	1	0.979592	1	0.994253	0.986755	0.9375	0.915344	
0.954424	0.978947	1	1	0.979592	1	0.994253	0.986755	0.953125	0.920635	
1	1	1	1	1	1	1	1	1	1	
1	1	1	1	1	1	1	1	1	1	
373	95	58	99	49	49	174	151	64	378	



■ FIGURE 8.2. *Cumulative graphs, Castillo collections*

outline—they can be shown to hold with the far superior data now available—the characteristics of the collections, many of which we know now to have been mixed, were in many respects misleading.

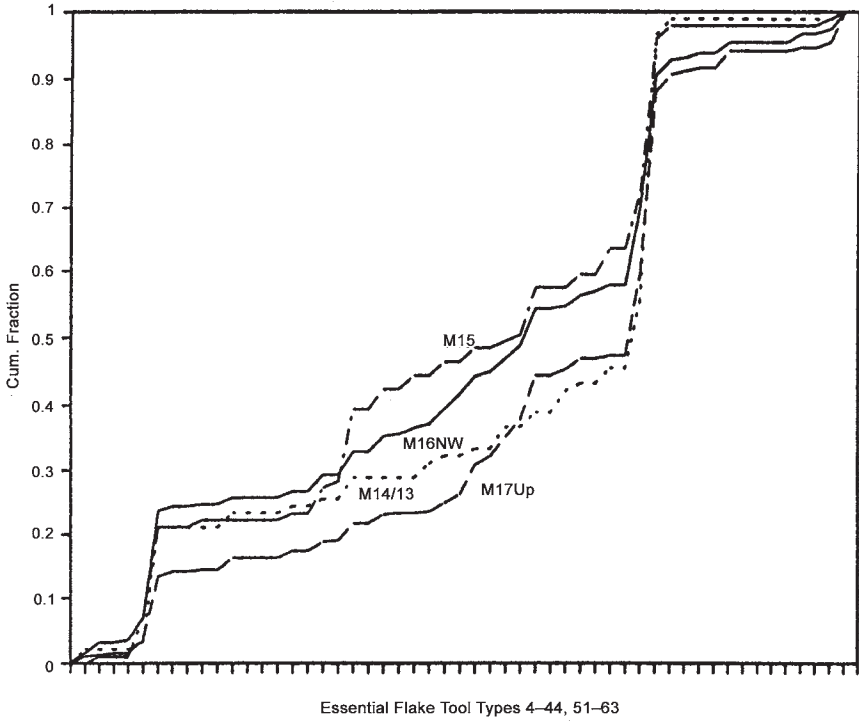
It also led me correctly to conclude that in some respects the Bordes system of facies designation was far from perfect. One of the strengths of the system, in theory, was the fact that it used the characteristics of whole assemblages, rather than the presence or absence of a few diagnostic “guide fossils,” to classify assemblages. Yet, in practice, it was sometimes impossible to tell which one of two facies an assemblage belonged to in the absence of one or two diagnostic types. The Mousterian of Acheulean Tradition Type A contained from 30 to 40 percent sidescrapers, while in the Typical Mousterian there could be from 20 to 55 percent of those tools. The ranges of these thresholds overlapped. The Denticulate Mousterian had at least 35 percent denticulates and few sidescrapers, characteristics that didn’t differentiate it from the Mousterian of Acheulean Tradition, Type B. To define a collection as Mousterian of Acheulean Tradition, there had to be appreciable numbers of either bifaces (Type A) or backed knives (Type B). The Charentian macrofacies always had more than 55 percent sidescrapers, but an assemblage with even more numerous



■ FIGURE 8.3. *Cumulative graphs, Denticulate Mousterian (1), Morin*

sidescrapers couldn't be assigned to that taxon unless it had either good numbers of sidescrapers with a special kind of shape and retouch—Quina types—or good proportions of tools on Levallois flakes. In the museum collections I had examined there were sidescraper-rich collections that failed to meet these criteria. While Bordes claimed that well-excavated assemblages were not ordinarily problematic—that few or no assemblages were truly intermediate or hard to classify—the Cantabrian collections suggested that this might not be the case. (Later excavations soon yielded numerous “intermediate” or unclassifiable assemblages.)

The results of this study were incorporated in my 1964 doctoral dissertation for the Department of Anthropology of the University of Chicago, entitled “Mousterian Developments in Cantabrian Spain.” I then set out to learn about Mousterian developments elsewhere in Spain. During 1966, with a Richard Carley Hunt Fellowship from the Wenner-Gren Foundation for Anthropological Research, I classified all Mousterian collections then known from Spanish sites outside Cantabria. Those collections showed considerable regional variation, but that (apparently) stylistic variability was masked or essentially lost in the Bordes typology. Then my view of Mousterian complexity was more radically changed by the data from renewed excavations at Cueva Morín.

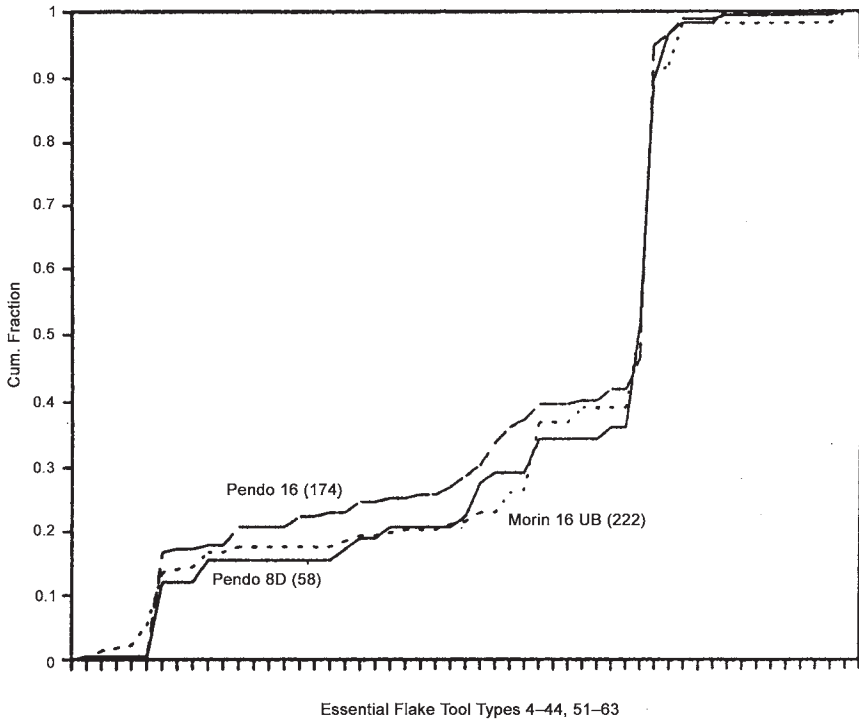


■ FIGURE 8.4. Cumulative graphs, cleaver flake assemblages, Morín

■ CANTABRIAN FACIES II: NEW EXCAVATIONS AT CUEVA MORÍN

Morín, where our tests showed that extensive in situ deposits of cleaver-flake rich Mousterian still remained, seemed an ideal locale for excavation to clarify the nature of the Cantabrian cleaver flake Mousterian. Horizontal distributions could be exposed over large areas, and good bone preservation would permit the study of associations between particular faunal elements and particular types of stone tools.

In 1968, González Echegaray and I began work at Morín, financed by the National Science Foundation. Instead of the single homogeneous Mousterian deposit the earlier excavators thought they had discovered, we found eight different Mousterian deposits, and beneath them a ninth (Level 22), probably Mousterian, but too poor for certain classification (González Echegaray and Freeman 1971, 1973). The sediments were studied by Karl Butzer, who provided a paleoclimatic interpretation and a suggested chronology. Unfortunately, the Mousterian levels were not directly datable, and radiocarbon dates for the earliest Upper Paleolithic levels were not all satisfactory; we now suspect that they should have been very much earlier. Nor was pollen recovered from the Mousterian samples. Table 8.2 shows the facies



■ FIGURE 8.5. Cumulative graphs, Denticulate Mousterian (2), Morín and el Pendo

attribution and climatic correlates for the earlier levels at this site (and at el Pendo). The Morín levels are those prefixed with an M. We had excavated these assemblages carefully, controlling for microstratigraphic difference as carefully as possible. By the end of the second and final season, the “essential” tool type count for each level was at least 90, and four of the assemblages were substantially larger. (Counts and cumulative percentages of essential tool types for the Morín Mousterian assemblages are given in Figure 8.1.)

Since we were now dealing with substantial and well-excavated artifact assemblages, it was surprising that some of the assemblages were still hard to classify. We no longer expected to find intergradation between large uncontaminated assemblages, or to find assemblages that fell between facies: discussions with Bordes had convinced me that my thesis problems were due solely to mixture or selection in the old collections. Yet intergradation is precisely what we found; and in this case, what our eyes saw, statistical tests confirmed.

Bordes’s practice in attributing assemblages to the facies was to use a series of fixed thresholds of abundance for sidescrapers, Charentian tool types, denticulate tools, etc., and a visual appreciation of similarity or difference between cumulative

■ TABLE 8.2. *Mousterian and Early Upper Paleolithic levels at Morin and el Pendo*

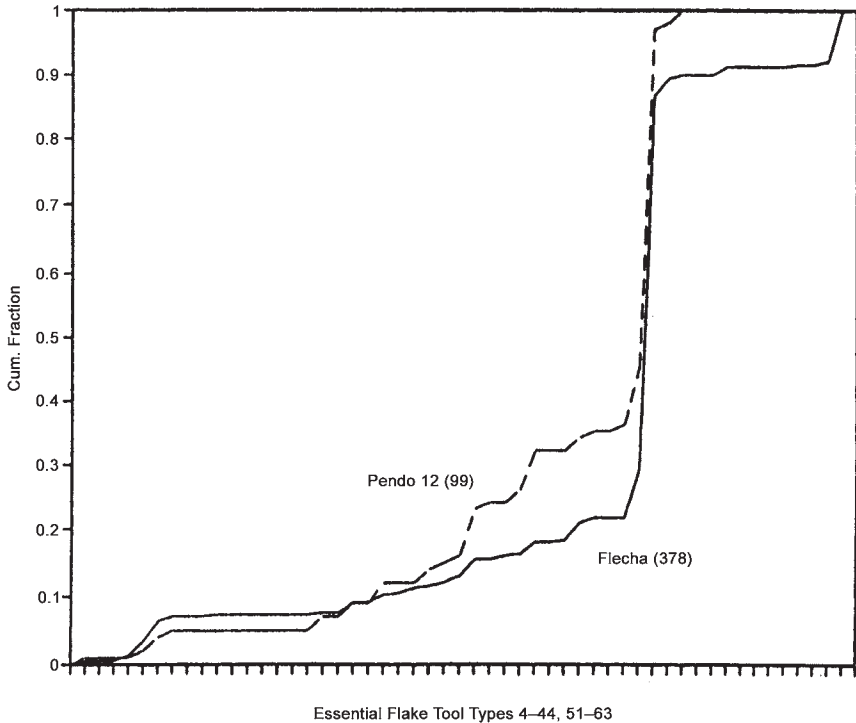
<i>Level</i>	<i>Attribution</i>	<i>Paleoclimate</i>	<i>¹⁸O Stage</i>
M10,P8	Chat., Aurig.	Cold	3
M11	Dentic. Moust.	Temperate	3
M12,P8D	Dentic.	Cool, moist	3
M13/14	Typic.(ss) + CF	"	3
M15	"	Temperate	3
*M16	Typic.(ss) + CF	Cool, moist	3
MUp17,P11	Typic. + CF, Dentic.	Cool, dry summers	3
MLo17,P12	Dentic.	Cool, moist summers	3
P13	Typic.(ss) + CF	Temp., warm summers	3
P14	Typic.(ss)	"	4
P15	1 tool	Cold	4
P16	Dentic.	Cold	5

Data from González Echegaray and Freeman (1978); González Echegaray et al. (1971, 1973, 1980); climate and isotope stage from Butzer (1981).

* The contents of M16 are more complex than indicated here (see text).

percentage graphs. No one could say how meaningful these thresholds really were, or how well they differentiated assemblages, for no one could say just how much graphs had to diverge before they were really different. Some difference is always present, even between samples from the same assemblage, just due to chance alone, and no one in Mousterian studies knew how to calculate the possible contribution of such random errors to assemblage differences. By 1968, this had changed. We had found a powerful statistical tool for the objective evaluation of similarity between cumulative percentage lists: the Kolmogorov-Smirnov test, still the best one available for that purpose. Unlike tests that evaluate differences in central tendency—median tests, the Mann-Whitney U test, and such others—it is sensitive not just to differences in mean or median value, but to the magnitude of differences in any part of the frequency distribution. More powerful than chi-square, it is also more efficient. In any collection, several tool types will usually be unrepresented. With chi-square, empty categories often have to be omitted or collapsed, and that is not necessary with Kolmogorov-Smirnov. True, Kolmogorov-Smirnov is sensitive to the order of the variables, a fact that has been seen as an objection to it, but those who use the Bordes type list always adhere to the same ordering. While that order is arbitrary, as long as it is invariant, the test can always be used to evaluate similarity between collections, and to check the reliability of subjective evaluations of similarity and difference.

When very large collections are compared, relatively small differences between them may have considerable significance. The fact that the Kolmogorov-Smirnov test detects significant difference between two assemblages is not always sufficient reason for deciding that they belong to different facies. But where the test can detect no significant difference between two assemblages that would be or have been assigned to different facies by any classifier following the Bordes system, there is obviously something wrong with the facies classification. That is precisely what we

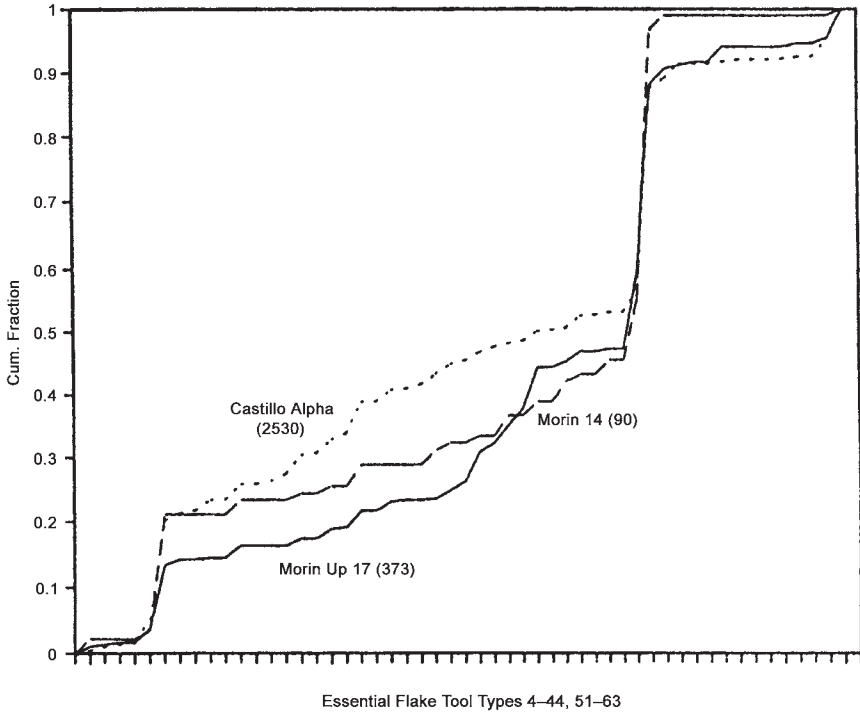


■ FIGURE 8.6. *Cumulative graphs, Denticulate Mousterian (3), el Pendo and la Flecha*

found once we began to use the Kolmogorov-Smirnov test on Cantabrian Mousterian assemblages.

By the end of the second season (1969), we had recovered three assemblages that presented no classificatory problem whatever. Denticulate Mousterian, of the now-familiar, technically non-Levallois, unafaceted, variety was obviously present in Levels 11, 12, and Lower Level 17 (Fig. 8.3). The Kolmogorov-Smirnov test showed that while there were significant differences between Levels 11 and 12, both assemblages were very similar to that from Lower Level 17, and all three fit the Bordes facies definition.

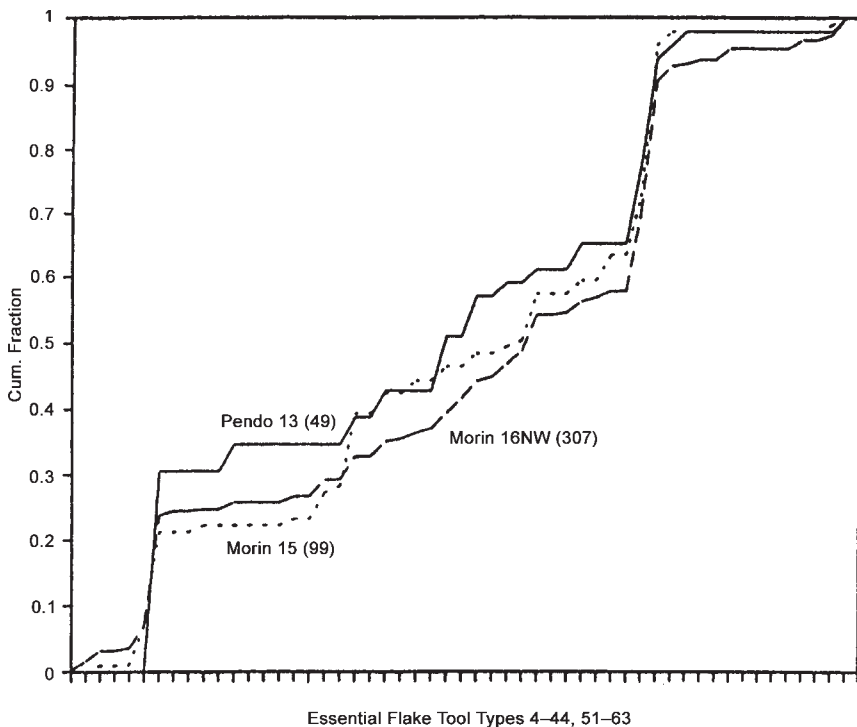
All of the other levels (Upper 17, 16NW, 15, and 14/13) contained varying quantities (just one from 14/13) of cleaver flakes. That of course was of little help in facies diagnosis. Unexpectedly, unlike the old, mixed, and selected Morin collection, none of these assemblages had enough sidescrapers to be called Charentian, and none had a high Charentian index (Fig. 8.4). The Kolmogorov-Smirnov test indicated substantial intergradation among them. Levels 13/14, 15, and 16 were each similar to the others. Upper 17 was certainly similar to 13/14, too, but not to the others—but, more important, it was also not significantly different from one of the Denticulate assemblages, that from Level 11. This paradoxical relationship was not



■ FIGURE 8.7. Cumulative graphs, Typical Mousterian (1), Castillo and Morin

unique: Level 13/14 was also neither significantly different from Level 11, nor from Lower Level 17.

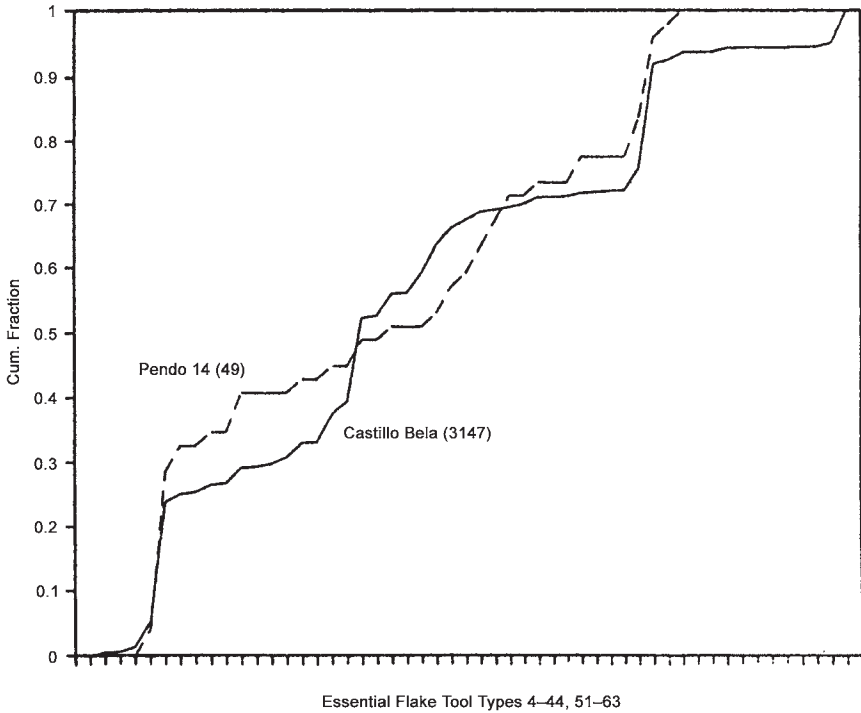
The cumulative percentage list for Upper Level 17 was so strikingly similar as to be nearly identical to the Pech de l'Azé 4 collection. Bordes classified that collection as Mousterian of Acheulean Tradition, Type A, despite the fact that it lacked bifaces. Assignment of the Morín levels to this facies seemed a very reasonable possibility. The number of flake tools showing some bifacial trimming was larger in these levels than in others. The proportions of sidescrapers in most of the levels were well within acceptable ranges for the Mousterian of Acheulean Tradition (as then defined), and Bordes himself had begun to recognize somewhat broader tolerances for sidescraper thresholds than those originally specified. The proportion of denticulate tools they contained was also within the range for Mousterian of Acheulean Tradition, but too high for Typical Mousterian as that facies was defined at the time. Though none of the Morín levels had more than a very few true bifaces, we suggested that the cleaver flake might be the local equivalent of the true bifaces characteristic of this facies in France. What we proposed amounted to the recognition of a new subfacies, within the Mousterian of Acheulean Tradition Type A. Into this new subfacies we proposed putting all the Morín levels with cleaver flakes—Levels 13/14, 15, 16, and 17.



■ FIGURE 8.8. *Cumulative graphs, Typical Mousterian (2), el Pendo and Morín*

While we early realized that intergradation was the rule for these assemblages, it was only during our second season that we began to see its full implications, and to realize that the problem of finding a way to somehow encompass these assemblages in the Bordes facies classification was a meaningless academic exercise. The facies didn't really seem to exist in Cantabria. They were no more than arbitrary segments of a continuously intergrading spectrum. Of course, this was a revolutionary idea. We anticipated difficulty in convincing most prehistorians that a system, based on the work of the greatest authority in Mousterian studies, should be abandoned because of some anomalous assemblages in Cantabrian Spain. For the time being, it was necessary to do a kind of "schizophrenic" prehistory—to follow the Bordes tradition in Mousterian studies, so that we could continue to communicate with our colleagues in terms that they would accept and understand, on the one hand, and on the other, to continue to develop and present the evidence that we knew could eventually undermine that system's very foundations.

Just before the second volume of the Morín monograph (González Echegaray and Freeman 1973) appeared, Henry de Lumley, similarly faced with well-excavated collections that would not fit into any of the traditional facies defined by Bordes, circulated a classification of Mousterian industries of the French Midi that effectively



■ FIGURE 8.9. Cumulative graphs, Cantabria's most scraper-rich collections, Castillo Beta and el Pendo 14

proposed new sidescraper and denticulate thresholds for the Typical Mousterian facies; he included in that facies collections with more sidescrapers or more denticulates than the original definition would allow (de Lumley 1969-1971; de Lumley and de Lumley 1972). His scheme was received with little opposition.

In our second volume, we discussed the possibility of an alternative assignment of the Morín cleaver flake Mousterian to the newly amplified Typical Mousterian, concluding that whether or not that assignment was appropriate seemed to us to be simply a matter of preference. Unquestionably, the Morín assemblages could be classified that way. But at that point we were not at all in favor of widening the definition of the Typical Mousterian. Broadening its definition so much would make it a sort of trash can to contain anything and everything that didn't easily fit any of the other, more narrowly defined facies. So, we continued to call the levels a special Cantabrian variant of the Type A Mousterian of Acheulean Tradition. But we stressed that although either alternative classification might be used, neither was really preferable, and that the facies designations were no more than arbitrary divisions of a continuum of variability.

The Morín volumes were reviewed favorably by Mme. Bordes, who, however, took exception to the use of the designation Mousterian of Acheulean Tradition for

assemblages lacking true bifaces. There is no doubt that her critique added pressure to the evidence suggesting the alternative classification, but even before it appeared, new data from el Pendo convinced us that a classification as Typical Mousterian was more rational, even though the flake tool series in question were statistically as similar to some assemblages Bordes called Mousterian of Acheulean Tradition as they were to any Typical Mousterian.

In 1978, we published a short popularized account of the Morín work (González Echegaray and Freeman 1978). It reflected our new understanding of the nature and reality of the Mousterian facies, stemming from our analysis of the assemblages from el Pendo. It called the problematic assemblages Typical Mousterian, while again stressing that the facies were really no more than arbitrary constructs, since intergradation between Cantabrian Mousterian collections was complete and continuous. But it was not until 1980, in the publication of the el Pendo materials, that this statement was most forcefully made; there we finally insisted that the facies concept had outlived its usefulness.

■ CANTABRIAN FACIES III: NEW DATA FROM EL PENDO

In 1972, González Echegaray, who had been charged with the publication of the 1953–57 excavations at el Pendo after the death of Martínez Santa-Olalla, invited me to study the assemblages from the Mousterian levels at that site. The five usable el Pendo collections, recovered by a team including González Echegaray, André Cheynier, and both André and Arlette Leroi-Gourhan, were excavated with modern techniques and due attention to microstratigraphy; they are certainly as well controlled as assemblages from more recent excavations. As was the case at Morín, the el Pendo assemblages were both illuminating and surprising (González Echegaray et al. 1980).

With Karl Butzer, we had taken a suite of sediment samples from the site in 1969. González Echegaray and Freeman amplified that sample series in 1972. The samples were analyzed by Butzer. Unfortunately, there are no radiocarbon dates for the site. Pollen samples taken in the 1950s had been analyzed by Arl Leroi-Gourhan: only Mousterian Level 9, with too few tools to classify, provided a useful pollen spectrum, suggesting temperate mixed forest; arboreal and non-arboreal pollen are present in approximately equal proportions. Refer to Table 8.2 for the facies attributions, Butzer's climatic interpretation, and suggested geostratigraphic age for these lower levels at el Pendo.

The el Pendo Mousterian artifacts were most informative. Two facies, as newly defined following de Lumley's work, were obviously represented: the Denticulate Mousterian, present in Levels 16, 12/11, and 8D (Figs. 8.5, 8.6), and another facies, found in Level 13, with cleaver flakes (Fig. 8.8), and Level 14, without them (Fig. 8.9). In Level 16 we have a good case of an assemblage falling on the boundary between two facies. It might have been called Typical Mousterian, since it is quite similar to that from Levels 13/14 at Morín, which cannot be forced into the Denticulate facies—sidescrapers amount to 30 percent of its "essential collection." However, the

closest affinities of Level 16 are with Level 8D from its own site, and that is certainly a Denticulate Mousterian level.

The assemblages from Levels 13 and 14 at el Pendo had far too many sidescrapers to be considered Mousterian of Acheulean Tradition, but Quina types and pieces made on Levallois flakes were not at all abundant, so neither collection fit the definition of either Charentian subfacies, despite the fact that the graph of Pendo 14 is so similar to that from the single Charentian level from Cantabria, Castillo Mousterian Beta (Fig. 8.9). The only possible way these levels from el Pendo could be classified in the current facies scheme was as an expanded Typical Mousterian, of the newly recognized sidescraper-rich variety. When this classification is adopted, as now seems the best alternative for those who continue to use the facies designations for purposes of communication, it forces the reclassification of the Morín collections that are statistically indistinguishable from them, and this includes all those collections that we formerly called Mousterian of Acheulean Tradition (Figs. 8.7, 8.8). It also requires the reclassification of Mousterian Alpha at Castillo. Had it been possible to analyze the el Pendo collections before excavating at Morín, we might never have considered attributing any of the Morín assemblages to the latter facies.

■ FACIES DIFFERENCES: MORE FROM MORÍN

The facies as understood by Bordes were non-overlapping and largely nontemporal assemblage groups, whose definition was based on different proportional content of particular tool groups such as sidescrapers and denticulates. Bordes believed that the differences between the facies were not related primarily to the passage of time (despite some admitted temporal replacements) or to their adaptation to different environments, or to technologically “functional” differences between the tool groups that characterized them, but to the use of tool proportions as the stylistic markers of distinct, identity-conscious socio-cultural groups or vaguely defined “tribes.” Since no known socio-cultural groups have stressed their uniqueness by making different proportions of the same kinds of tools made by all groups, Bordes’s “stylistic” explanation for facies difference seemed unlikely. To prove the “stylistic” argument wrong required solid confirmatory evidence. Such evidence was found at Cueva Morín.

During the 1968–69 excavations, it was noted that some different tool types such as cleaver flakes and sidescrapers tended to be found in separate spatial concentrations in Level 17. If one only analyzed materials from selected grid squares, the collection looked quite Charentian, whereas if one selected other squares, the assemblage appeared to be more denticulate-rich. That suggested that had one by chance excavated in different restricted areas of the same archeological level, the partial assemblages recovered would have been assigned to quite different facies, even though all the recovered artifacts might have been made and used by a single social group. Bordes recognized that sometimes particular tools were found in spatially restricted accumulations, but insisted that if a “large enough” assemblage were excavated, those “random” differences would be evened out, and a faithful picture of the total characteristics of a whole assemblage would be obtained. However, the

spatial concentrations we discovered seemed deliberate, and there is no basis for believing that intentional differences must “cancel” each other in large excavations: if the spatial division of labor was systematic, compensating differences are unlikely to appear. The Level 17 evidence was no more than suggestive, since the sidescraper-rich squares in Level 17 were not contiguous, and the contents of several spatially segregated squares had to be combined to produce a large enough collection of “essential” tools—a minimum of 100 pieces—to produce what Bordes considered a minimally reliable graph. Later, however, we found a much more convincing case (Freeman 1992).

We had excavated a large (307 “essential” pieces) assemblage from Mousterian Level 16 from a 7-square-meter area in the northwest part of the vestibule—Level 16NW. Its cumulative percentage graph (Fig. 8.4) and indices indicated that the assemblage should be classified as sidescraper-enriched Typical Mousterian (with cleaver flakes). An adjacent part of this level, some 5 square meters in extent, was removed intact as part of a block of sediments containing an Aurignacian burial, and excavated later in laboratories of the Smithsonian Institution. This part of Level 16, designated 16UB, also yielded a large assemblage—222 essential tools—that unquestionably came from exactly the same level as the tools from 16NW. Its partial assemblage was quite different: it contained no cleaver flakes at all, and more than 37 percent denticulates, while in one small area, 40 centimeters in diameter, we found a cache of 14 Tayac points, a rare type in the rest of the level. The assemblage from Level 16UB was clearly Denticulate Mousterian, not Typical (Fig. 8.5). Yet these two large subassemblages were found in contiguous parts of a single archeological deposit, and the areas from which they were recovered were so small as to make the suggestion of simultaneous occupation of a single level by two different “tribes” untenable. The case of Level 16 was by itself adequate disproof of the “stylistic” theory. It suggested convincingly that causes for the different percentages of particular types were to be sought primarily, though not exclusively, in the economic uses to which different types of tools were put.

It is not difficult to understand why Bordes’s attempt to define stylistically significant characteristics of Mousterian assemblages did not succeed. Such characteristics may well exist. But in trying to arrive at minimal definitions of the tool types that would hold universally, Bordes relegated all the potential stylistically informative attributes of artifacts to semi-oblivion in the “descriptive narrative” that was a secondary accompaniment to the studies of cumulative graphs and characteristic indices that were the principal focus of his publications and the basis of his facies assignments. The facies could not be stylistic variants because he had virtually eliminated stylistic attributes from consideration at the outset.

Bordes was proud of the fact that traditionally, folding knives used in his beloved Carsac had wooden handles that were differently shaped and decorated from those made, say, in the Lot or the Paris basin. Yet in classifying stone tools he insisted that a knife should be defined ignoring decorative or regional differences: a knife should always be called a knife, no matter where, when, or by whom it was made. He did not seem to recognize that applying the same rule to the classification of modern

folding knives would ignore just those stylistic variations that he so enjoyed in his own cultural tradition.

■ THE FACIES QUESTION DISSOLVED

In the Morín and el Pendo monographs, we showed how the statistical procedure called the Kolmogorov-Smirnov test may be used to produce a distance-like measure for discussing how similar or different assemblages are, and stressed that it is superior to other distance measures—it takes into account sample size and the contribution of random error or chance to assemblage difference, and is a more powerful measure of difference than any alternative. That statistic, Kolmogorov-Smirnov *D*, is a measure of the probability that chance alone could produce a difference as large as the one we actually observe. The smaller the value, the more likely it is that the difference observed is not “significant,” but is due just to chance. At least one in every five pairs of samples derived from a single original population will be so different by chance that their *D* value will reach 1.07. When *D* is 1.36 or greater, there is less than one chance in 20 that the samples could come from a single population, and when it reaches 1.63, the chances drop below one in 100. Most people would say that one chance in a hundred is pretty long odds—that there is very good reason to believe that samples this different are really different for important reasons other than chance. When samples differ by chance alone, they are very “close” in their characteristics; when they are very different, and *D* is large, they are very “far apart.” That is the reasoning that supports using the Kolmogorov-Smirnov value as a distance measure.

Applying the test to the Cantabrian Mousterian provides objective evidence that the facies are arbitrary constructs. Table 8.3 shows the Kolmogorov-Smirnov *D* values for the 15 most reliable Mousterian collections from the Autonomous Region of Cantabria—all the well-excavated assemblages from el Pendo and Morín, and the two large collections from Castillo. *D* values too low to indicate that chance alone could produce the observed differences at least as much as once in 20 cases are in bold face. (One borderline case is underlined.) For our purposes, the difference between assemblages with so low a *D* is insignificant.

It is easy to see that the assemblages intergrade completely—there is no group of assemblages whose members are like one another but consistently different from all the rest. Mousterian Alpha is only like Morín 13/14, but on the other hand Morín 13/14 is also like nine other levels. Mousterian Beta is only like Pendo 14, but Pendo 14 is also like Pendo 13 and Morín 15. No clear groups of similar assemblages stand out, and there are no real gaps separating any assemblage or set of them from the rest. The relationships between these assemblages could be shown diagrammatically as a series of linked rings, forming a continuous, complex chain, as we did in the monograph on el Pendo.

Figure 8.10 is another graphic depiction of assemblage relationships: a dendrogram produced by a single-linkage cluster analysis based on the Kolmogorov-Smirnov values. While far from perfect—alternative clustering procedures such as

mean linkage or complete linkage algorithms produce somewhat different arrangements—clustering with the Kolmogorov-Smirnov measure does better express overall similarity between lists including all essential flake tools than can any distance measure that is based on reduced type lists. The resulting dendrogram is a more reasonable and realistic expression of similarities than the one published by Cabrera and Bernaldo de Quirós (1992: 107), and unites collections that would have been considered similar by Bordes. All cluster analyses have the disadvantage that they compress multidimensional difference into two dimensions, and since they must by definition produce groups, they also suggest to the unwary classifier that the resulting groups must be real—that is, separated by significant gaps—which is not always true, and is certainly not the case here.

Assemblage intergradation is in fact sufficiently obvious when the cumulative diagrams for 16 Mousterian assemblages are drawn together on the same chart, as is done in Figure 8.11. The figure simply does not show the modal clusters of graphs one would expect to see if the facies were really different groups of assemblages—no significant tendency for clustering appears. The “facies” are in fact only arbitrary segments of a continuously intergraded series. Each assemblage curve is just another somewhat idiosyncratic part of the intergrading spectrum.

The conclusion is obvious. The facies as Bordes defined them—as mutually exclusive, well-differentiated modes of proportional representation of particular artifact types—don’t really exist: they are arbitrary constructs of the classifier. There is no sense in searching for the causes or correlates of facies differences: if they don’t exist, they have no causes.

Bordes’s systematization was especially fruitful. Yet, as long as we continue to work exclusively within the framework he defined for us, we shall be hampered by the limits and inadequacies they impose. His vision of the facies, one of his greatest analytical accomplishments, is now outmoded. Unless it is abandoned, there can be little further progress in Mousterian research.

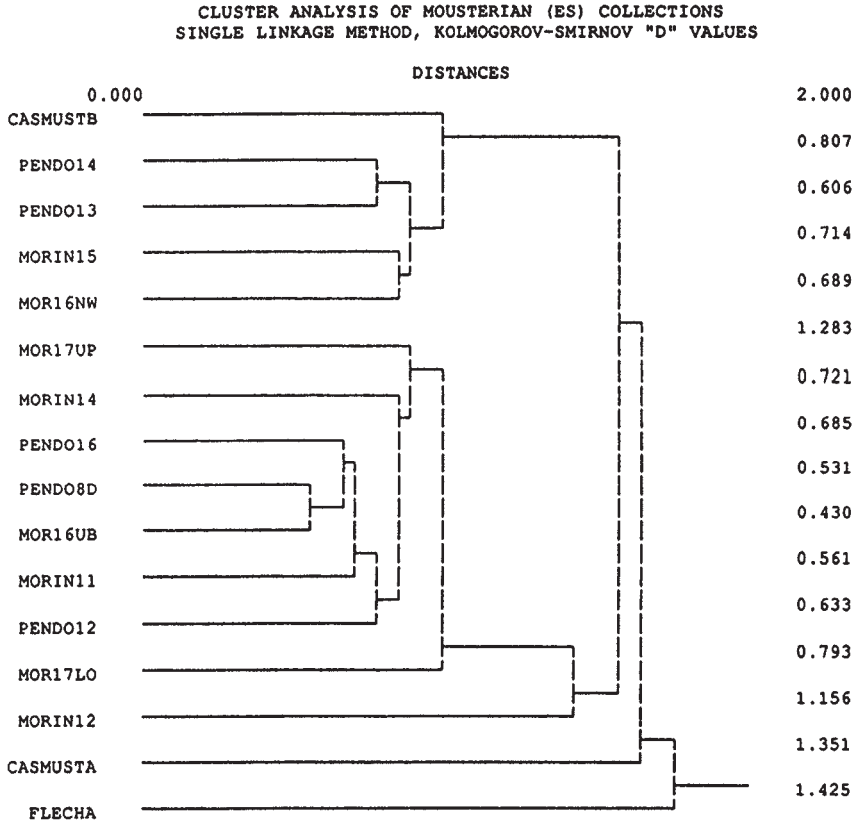
■ DIMENSIONS OF ASSEMBLAGE VARIABILITY

Does this mean that Mousterian studies are fruitless, or that there is no meaningful way to classify assemblages? Not at all. Bordes’s facies were only an analytical construct. That they don’t exist doesn’t mean that the Mousterian *assemblages* have disappeared: they are as real as ever. Some dimensions of their variability were inadequately explained using Bordes’s analytical framework that simply means that we need to develop other procedures and formulate better definitions to move ahead. Bordes’s definition of the facies was an essential step toward understanding Mousterian interassemblage variation: a valuable working hypothesis that advanced the discipline despite its errors. It stimulated the very research that made it possible to show that the differences he thought to be most important were not the result of stylistic stressing of group identity, by long-lived, ethnically distinct socio-cultural groups, as he postulated. Through the very process of that invalidation, we are led to a deeper understanding of differences between assemblages, and to develop new and

■ **TABLE 8.3. Kolmogorov-Smirnov Delta values from comparisons of fifteen Mousterian collections**

(Boldfaced numbers indicate no significant difference at the <.05 probability level.)

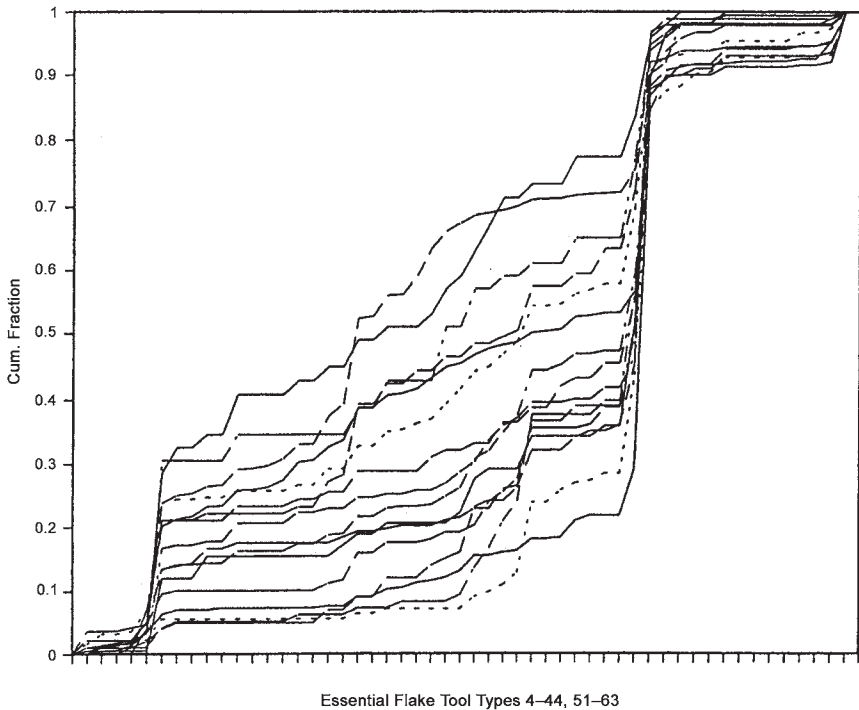
	Site and Level									
	CASMOUSTA	CASMOUSTB	MORIN 11	MORIN 12	MOR 13-14	MORIN 15	MOR 16 NW			
CASMOUSTA	0									
CASMOUSTB	8.274193	0								
MORIN 11	3.499146	6.440248	0							
MORIN 12	5.892446	9.383481	1.549176	0						
MOR 13-14	1.351082	3.349851	1.116645	2.095106	0					
MORIN 15	1.543199	2.069466	2.265946	3.333333	1.283053	0				
MOR 16NW	2.170137	4.494186	2.584963	4.266291	1.293794	0.688915	0			
MOR 16UB	3.550531	6.648985	0.842113	1.650650	0.884350	2.110979	2.529289			
MOR 17UP	3.614483	7.551477	1.237471	3.079632	0.720618	1.904490	2.001075			
MOR 17LO	3.498275	5.580693	0.859603	1.156364	1.618063	2.648867	2.744386			
PENDO 8D	1.829069	3.440901	0.561311	1.335959	0.684905	1.658820	1.572949			
PEN 12-11	2.916647	5.038244	1.003900	1.213724	1.359342	2.274141	2.209082			
PENDO 13	1.430595	1.441947	2.294406	3.091354	1.341104	0.714018	0.834862			
PENDO 14	1.855075	0.806718	2.851347	3.878080	1.958012	1.255727	1.594103			
PENDO 16	2.292711	5.043036	1.163315	2.570491	0.693460	2.079250	2.474486			



■ FIGURE 8.10. *Dendrogram of relationships among sixteen Cantabrian Mousterian collections; right column shows distances separating clusters (scaled from 0 to 2.0)*

better schemes for their classification. Any future advance in our understanding of the Mousterian will inevitably be built over the foundation laid by François Bordes. And, since many prehistorians, slow to abandon old and accepted ways, were trained to use them, the facies designations will continue in use, for some time, as a means of communicating about the characteristics of assemblages.

Though the lines between the Mousterian facies have disappeared, inter-assemblage variation is by no means random. Even with Bordes type definitions, it is possible to detect a considerable degree of patterning in that variation. Meaningful regularities can be observed in differences between his graphs of assemblages and their characteristic indices. Bordes knew more about Mousterian collections than any other prehistorian; his typology and indices reflect factors systematic empirical observations show to be important. Those reflections can, however, be improved and their reasons clarified.



■ FIGURE 8.11. *Cumulative graphs of the same sixteen collections, illustrating their intergradation*

One fruitful approach to the design of better methods of interassemblage comparison is statistical. Multivariate tests on collections (“Factor Analyses” on rank-order correlation coefficients expressing the relationship between Bordes’s types) long ago suggested that the Mousterian assemblages are made up of different groups of functionally related tool types, and that those types tend to vary in abundance in strict relationship to the frequency of other types in their group. Between groups, no such relationship holds. Three principal groups can be defined. The first consists of many kinds of sidescrapers; the second of notches, notched triangles, denticulates, burins, and alternate burinating becs (perforators may also be affiliated with this group); and the third of cleaver-flakes, bifaces, knives, and, sometimes, choppers and Levallois flakes. Endscrapers, some truncations, and Tayac points may constitute another group (in tests on some levels, they appear to be associated; if different assemblages are included their relationships change).

Much of the difference in content of (Bordes’s) tool types in Cantabrian assemblages is adequately expressed by their linear placement along an axis representing increasing abundance of sidescrapers in one direction and increasing abundance of notches, denticulates, and related tools in the other (even though the tool types in these two groups do not really vary in inverse fashion). This axis is very robust.

It appears in virtually all analyses of assemblage differentiation: cluster analyses, “Factor” analyses, Kolmogorov-Smirnov test results, etc., in tests run by different analysts using data from different Mousterian sites in France and Spain, and it was a principal dimension of variation in Bordes’s facies classification. Adding another axis perpendicular to the first to represent abundance of the cleaver flake group in one direction and endscrapers, etc., in the other permits a usable approximate three-dimensional representation of relationships between all the assemblages, even though it is not a very “realistic” spatial depiction of those relationships. Presumably a more suitable representation can be developed when well-excavated assemblages are recovered from Castillo and other sites in the not-too-distant future. Such representations must eventually be replaced by more adequate analyses based on other approaches to typology, and other ways of looking at assemblages.

If we are ever to understand variation in Mousterian assemblages, artifact classification must be reformed. The Bordes typology is itself as good a point of departure as any; though accused of subjectivity it rests on a solid empirical foundation and Bordes’s profound knowledge of flint-knapping. From that starting point, we may hope to condense types that are simply variants of each other, to define new types as necessary, and to reincorporate those attributes excluded by Bordes that are most likely to bear the load of stylistic information he sought. Characteristics considered by the new typology will include many details of morphology that he downplayed—details such as the asymmetrically “skewed” appearance of flakes from Olha that makes them look so different from those found at Castillo, the remarkable straightness of scraper and point edges in some collections, or the peculiar “spire-ended” shape of points from some sites in the Middle East. A closer search for patterning in the types of working edges combined on single pieces such as déjeté sidescrapers is essential, and some new combination tools will be recognized. Before his untimely death, Bordes foresaw the need to admit new types for combined perforator/sidescrapers and notch/sidescrapers. Regional peculiarities in the production of retouch not due to constraints of raw material may betray shared toolmaking traditions. Careful studies of wear-polish and the effects of resharpening can add much functional information. Our experience suggests that morphological, rather than metric, attributes may prove to be the more stylistically informative. These are only a few suggestions from a much larger list of needed modifications.

The powerful computational means now available to anyone with a desktop computer and adequate software permit the objective definition of types and attributes by applying such statistical procedures as the Mann-Whitney test and discriminant function analysis (as de Heinzelin suggested) to multiple features of tools from large unmixed assemblages. Analogous “attribute clustering” procedures were applied with some success by Movius and his students at Pataud—only available sample sizes limited what they could achieve. Multivariate procedures for assemblage comparison will provide the basis for a better classification of whole assemblages. Studies of the spatial distribution and frequency relationships of artifacts and contextual data will show how tasks were organized, and all these lines of evidence will converge to indicate just what those tasks were. Working out a step at a time

from the types identified in single assemblages, to other assemblages of the same complex in a single site, then to assemblages at neighboring sites in an environmentally similar locality, and then to successively larger and more diverse regions, will maximize results, permitting a better grasp of the real nature of interassemblage difference than has ever been available.

I have tried to tell a twofold tale. One part of my story chronicled fruitless attempts to rewrite the patchwork *précis* of the facies concept, as new contradictions kept appearing. The successive reclassifications of the Cantabrian assemblages were an attempt to repair a paradigm that was not just incomplete or dented, but irremediably broken. The other part was a brief history of some of the new excavations that provide the documentary facts from which a modern theoretical synthesis will be written.

The image of the kaleidoscope, used before, may seem an apt analogy for the story of Mousterian studies in Cantabria. There is little that seems stable in the way shifting facies designations have been applied to the single Castillo Alpha collection. The nonspecialist reader may well get the impression that Mousterian studies are little more than a game played by silly children. That is certainly not the case. Paleoanthropology is not simply play with a kaleidoscope of imagined interpretations. It is, instead, the search for reflections of other worlds, remote from us but as real as our own, in a dull and tarnished mirror.

The changes I have chronicled were not just different and equally valid glosses of a fictional text, to be judged by the quality of imagination invested in each. Nor were they the inevitable result of classificatory difficulties inherent in the artifacts: they stemmed instead from inappropriate preconceptions, or incorrect “hypotheses,” if you prefer. The facies classification was a hypothesis; we tested it; it was wrong.

Efforts spent trying to patch the old, broken synthesis were not time wasted. With each patch we learned something of value about Mousterian assemblages. The picture that appears as the old hypothesis crumbles away will be more complex, but at the same time somewhat better focused, more consistent and coherent, and different as well, because its constituent elements are changed. No amount of play with a kaleidoscope could accomplish that.

Progress in the last thirty years has led us to a better appreciation of the nature of the Mousterian complex in Cantabria. Research is generally much better informed, more meticulous, accurate, and reliable, and at the same time more sophisticated, than it has ever been. Our investigations helped reveal the inadequacies of what for many years seemed a viable and robust classification; at the same time, they have begun to unveil the still-hazy outlines of a new, more realistic Mousterian synthesis. Though we cannot yet see many of its details, new research in Cantabria and elsewhere will ensure that they will not remain hidden for long. It is a good time to begin studying the Mousterian.

NOTES

1. A number of others have also studied the Spanish Mousterian, or examined some Cantabrian collections. There is often substantial disagreement between

their classifications of these assemblages. That my classification sometimes differs from Cabrera's is understandable. We did not examine exactly the same collections. Differences between my classifications and those of others may require other explanations. Major discrepancies are not inevitable, or due, as Straus (1992) would have it, to inherent subjectivity in the recognition of certain types. Bordes took pains to train me to replicate his own classification with a minimal margin of error. Apparently subjective aspects of the procedure turn out to have a sound empirical base, that cannot be appreciated from the available written descriptions: a real apprenticeship is required to learn correct procedures. Some who have studied Spanish Mousterian assemblages never received adequate training. Without it, certain consistent typological errors are inevitable. They include: (1) Misunderstanding of Levallois technique. It is not platform faceting, and Levallois flakes must be distinguished from irregular flakes from disc cores, large regularly shaped ordinary flakes, or flakes from "bifacial trimming" of disc cores or bifaces. (2) Failure to recognize rarer types, such as notched triangles, alternate burinating becs, bifacial leaf-shaped pieces, "hachoirs," etc. (3) Misunderstanding of burination, and particularly confusion of narrow projections or broken surfaces with burins. (4) Where it occurs, confusion of geological crushing with retouch, especially denticulation. (5) Misunderstanding of Quina retouch and Quina scrapers. Not all steep, convex scrapers are Quina. Not all step-flaked scrapers are Quina, even when they are convex. (6) Misunderstanding of distinctions between platform regularization, faceting, and other regular retouch on flake butts, and of when the latter may legitimately be classified as tool-forming retouch.

2. This problem has not been satisfactorily resolved: collections are often harder to locate and study now than ever before, despite the repatriation of Spanish collections by the French. Victoria Cabrera, in her truly superb attempt to draw together and publish all existing information on the Castillo excavations by Breuil and Obermaier (Cabrera Valdés 1984: 143–98), was able to classify only 705 flake tools from Mousterian Beta (Level 22) and 681 from Level 20 (Mousterian Alpha). These were mostly pieces that had been returned by the French to the National Archeological Museum in Madrid. She only saw a tiny fraction of the much larger collection in the Santander Provincial Museum, and learned that by 1979 much of the Castillo material had seemingly lost provenience data while stored for remodeling of that museum. Klein and Cruz-Urbe (1994) classified all Castillo faunal remains in Madrid, but saw none of the pieces classified earlier by Altuna, who must have had access to part of the collections housed in Santander. Since my research in 1962, no prehistorian has been able to locate and examine the whole artifact collection from any Castillo level, and it is not clear that it will ever again be possible.

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The Mousterian, Present and Future of a Concept

(A PERSONAL VIEW)

INTRODUCTION

The “Mousterian” is a stone artifact industrial complex restricted mostly to Europe (and in most characteristic form to Western Europe) and parts of Western Asia and North Africa. If we disregard the difficulty of differentiating it from the latest Acheulean and affiliated industries, the Mousterian seems first to appear during the Last Interglacial, more than 130,000 years ago, and to be replaced by Upper Paleolithic industries some 40,000 years ago. The term Mousterian was first applied by Gabriel de Mortillet (1869, 1872) to collections from the site of Le Moustier (Dordogne, France), made by Edouard Lartet in 1864 (which the discoverer assigned to the Epoch of the Mammoth). The first Mousterian artifacts excavated in Spain were those from Covalejos (1872) and Fuente del Francés (1880), found by E. de la Pedraja, who understandably did not give them that newly minted designation. For many years, the definition of Mousterian assemblages was complicated by the use of systems of artifact classification that included overlapping categories and such classificatory paradoxes as “round points.” We owe to the late François Bordes and his colleagues the systematization of artifact type definitions and the elimination of such obvious absurdities.

The complex is for the most part distinctive, although at the early end of its range assemblages intergrade so thoroughly with the latest Acheulean industries

and such oddities as the “proto-Quina Tayacian” that any boundary between them is blurred, and some final Mousterian industries have so many backed knives and other supposedly “Upper Paleolithic” tool types that they, too, seemingly intergrade with such early Upper Paleolithic complexes as the Chatelperronian. At its inception, it is quite difficult to draw any clear-cut distinction between Mousterian and earlier industrial complexes, and perhaps this situation should lead us to reexamine the traditional definition of the Mousterian as a distinctive complex. However, at the recent end of its trajectory, careful attention to the choice of raw materials and flaking techniques, and the nature of retouch and its by-products, should help to differentiate the Mousterian from true Upper Paleolithic complexes. Not just backed knives but several other tool types ordinarily thought characteristic of Upper Paleolithic industries also occur in perfectly ordinary Mousterian assemblages, though not usually in the relative numbers they later assume. Such tools are burins, endscrapers, truncations, backed elements other than “knives,” blades, and even the occasional bladelet. While these precocious types occur in the Mousterian, and Chatelperronian levels in general contain a few types usually associated with the Mousterian, there is little room for confusion of the Cantabrian Mousterian with either Chatelperronian or Aurignacian industries. The Chatelperronian levels at Morín and el Pendo (the latter atop an Early Aurignacian level) are quite different in lithic contents—tool type proportions, choice of raw materials, flaking techniques—from the Mousterian occupations that precede them.

There are Mousterian flakes that show apparent bladelet removals, often grouped at the flake butt near projecting ridges between flake scars. Some authorities interpret them as “bladelet cores,” but the removals may instead have simply been intended to thin the thick ridges, rather than aiming to produce bladelets. (The infrequency of finds of the tiny bladelets themselves is likely due to the fact that the ultra-fine screens needed to recover them were not used by the excavators.) Some assemblages contain high proportions of flakes from preformed cores (so-called Levallois flakes, points, or blades), while others have rare Levallois flakes or none at all. Since Levallois technique is relatively wasteful of raw material (a good deal of material may be lost through the process of core preparation), although it is conservative of the effort needed to bring the finished flakes to final form, after preparation intended to facilitate the sequential removal of several similarly shaped flakes, it is most likely to abound where (and when) there are natural exposures that provide ready access to suitable raw material in large sizes. Bifacial tools may be present, and in some assemblages, even abundant.

A major difficulty in studying Mousterian assemblages has been establishing their respective age. Suitable radiometric techniques that can be used to determine the actual age of Mousterian materials found in terrestrial sediments, within tolerable limits of accuracy, are deplorably almost nonexistent. For earlier periods such techniques as potassium/argon dating and uranium series dating are available, and though the dates they provide have relatively large margins of error, those margins are acceptable when the dated materials themselves are very old. For later periods, the radiocarbon technique is highly satisfactory, particularly when performed with

accelerator mass spectrometry, although ages beyond 50,000 years estimated with this technique are suspect, and are probably best regarded as minimum estimates. AMS radiocarbon dating has demonstrated that in Cantabrian Spain early Upper Paleolithic assemblages make their appearance about 40,000 years ago: as early as in Central Europe and several thousand years earlier than was originally thought. (The evidence that the first Upper Paleolithic industries in Cantabria date back that far is incontrovertible, as is proved by the series of good dates on carefully excavated Aurignacian Level 18 at the cave of Castillo.) In the intervening period, such relatively unproven or questionable techniques as amino acid racemization, hydration, fission-track, and thermoluminescence dating have been applied, yielding what are at best “consensus dates”: i.e., they seem not to disagree with the preconceived ideas of age held by many specialists, or at worst are so wildly unreasonable that they are dismissed by all scholars. Mousterian stone tools may be large and crude in appearance. For that reason, assemblages from quarry/workshop sites where flawed and abandoned roughouts abound, or others with a large proportion of large, “heavy-duty” pieces that may probably have been expedient tools, are often mistakenly attributed to the Mousterian solely on the basis of their primitive appearance. This problem is not unique to the Mousterian; collections of Acheulean tools have been inappropriately assigned early “dates” on the basis of their relatively crude appearance, and to a lesser extent this erroneous practice extends to some Upper Paleolithic assemblages as well.

Internally, the Mousterian complex is heterogeneous. In Western Europe, François Bordes distinguished four assemblage types or “facies” within it, largely based on high percentages of sidescrapers at one extreme, denticulates and notches at the other, and the presence or absence of certain diagnostic implements. The idea of the “facies” was derived from Bordes’s geological training, but strangely, although true geological facies intergrade, Bordes insisted that the Mousterian facies were mutually exclusive, non-overlapping entities. To define them, one suspects that Bordes had to ignore or dismiss as “mixed” some well-excavated, intergrading assemblages. He called the four facies the Charentian (with sidescraper-rich subtypes Quina and Ferrassie, differentiated on the proportional representation of Levallois technique), the Denticulate Mousterian (rich in notched and serrate-edged tools), the Mousterian of Acheulean Tradition (subtypes A with handaxes and B in which handaxes were replaced by backed knives), and the “Typical Mousterian” (both of the last-mentioned facies as originally defined have only moderate quantities of sidescrapers or denticulates). Our own research on Spanish Mousterian assemblages and that of H. de Lumley in Provence indicated years ago that these “polar opposites” actually intergrade quite completely, and so designations for what Bordes would have thought to be “anomalous” assemblages, such as the “sidescraper-rich Typical,” have had to be invented. (This seems unsatisfactory, since it makes of the Typical facies a catchall into which everything that will not fit one of the other facies is crammed.) However, I believe that the practice we shall have to follow to further Mousterian studies will involve the eventual abandonment of the facies concept and a concentration on the development of new ways of classifying artifacts, toolkits, and individual assemblages.

PROBLEMS OF CLASSIFICATION

Until recently, so little has been known from Spanish sites about the relationship between Mousterian tool types or assemblages and paleoenvironmental conditions that most of what I can say about the complex from personal experience comes from the study of the stone artifact assemblages.

Anyone who has experienced the problems of classifying stone artifacts from both Middle Paleolithic and Upper Paleolithic sites cannot fail to have noted that the latter are far and away the easier tools to classify. Partly, that is due to the number of multiple-edged Mousterian artifacts that seem to show no significant tendency for particular types to combine. Partly it is due to the fact that some working edges are ambiguous, so that it would be as easy to call them denticulates as sidescrapers. Bordes dealt with such pieces by assigning them to the type that appeared “better made” or “less common” in the context of the assemblage being classified. But these are unsatisfactory solutions, being both subjective and hard for others to replicate. Probably a more objective classification of such artifacts can be facilitated by the use of techniques of fuzzy logic/neural networks (von Altröck 1995, 1997).

In fact, the overall shapes of Mousterian flake tools are simply not as tightly standardized as they become later, in the Upper Paleolithic. In fact, in Mousterian assemblages the shape and size of retouched *working edges* are better standardized than are whole tools. The great variability of artifact size and form that was tolerated in Mousterian industries is clearly shown by the variability of their sizes. Measuring individual tools in samples of a single well-defined tool type collected from different Paleolithic sites, or other sites occupied by modern *Homo sapiens sapiens*, one discovers that most often the difference between the means or medians of the samples is statistically significant: when tools made by fully modern people are compared, the only exceptions seem to be those categories of such coarse stone implements as hammerstones or mortars and pestles, whose size is determined by constraints imposed by considerations of manageability and the requirements of the physical activity that employs them. In such cases, adding samples from different sites together increases the variability and standard deviation of the collection, a clear indication of dissimilarity of the individual samples.

But with products of Neandertals, the situation seems to me to be quite different. Samples of well-defined types from any single site are very variable in their dimensions and other characteristics, but when discrete samples are added, it is as though they all came from the same parent population. One Cantabrian Mousterian type offers an informative example. At several sites, including Cueva Morín, el Pendo, and most importantly el Castillo, Mousterian assemblages that contain characteristic cleavers made on flakes have been recovered. While cleaver flakes seem reminiscent of earlier Acheulean industries (so much so that in 1961 Pierre Biberson called what may be a cleaver flake-bearing Mousterian from the “brecchioid limestones” at Cap Chatelier at Sidi Abderrahman “Acheulean Stage 8”), the assemblages with cleaver flakes from Cantabria do not seem to be very ancient within the Mousterian complex. (Bordes misreadingly baptized these Cantabrian assemblages “Vasconian”;

■ **TABLE 9.1.** *Descriptive statistics, Cantabrian cleaver flakes: Lengths (cm)**

<i>Variable</i>	<i>N</i>	<i>Mean</i>	<i>Median</i>	<i>Tr Mean</i>	<i>St Dev</i>	<i>SE Mean</i>
Elpl	25	10.383	10.250	10.357	1.729	0.346
Morl	62	11.135	11.200	11.126	1.633	0.207
Castl	156	10.758	10.800	10.752	1.696	0.136
Alcl	5	10.920	10.500	10.920	1.721	0.770
<i>Variable</i>	<i>Min</i>	<i>Max</i>	<i>Q1</i>	<i>Q3</i>		
Elpl	7.250	14.100	9.450	11.700		
Morl	7.100	15.000	9.700	12.415		
Castl	6.400	15.100	9.400	12.000		
Alcl	9.400	13.700	9.550	12.500		

* (Elpl = el Pendo; Morl = Morín; Castl = el Castillo; Alcl = Alcedo).

These calculations were made using the Minitab© statistical software program. Measurements are maximum lengths of specimens parallel to the “axis of symmetry,” based on artifacts in museum collections from older excavations, except in the case of Cueva Morín, where specimens excavated during the 1960s have been added to the museum pieces.

in addition to the name, which wrongly suggests that they are common in Basque country, the term suggested inappropriately that the assemblages were alike in many ways, when in fact they are internally heterogeneous.) When we consider the length measurements of (unbroken) cleaver flakes from the four Cantabrian collections discussed below, the lack of standardization of measurements and the similarity of collections is obvious.

As the table (Table 9.1) of descriptive statistics shows, the means, medians, and maximal and minimal values for lengths of cleaver-flakes from four Cantabrian sites all seem quite similar to one another, despite the fact that the smallest collection, that from Alcedo, contains only five pieces, and the largest, that from Castillo, has 156. Since so many of the pieces are from older collections, and since it could not even be assumed that the underlying distribution of lengths was normal and unimodal, I chose to compare collection medians rather than means as measures of central tendency, using the non-parametric Mann-Whitney test instead of the more usual “Student’s t.” (The use of the “t”-test would probably have been justifiable, as the distribution of lengths in the large Castillo collection shows, but the results would not be appreciably different.) The tests (Table 9.2) show that none of the collections differs from any of the rest as much as they would be expected to differ by chance alone (at the 0.95 level). The two largest collections, that from Morín (62 pieces) and that from Castillo, are no more different than one would expect samples from the same population to be nearly 15 percent of the time.

I have also identified what I believe to be a series of trimmed tools in bone from Mousterian levels at Morín and el Pendo (Freeman 1978, 1980; González Echegaray, Freeman et al. 1971, 1973). The fact that the bone supports for the more or less standardized retouched edges are not regularly patterned has led others who think in terms of more regular bone and antler types of the Upper Paleolithic (where crude bone tools also exist, but are often ignored) to challenge that identification. But (1)

the pieces in question are not simply crushed by trampling; (2) the only creature capable of mouthing the larger bones would be the spotted hyena, which is absent entirely from the levels with “bone tools” at el Pendo, while 2 premolars and a coprolite are its only remains in one of the two Morín levels with such pieces—convincing evidence that hyenas denned in any of these occupation levels is lacking; (3) some Upper Paleolithic levels, such as Level 5 at Cueva Morín, also have hyena remains, but they lack these kinds of “worked bones” entirely; (4) the pieces identified as deliberately fabricated tools closely parallel in frequency the proportional representation of analogous stone tool types from the same levels; and most conclusive of all, in my opinion, (5) the identification of deliberate retouch on these pieces was

■ **TABLE 9.2.** *Mann-Whitney U test for difference between medians**

Elpl	N = 25	Median = 10.250
Mor	N = 62	Median = 11.200
Point estimate for ETA1–ETA2 is –0.700		
95.1 percent CI for ETA1–ETA2 is (–1.700, 0.100)		
W = 921.5		
Test of ETA1 = ETA2 vs. ETA1 not = ETA2 is significant at 0.0950		
The test is significant at 0.0949 (adjusted for ties)		
Cannot reject at alpha = 0.05		

Elpl	N = 25	Median = 10.250
Castl	N = 156	Median = 10.800
Point estimate for ETA1–ETA2 is –0.400		
95.0 percent CI for ETA1–ETA2 is (–1.200, 0.400)		
W = 2016.0		
Test of ETA1 = ETA2 vs. ETA1 not = ETA2 is significant at 0.2878		
The test is significant at 0.2877 (adjusted for ties)		
Cannot reject at alpha = 0.05		

Elpl	N = 25	Median = 10.250
Alcl	N = 5	Median = 10.500
Point estimate for ETA1–ETA2 is –0.300		
95.5 percent CI for ETA1–ETA2 is (–2.250, 1.200)		
W = 381.5		
Test of ETA1 = ETA2 vs. ETA1 not = ETA2 is significant at 0.7596		
The test is significant at 0.7594 (adjusted for ties)		
Cannot reject at alpha = 0.05		

Morl	N = 62	Median = 11.200
Castl	N = 156	Median = 10.800
Point estimate for ETA1–ETA2 is 0.400		
95.0 percent CI for ETA1–ETA2 is (–0.150, 0.900)		
W = 7,396.0		
Test of ETA1 = ETA2 vs. ETA1 not = ETA2 is significant at 0.1489		
The test is significant at 0.1488 (adjusted for ties)		
Cannot reject at alpha = 0.05		

continued on next page

■ TABLE 9.2—*continued*

Morl	N = 62	Median = 11.200
Alcl	N = 5	Median = 10.500

Point estimate for ETA1–ETA2 is 0.300
 95.1 percent CI for ETA1–ETA2 is –1.301, 2.001)
 W = 2,119.5
 Test of ETA1 = ETA2 vs. ETA1 not = ETA2 is significant at 0.7930
 The test is significant at 0.7929 (adjusted for ties)
 Cannot reject at alpha = 0.05

Castl	N = 156	Median = 10.800
Alcl	N = 5	Median = 10.500

Point estimate for ETA1–ETA2 is –0.100
 95.0 percent CI for ETA1–ETA2 is –1.700, 1.500)
 W = 12,630.0
 Test of ETA1 = ETA2 vs. ETA1 not = ETA2 is significant at 0.9573
 The test is significant at 0.9572 (adjusted for ties)
 Cannot reject at alpha = 0.05

* These calculations were made using the Minitab® statistical software program. Measurements are maximum lengths of specimens parallel to the “axis of symmetry,” based on artifacts in museum collections from older excavations, except in the case of Cueva Morín, where specimens excavated during the 1960s have been added to the museum pieces. ETA is the symbol used for the median.

verified by a faunal expert, Jesús Altuna. It seems likely that the variability of patterning of overall shapes and sizes of these pieces has been the major—perhaps the only—obstacle to their acceptance as tools.

The “looseness of patterning” I have noted in connection with artifact design and production is also observable in other aspects of Neandertal behavior. Spatial differentiation of activities is not as well developed as it became in the Upper Paleolithic. At Cueva Morín, the demarcation of different areas within the occupation of Mousterian Upper Level 17 is not nearly as crisply marked as it became later, in Earlier Aurignacian Level 8a (Freeman 1993). Despite the presence of remnants of buildings in both levels, the structure of the Aurignacian level is more regular, and simpler, than it was in the Mousterian level, and the areal differentiation of the distribution of different “toolkits” is much better demarcated.

Since their definition, most of our interest has been focused on Bordes’s facies and a diligent search for the causes of facies difference. In the process of learning about those causes, we have found that we were chasing an illusion. I believe that to progress in Mousterian studies we must shift our attention away from the supposed Mousterian “facies,” and even away from the whole assemblage as the unit of study (though we must still take into account each and every artifact from every level). Instead, in future our attention should better be focused on identification and study of the *toolkits* that make up each whole assemblage, on the relations between them and their relationships to contextual information such as sediments and biotic remains, and on the positions (activity areas) that the toolkits occupy within an occupation. The idea that if whole excavated assemblages are studied they will

document all the activities undertaken within a level is fallacious. Even if tool types were homogeneously distributed over a living floor (and they never are), we could still not recover those perishable parts of the inventory that are almost certainly important to the interpretation of represented activities, and in any case archeologists virtually never excavate an occupation level in its entirety.

There are two reasons that I believe that Bordes's facies concept will have to be abandoned. First of all, the facies are now known to be arbitrary constructs of the classifier, imposed on assemblages that actually seem to intergrade completely, as least in my own experience with Cantabrian sites (Freeman 1994). Secondly, it is now known that well-excavated assemblages actually occupy somewhat different positions along a continuum that has denticulate-rich assemblages at one extreme and sidescraper-rich assemblages on the other, and that Bordes's interpretation of the causes of facies difference as stylistic rather than functional was fallacious. Paradoxically, while in his artifact classification Bordes virtually eliminated most possibly stylistic attributes of stone tools from consideration, insisting, so to speak, that a knife from any period or part of the world should be called by the simple typological designation "knife," and regarding the attributes that set knives from one part of the world off from those used in another as "accidental," he nevertheless regarded the differences between his "facies," which are after all just groups of tool types, as being the *stylistic* manifestations of group identity used by different "tribes," who signaled their uniqueness by the proportions of the different types of tools that they made and used. Such a cumbersome means of marking intertribal difference is unknown to modern anthropologists who have studied living groups, who signal their identity by signs such as body-marking, clothing, headdress, adornment, or banners or the marks they put on easily decorated items such as animal hides, textiles, ceramics, bone, and wooden artifacts, etc. Some of these signs can easily be seen at a distance. If further proof that the facies cannot be the tool groups distinctive of different "tribes" were needed, it is provided by Level 16 at Cueva Morín, where large samples of tool types that Bordes would have had to assign to different facies (307 "essential" flake tools from the Denticulate and 222 from the Typical Mousterian) were recovered from a single contemporaneous occupation in areas far too small (3 square meters as opposed to 7 square meters) to have been possibly inhabited at the same time by two different tribes. In such cases, the likely explanation of assemblage differences is functional, not stylistic in Bordes's sense.

■ THE AUTHORS OF THE MOUSTERIAN

So much for the nature of the artifactual evidence. What about the physical type of the people who made Mousterian tools? The authors of Mousterian assemblages are almost all assumed to have been Neandertals. There have been indications at some sites of a supposed "co-existence," and perhaps even a genetic continuity of Neandertals and fully modern *Homo sapiens sapiens*. However, more recent studies seem to show that Neandertals are not in fact ancestral to modern humans, but a specialized side branch of the human family tree. Despite the opinions of some

authorities, the nature of the artifacts they made tells us very little about genetic continuity between Neandertals and modern people. This is in spite of the fact that (at least late) Neandertals buried their dead, cared for the incapacitated members of society, and occasionally engraved or pecked geometric patterns into hard surfaces and used “pencils” of coloring material to decorate something, perhaps their own skins. Some years ago, we found indications that a real gap divided the behavior of Mousterian peoples from that of their Upper Paleolithic successors at sites such as Cueva Morín, despite the fact that some of the tools made by both groups looked superficially quite similar. However, anyone who mistakenly believes that the difference between the two is due to some such single, oversimplistic causal factor as the lack of a gene to permit articulate speech betrays his or her ignorance of the complexities of Neandertal behavior and human evolution in general. Whatever the case, any real proof of the relationship of Neandertals to modern *Homo sapiens sapiens* will have to come from studies of skeletal materials and genetics; it cannot be based on an examination of stone tools. Biological and cultural evolution are not the same, and then as now, there was no necessary correspondence between hominid physical type and culture, no matter what some scholars may erroneously think.

■ VARIABILITY AND INNOVATION

During the 2004 Neandertal Workshop in Santillana del Mar, Francesco d’Errico asked the assembly whether it is possible to equate behavioral variety with innovation. Clearly, the answer is no! Innovation extends the behavioral repertoire by the addition of novelty. Variability is simply the exercise of different parts of a behavioral repertoire that may already preexist the choice. The distinction is important. Within any industrial complex, such as the Solutrean, the degree of variability of stone tools is really not much greater than that marking the difference between the Mousterian “facies.” And the number of tool types represented in a well-excavated assemblage of given size does not really distinguish Mousterian and Upper Paleolithic assemblages at all well. (This fact is not generally appreciated, because the de Sonneville–Bordes/Perrot Upper Paleolithic type list contains so many more types than the Middle Paleolithic type list of Bordes.) However, that is far from the case for rates of innovation. Whether manifest in the addition of several new tool types, including bone and antler artifacts, to the general inventory of Upper Paleolithic tools, or the specialization of sites and occupations, or the comparatively rapid turnover of industrial complexes, or the addition or “popularization” of personal adornment and wall art, innovations took place very much more rapidly during the Upper Paleolithic than they did during the Mousterian.

There are certainly cases where Neandertals have apparently behaved in novel ways: burying the dead and the use of adornment and body embellishment may be two examples of Neandertal behavioral innovation. But one “innovation” that caused considerable excitement when it was announced was the use of flowers as grave offerings at Shanidar cave, found during the 1960s (Solecki 1971, 1975; Leroi-

Gourhan 1975). In this case, the excitement may have been premature. Instead of occurring as usual in isolated grains, the pollen from eight of the represented species apparently associated with Burial IV at Shanidar was in many cases found in clusters of “from 2 to more than 100 pollen grains. Certain of these clusters have retained the form of the anther of the plant” (Leroi-Gourhan 1968, 1975: 562). This suggests that the pollen was not windborne, but came from whole flower heads deposited in the sediments. The flowers identified were from the genera *Achillea* (a daisy), *Centaurea* (the corn flower group), *Senecio* (ragwort), *Muscari* (grape hyacinth), *Ephedra* (horsetail), and *Althaea* (hollyhock). The pollen analyst believes that the flowers were probably collected from May through July, which is in approximate agreement with the date of discovery of the Shanidar IV burial during excavation in early August 1960. All the represented genera are known to have medicinal properties, a point which was stressed at the time (Solecki 1975), but that is nothing extraordinary. Virtually all plants grown as ornamentals have been claimed to have medicinal properties at one time or another in the past. The genera identified all have members that the local Kurds grow in their gardens, and Solecki tells us that his workmen were in the habit of bringing whole flowers into the site tucked into their cummerbunds or the handles of their wheelbarrows (1971: 93–94, 176). I have always suspected that the flower pollen from Shanidar IV could be the result of accidental modern contamination. After the samples were subjected to acidic treatment to remove pollen from the surrounding sediment, it would have been difficult, to say the least, to tell ancient from modern pollen. The discovery of the delicate scale from a butterfly’s wing in one of the pollen samples does not help the case for contemporaneity of the pollen and the burials. Last, pollen samples from Shanidar also contained vestiges of tobacco pollen and that from cultivated date palm, which were certainly not plants known to the Neandertals. Were innovative Neandertals at Shanidar the “First Flower People,” as Solecki would have it? Perhaps, but the case is still not proven.

INFERENCE AND SPECULATION

Variability and imprecise standardization (looseness of patterning) are hallmarks of Mousterian tools. The nature of this variability is such that the processes of teaching each succeeding generation of Neandertals how to make tools must have been very different from the socialization processes of modern *Homo sapiens sapiens*. In general, the fact that given artifact types from a single site are internally quite variable, while the ranges of variation within a given type from different sites in a wide region overlap to a large degree, shows that there is little or no deliberate stylistic information encoded in the types as Bordes defined them, nor as the measurements show is there the sort of unconscious stylistic load that is often incorporated in the products of different modern identity-conscious socio-cultural groups. In part that may be an accidental consequence of the fact that it is hard to alter lithic artifacts without affecting their function, but it may also reflect an important adaptive reality. It may well be that intergroup boundaries were not as purposefully maintained, signaled, and defended as they are among most modern societies. The distinction of “we”

from “they” may have been adaptively dysfunctional as long as human groups were small and resources abundant. Appropriate mates would have been hard for an adult to find in a co-resident group whose maximum numbers were only as large as a few score people of all ages. Permeable group boundaries would have eased intergroup movements of personnel, and could have been advantageous to survival.

As a student, I was taught that the processes of natural selection would in the long run result in enhancing the formal differentiation of tool types, as a result of their increasing functional specificity, or suitability for different tasks. But this process may take either (or both) of two directions. Tools may either be improved by making them better suited to the performance of different elementary tasks, such as hacking, slicing, or perforating (regardless of the substrate on which they are used), or they may be designed to improve their efficiency in performing a limited set of tasks on a selected substrate (the apple corer and peeler is one example, the nut-cracker another, and the whaler’s harpoon head a third), or on a material that only exists in a particular world region such as the tropics (manioc shredders or the ice knives used by the Inuit, are examples).

Through the Early Würm/Weichsel, the evolution of artifact morphology generally reflects continued design improvements that made working edges and whole tools increasingly efficient for use in a small number of primary operations. I have called this kind of adaptation of the tool inventory “technique-oriented.” Tools for chopping become more effective choppers (and less efficient hammers or slicers), slicing implements become more efficient as slicers, and in general each implement type becomes more differentiated and better adapted to a specific kind of manipulation. But those primary operations may be performed on a variety of materials: skins, vegetal materials, and meat can all be sliced with the same cutting edge. This, it seems to me, is the most appropriate interpretation of the saying that some Mousterian tools are “general purpose tools”: they can perform the same limited set of operations on a variety of resources in vastly divergent environments. There is little evidence that any tool type was specifically tailored to work on one material alone.

There are a few stone tool types, such as the Szeletian knives of Central/Eastern Europe and the cleaver flakes of North Africa and parts of Spain, that apparently signal some degree of regional diversity, seeming already to reflect the beginnings of regionally distinctive adaptations, although the artifact classifications currently in use ignore the most obvious “stylistic” attributes of tools. While there do seem to be some interregional boundaries across which notable differences in artifact inventory can be discerned, the size of the areas in which artifact series are homologous at any time seems surprisingly large compared to the Upper Paleolithic condition. When idiosyncratic attributes of artifact assemblages confined to more restricted geographic regions do at last begin to appear during the Middle Paleolithic, they do seem to be the result of conscious, deliberate stylistic differentiation.

This is in marked contrast to the development of some of the tool types in the Upper Paleolithic artifact inventory which are regionally restricted or must have been used on a specific small number of locally available resources. I have called

this “regional-and-resource-oriented” adaptation. In addition to the specialization of tools for use on specific resources in particular regions, enigmatic decorative devices appear, such as geometric patterns incised in bone artifacts, or painted symbols such as those accompanying some of the large painted animal figures at Lascaux. These may have signaled both individual and group identity, as it became increasingly necessary to demarcate regions and to stress one’s claim to rights and privileges within them. Another, non-artifactual, kind of evidence for the appearance of this new adaptive orientation is the discovery in increased numbers of bones of small, nocturnal, burrow-dwelling fur-bearers in Upper Paleolithic occupations. Such creatures, ordinarily invisible to hominids living in their vicinity, can only be taken in numbers with traps that have been specially designed to take advantage of the animals’ size and behavior. Such traps have not yet been identified in most Upper Paleolithic sites, but the bones of their prey attest to their former existence.

At one time animal bones from Mousterian levels as a rule suggested to me an opportunistic exploitation of all mammals available in the environment that were “easy enough to see and easy enough to hunt.” I contrasted this with the intentional selective harvesting of wild resources attested for some Upper Paleolithic sites, where those few resources that were most productive were deliberately chosen. Since that time, some Mousterian levels have been excavated that indicate that hunters occasionally focused on the procurement of one or a small number of species, as they did more commonly during the Upper Paleolithic. However, it is my impression that when resources are present in a Mousterian site, they are usually (though not always) those that were readily available in the near vicinity of the site itself, and that they were not transported in any quantity for any great distance: large numbers of shellfish do not travel far from the coast, and alpine animals are only abundant in alpine sites, etc. Now, as any human group becomes more familiar with its natural surroundings, in a process the late Robert Braidwood called “settling-in,” it is almost inevitable that it will eventually learn to exploit those resources that are locally most available and easiest to take, either seasonally or throughout the year. (That, it seems to me, is the explanation for the accumulation of shellfish remains in the Gibraltar caves.) Mousterian groups could very well have learned to move periodically from site to site to take advantage of resource availability, but there seems to me to be little evidence for the movement of materials between regions.

During the Upper Paleolithic, humans settled areas much further into the inhospitable northerly latitudes than they ever had done earlier. Apparently this movement was facilitated by the development and extensive use of storage devices such as pits, some apparently used for cooking. Storage facilities would of course help tide one over a lean season when resources were scarce, and would facilitate the protection of resources from the actions of competitors, and add to the available battery of food-preservation techniques. It is striking that while storage facilities are not entirely absent, they seem to be very rare in Mousterian sites: this certainly speaks to the tenuous nature of Mousterian life in seasonally inhospitable regions. It is evidence for the more limited nature of food preparation and preservation techniques (open-fire roasting, stone boiling in skins [?], food drying and chilling?) available to

Mousterian peoples, in comparison to the wider range (including baking or boiling in pits, the storage of roots and tubers in dark, humid conditions, reheating, and probably controlled fermentation or pickling, as well as better defense against carnivores or destructive small organisms) that were available during the Upper Paleolithic.

In sum, I do not believe that selective exploitation by Mousterian peoples ever reached what may be called the final, perfected stage of “primary hunting/gathering efficiency.” That would have involved designing specialized tools for use in a specific region of operation or on a specific set of resources. How can one differentiate Mousterian behavior from that characteristic of the “wild-harvesting” adaptations of the Upper Paleolithic? The latter must leave distinctive traces in the archeological record: artifact inventories that are well differentiated on a microregional scale, including differentiation of attributes of what de Sonneville–Bordes considered single tool types (an example is the presence of different “styles” of Solutrean points, seen in the comparison of Smith’s 1966 study of French Solutrean pieces, the concave-based points common in Cantabria, or the stemmed points from Parpalló and Ambrosio in the Valencian region); accumulations of selected raw materials in sites; the nature of the selected prey; evidence for the harvesting of healthy individuals of all ages rather than just the young, the old, and the feeble; and evidence for the relatively long-distance transport of resources. One might also expect to find that different parts of the habitat were specifically chosen for the extraction of the different resources available in each: the mountain slopes for alpine animals, the coast for fish and shellfish, and specific quarry/workshop areas used for the acquisition of stone raw materials, and that this pattern of exploitation was combined with evidence for the long-distance transport, by a single human group, of quantities of materials from their sources to their areas of utilization, where they were not readily available (or with evidence of interregional exchange). This would seem to coincide with the pattern of movement of resources like marine mollusks or alpine animals documented for the Cantabrian Upper Paleolithic or at least its later manifestations. (It may be that Upper Paleolithic stone tools had a more limited, regionally restricted trajectory than did raw materials for tool manufacture, items for adornment, or foodstuffs.) While no single kind of evidence may prove conclusive of such an adaptation alone, in combination their presence is most suggestive. On the contrary, I believe that Mousterian sites provide much less evidence for the movement of goods between regions.

■ A POSSIBLE DIRECTION FOR FUTURE RESEARCH

In the past (1977, 2005) I have suggested some changes in our procedures that I believe might prove fruitful in future studies of the Mousterian complex. They involve the combination of a viewpoint that tends to be more anthropological than geological with new techniques for artifact classification (taking into account core reduction sequences, the staging of implement manufacture and resharpening, and the effects of continued use on working edges), and using proven quantitative procedures for the definition of stone artifact types from individual sites, the classification of toolkits,

excavated assemblages, and the areas where different activities were performed. My perspective borrows from B. Malinowski's (1960) concept of institutions, and adds a modification of F. Gearing's (1962) concept of the "structural pose."

We know that every living society apportions some special part of its complex set of behavioral inventories to individuals defined as especially suited to the requisite behavior patterns (and there is no reason to think that Neandertals were very different from modern humans in this respect). It is possible to study this apportionment in two ways. When one is interested in the learned behavioral patterns assigned to the several positions in a society that an individual may occupy, one should study the behavior patterns as the "rôles" of individuals. But when one focuses on the purposes of behavior, the individual performers and their positions are less pertinent than the patterns themselves, and the behavioral categories of greatest relevance are sets of responses that are culturally defined as appropriate to identifiable and recurrent situations. I have called these sets of responses the "functional modes" of a social group: modes such as dancing, curing, hunting, mourning, toolmaking, clothing manufacture, and fighting are examples. They are loosely defined and may even intergrade, for no human behavior (present or past) is ever packaged in minimal, contrastive, non-overlapping sets. Some of these functional modes, several of which may be operative in the behavior of individuals at the same time, are manifest in the behavior of single individuals, while others may require the cooperation of several individuals, organized into loosely constituted temporary groups or rigidly structured, long-enduring "corporate" bodies.

Each functional mode employs a cultural apparatus, consisting in the range of permissible behavioral alternatives open to the performers, a set of attributes and values that serve to guide performance, and sometimes, a set of physical equipment used by the performers, which may be called the *matériel*. Even in cases where the functional mode of behavior requires no *matériel*, its activities often alter the natural surroundings in recognizable ways.

Excavating relatively unmixed and largely "undisturbed" archeological occupations, the archeologist (or, if you will, the prehistorian or paleoanthropologist) can recover durable artifacts in association with patterned contextual materials such as fungal spores, chemical traces, remains of plants and animals, and sediments, as well as information about the location and the relative position and abundance of each of these categories of evidence. A quantitative search for significant patterned relationships between artifactual and contextual data can optimally evaluate the contribution of random effects to these relationships, and define related constellations of *matériel* which vary together, and independent of other sets. These, if correctly isolated, can represent the *matériel* and by-products of activities associated with distinct functional modes of behavior: some are the toolkits and by-products of activities associated with distinct functional modes of behavior: some are the toolkits and by-products of economic/technological activities; others may reflect organizational or ideological elements. Each different individual's behavior is to some extent idiosyncratic, and so the *matériel* and by-products of a set of activities undertaken by one group of performers can be expected to vary "stylistically" from those of

another set of performers doing the same or similar things. This is a potential route to the recognition of “team membership,” and the eventual definition of “regional style zones,” the delineation of different socio-cultural systems, and an understanding of their location and duration and of the stylistic changes they have undergone over time.

The program I have outlined is at present admittedly a visionary ideal, but it is not at all unrealistic: it can be realized as evidence from future well-controlled excavations becomes increasingly available. I believe that we are now poised at the brink of great developments in Mousterian research, and that investigations in Cantabria will soon add substantially to our understanding of this (in many respects) enigmatic industrial complex.

But the complex is likely to remain an enigma for some time, if we cannot overcome the traditional reluctance of some of our colleagues to criticize the conclusions of their teachers, even when those conclusions have been shown to be in error. That attitude is entirely mistaken, as I am sure those same teachers would agree. All honor and respect are due those pioneers in our field whose brilliant contributions brought us to our present state of understanding of the Mousterian industrial complex. But that does not mean that we can never be more than their carbon copies. It is of course much easier for today’s investigators to follow the path of least resistance, slavishly repeating the ideas of their teachers. But that would be a crippling error. As they become outmoded, older approaches must be supplanted by the newer, more productive perspectives that we have gained as a result of seeing where the perspectives of the past fail to fit the data now in hand. That is the essential prerequisite of future progress.

The readiness of Cantabrian scholars to challenge theories that have outlived their usefulness is one of their strengths, “uncivilized” though that attitude may seem to some. I do not mean to imply that our field is advanced in the least by the snide critical attitude of a very few of our colleagues from other countries, who have gained their reputations by criticizing the work of others with “data” or “reinterpretations” that they have fabricated just for that purpose, or by mastering the art of damning with faint praise. But a positive and well-intentioned critical approach to investigation is a hallmark of good science, and that is what the best younger scholars in Spain and the Cantabrian region do so well.

Spanish investigators, among whom I think particularly of those working with renewed intensity in Mousterian studies, are already taking their place as recognized leaders in prehistoric research. I hope that work at the cave of Sidrón in Asturias may add immeasurably to our knowledge of regional Neandertals. I am confident that ongoing excavations at the cave of Castillo, following in the tradition established by the late Victoria Cabrera and her husband, Federico Bernaldo de Quirós, will continue to provide invaluable information about the development of the Mousterian in Cantabrian Spain, as well as about the nature of the transition to the Upper Paleolithic, not just in the region, but in Western Europe as a whole. We are at the threshold of a new era in Paleolithic studies. Let us all hope that its results will be as bright as its promise.

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Research on the Middle Paleolithic in the Cantabrian Region

WHERE HAVE WE COME FROM? WHERE ARE WE NOW?*

INTRODUCTION

The story of investigations of Mousterian sites in Cantabria has respectable antiquity, and Cantabrian research since its commencement has made contributions of great consequence to our understanding of the Mousterian complex of industries. Eduardo de la Pedraja first excavated Mousterian levels at the site of Covalejos in 1872 and Fuente del Francés in 1880; between 1878 and 1880 Sanz de Sautuola himself discovered the archeological deposits in the Cueva del Pendo or San Pantaleón (though without excavating its Mousterian materials). The recovery of Mousterian materials continued sporadically throughout the earlier part of the twentieth century. During the 1950s, the leading authority on the Spanish Mousterian was Prof. Francisco Jordá Cerdá, director of the Museo Arqueológico Provincial in Oviedo, who himself had developed an overview of the peninsular Mousterian, including the Cantabrian collections. Under the direction of J. M. de Barandiarán, J. Altuna, and others, some research had been done at the Basque sites of Lezetxiki, Axlor, and

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Amalda. Other specialists had shown an interest in the Mousterian assemblages from some Cantabrian sites, particularly the well-known caves at Castillo, el Pendo, Cueva Morín, and Hornos de la Peña, and the lesser-known sites of la Mora, the Fuente del Francés, the Abrigo de San Vitores, and the railroad cut at Unquera. Most of the investigators of Cantabrian sites were local, but others, including Breuil, Obermaier, Shallcross, and Wernert, were foreign: Cantabria has always received serious international collaborators cordially, placing very few restrictions on the activities of those willing to share their research responsibilities with their Spanish colleagues. The result has been a fruitful cross-fertilization of theoretical approaches, which, when coupled with the richness of the region in Paleolithic remains, has placed Cantabrian research at the forefront of Mousterian studies.

Needless to say, some important Mousterian sites discovered in Cantabria were first investigated many years before my own studies began (some during the nineteenth century), often with techniques that today are considered unacceptably primitive, and in a few cases, they have since been destroyed and their collections lost. Other excellent investigations remained poorly published. Investigations of the Cantabrian Mousterian were scarcely the central focus of attention for the majority of prehistorians. In fact, as a student, I was told that “most prehistorians seem to be afraid of the Mousterian. Whenever a set of tools that are large, or even smaller but crudely made, but are not obviously Acheulean handaxes are found, instead of trying to understand it they call the collection ‘Mousterian’: that effectively sweeps it under the rug and out of sight.”

Excavations at the Cueva del Castillo in the early part of the last century were very respectable for their time, and yielded spectacular and rich Mousterian collections. A timely and full publication of those excavations would almost certainly have stimulated Middle Paleolithic investigations in Cantabria. But monographic publication of research results at Castillo had to await the painstaking synthesis of the late Dr. Victoria Cabrera in 1984 (Cabrera Valdés 1984). Mousterian studies would surely have received renewed impetus had the truly superb investigations undertaken during 1953–1957 by an international team under the direction of Dr. Julio Martínez Santa-Olalla been published expeditiously, but that was not to be, and no monographic description of that work appeared until 1980 (González Echegaray et al. 1980). It was not, in fact, until the last quarter of the twentieth century that new generations of scholars began to focus attention once more on this fascinating period of prehistory. Then, under the direction of the author and Dr. Joaquín González Echegaray (at Cueva Morín and elsewhere), Drs. Victoria Cabrera and Federico Bernaldo de Quirós (esp. Castillo and el Pendo [Montes Barquín, J. Sanguino et al. 2001]), and Ramón Montes (esp. at el Pendo), new teams of investigators once more began excavating Mousterian levels and analyzing the assemblages they yielded. We have every reason to expect that the new excavations at Castillo will add appreciably to our knowledge of Cantabrian Mousterian adaptations when they are fully published.

The situation outside Cantabria at the time I began my own studies of the Mousterian was summarized by the Neanderthal Centenary Conference in Düs-

seldorf in 1956, published in 1958. Contributions to that conference volume (Von Koenigswald 1958) raised questions of two sorts. First of all, what is the relationship of the Neandertals to modern humans in Europe and Asia? Second, what is the nature, and what are the relationships, of the Mousterian complex of industries? And what, if anything, can answers to the second question tell us about the first?

■ WHAT HAVE WE LEARNED?

Studies of Neandertal skeletons elsewhere in Europe reveal a great many differences between the physical characteristics of those skeletons and our own. Apparently, Neandertals used their front teeth to help hold and pull things in ways that would be quite unusual for modern humans. Biomechanical studies suggest that Neandertals were capable of throwing things very forcefully, and both of hugging items close to their bodies more powerfully, and with a stronger, more vice-like grasp between thumb and forefinger than is the case for the average modern. But, with the exception of the promising remains from el Sidrón in Asturias, there are no Neandertal skeletal remains from Cantabria.¹ Certainly, material that most agree is Neandertal has been found in some abundance elsewhere in Spain, but the question of the relationship between Neandertals and modern humans can still not be answered using bones from the Cantabrian sites. Local evidence all bears on the nature and relationships of the Mousterian complex of industries.

Despite all the contrary claimants, the analysis of this evidence must build on the pioneering work of François Bordes and a few of his dedicated French colleagues, such as his wife Mme. Denise de Sonneville-Bordes, and his teachers and collaborators Denis and Elie Peyrony, Maurice Bourgon, and Paul Fitte. Instead of studying single guide-artifacts, Bordes had shown that one could greatly improve our understanding of Mousterian collections by introducing systematic principles of artifact classification to replace the chaotic, unsystematic, and overlapping type definitions in previous use and by studying artifacts as parts of whole assemblages. Bordes had examined thousands of Mousterian artifacts from scores of archeological sites, many of them well excavated. He had discovered that the majority of these French assemblages fell into a few modal types he called the facies. The facies, he thought, were the stylistically distinctive products of different identity-conscious socio-cultural groups, rather than groups of tools with differing technological functions. He had started to extend his classification to other regions, including Italy, Central and Eastern Europe, and China. His work on climatic succession documented by loess deposits in the Seine basin showed that some of the facies were not chronological stages of assemblage evolution but rather long endured side-by-side, and he was able to dismiss some supposedly independent “cultures” such as the Levalloisian as really only technical variants of ordinary Acheulean or Mousterian industries, showing that others such as the Tayacian were mostly cryoturbated or geologically crushed pieces. His study of Paleolithic industries led him to question how the earliest Western European Upper Paleolithic artifact industries, then all assumed to have been produced exclusively by modern men, might relate to earlier Mousterian. Others, too, were fascinated by this

question, and were divided into several camps, some of which spoke of transitional Mousterian–Upper Paleolithic industries. To Bordes, the only obvious transitional industries were the Chatelperronian and the Aurignacian “0.”

In France, by the 1950s most prehistorians were satisfied that a broad picture of the evolution of Paleolithic artifact industries had been formulated, as had a framework of temporal periods and regional paleoenvironmental successions against which industrial development could be viewed. Crude though these schemes were known to be, they were judged reliable enough and adequate in general outlines. What remained to be done was to fill in the gaps in this picture: most Paleolithic prehistorians regarded the principal task facing them to be an increasing refinement of chronology and the ever more refined study of the microevolution of regional climates and artifact industries. No one then seemed to realize the extent to which Bordes’s concept of the facies, his recognition that they could not be explained as chronologically successive stages of industrial development, and his alternative proposal that they were instead the stylistically distinctive products of different social groups that developed independently of each other had begun to undermine this plan. In my own research, I was at first no exception.

In Cantabria, no one trained in the use of his methods but Bordes himself and his colleague Jacques Tixier (they had together studied the partial Castillo collection in Paris) had attempted to classify the Mousterian collections, or to examine the supposedly transitional industries. (On account of the striking abundance of cleaver flakes in the Castillo Mousterian Level Alpha, Bordes had assigned that collection to a postulated new facies, which he misleadingly called the “Vasconian.”) Consequently, when I began my doctoral research on the Cantabrian Mousterian in 1962, my first task seemed logically to be the classification of all the tools in museum collections from the local Mousterian levels according to the Bordes typology, and the assignment of the collections to the facies that Bordes had defined, wherever possible. Only after having assigned the Cantabrian assemblages to Mousterian facies, establishing “*Quantas maneras son dellas,*” as Alfonso “El Sabio” would have said, could one proceed to determine why they differed and how they were related. Secondly, the evidence for changing Middle Paleolithic paleoenvironmental conditions would have to be reevaluated, so that the relationship, if any, between them and the contemporary Mousterian industries could be determined. In the process, it would also be necessary to examine all the so-called transitional Mousterian–Upper Paleolithic industries, to determine their makeup and relationships.

In 1962/3 I classified all the museum collections of Mousterian artifacts from Cantabria, and beginning with our excavations at Cueva Morín in 1969, González Echegaray and I eventually managed to study two dozen trustworthy Mousterian artifact collections, including nineteen from sites we had excavated or tested ourselves. We found that they included representative assemblages of Typical Mousterian, Quina Charentian, and Denticulate Mousterian, including a super-denticulate type with a much higher proportion of denticulate tools than was usual in French collections. Cleaver flake-bearing collections or assemblages from Castillo, el Pendo, and Morín proved to be otherwise heterogeneous, and so assignable to different fa-

cies, which effectively eliminated the so-called Vasconian from Bordes's facies list. Despite the contrary opinion of some early "authorities," the cleaver flake-bearing Mousterian with tools so reminiscent of Acheulean bifaces was not our oldest Mousterian, but as earlier excavators had correctly noted, was stratified atop levels of such "Mousterian of small types" as the Quina Charentian at Castillo and the Denticulate at Morín. True Chatelperronian was present in one level at Cueva Morín, while none of the supposedly transitional Aurignaco-Mousterian cases was trustworthy. Most such "transitional" levels were poorly excavated, mixing Mousterian with Upper Paleolithic materials, and others, such as the old collections made by the Conde de la Vega del Sella at the Cueva del Conde in Asturias, included purely Mousterian or Upper Paleolithic levels containing tools that had simply been misunderstood. What is more, the Chatelperronian from Cueva Morín, and that later identified in the collection of tools from Level VIII, overlying the earlier Aurignacian Level VIIIa at el Pendo, did not look like it sprang from the local Mousterian.

It has been both a strength and a weakness of my own research on the Cantabrian Mousterian that it has consisted entirely of the reinvestigation of sites that were previously known and excavated. Where I was restricted to information from older Museum collections, the available paleoenvironmental data were most often deficient. Such information, where present at all, was fragmentarily reported and only selectively saved, and so, working with materials that González Echegaray and I had not gathered ourselves, the collections of stone tools necessarily became our major focus.

In our investigations, we introduced statistical procedures such as the Kolmogorov-Smirnov test, rank-order correlation, and principal components analysis for the evaluation of the contribution of random sampling error to difference between assemblages. We were first able to confirm the lack of evidence for chronological development in the collections studied: no tool type ever replaced any other through time. That of course was just what we had expected. What we learned next was a surprise. We had worked to assign the collections we studied to Bordes's facies. But the statistical tests that showed that some types of tools were related to each other, and not related to other types, soon also indicated that the Bordes facies intergraded along axes of scraper richness as opposed to denticulate richness. The facies, we showed, were completely arbitrary constructs of the classifier. That becomes obvious to the eye when the Bordes diagrams of cumulative percentages of tool types in each are compared (Freeman 1994: 37–54, esp. fig. 4.2). What is more, the differences between these differently covarying tool categories seemed to be functional rather than stylistic. To clinch this interpretation, we found tools of two facies—Typical and Denticulate—in substantial numbers in separate restricted areas within a single archeological level (Freeman 1992b), where the areal distributions of tools were so small that no one could rationally suggest that two tribes had occupied that one level at the same time.

Despite my continuing insistence, which I shall return to below, that there is no necessary parallelism between the body forms of the makers of prehistoric industries and the kinds of tools they fabricated, there are nonetheless some very intriguing

characteristics of Mousterian assemblages that seem to indicate that their makers' mental capacity, degree of cultural complexity, and socialization procedures, and the rapidity of cultural process, must have been quite different from those of fully modern peoples. Several Mousterian stone tool types such as cleaver flakes seem to endure immensely longer in the archeological record than do comparable Upper Paleolithic types, and the measurements of such tools from different levels or sites—even sites separated by great distances—do not seem to differentiate them well; comparable Upper Paleolithic types from neighboring sites or adjacent levels are more often quite distinctive in their metric characteristics. In my opinion, some Mousterian levels (e.g., Morín, el Pendo) contain tools such as sidescrapers made on irregular pieces of longbone. The working edges of such pieces replicate in shape and in representation the several types of stone tools in the assemblage, but no attempt has been made to give a predetermined overall shape to the bone support. Since this technique is so different from that used to make Upper Paleolithic bone tools, where the overall shape of the piece is more often conformable to a regular pattern, some prehistorians have doubted that the Mousterian bone pieces are tools at all. Nevertheless, that is what they seem to be. Manifestly, the degree of regularity of Mousterian “models” and the application of Mousterian “standards” of form and measurement must have been much less rigid, and tolerated much more variability, than was the case during the Upper Paleolithic. That suggests that the “socialization process”—Neandertal modes of teaching and learning—must have been markedly different from our own.

There are comparable differences between Mousterian and Upper Paleolithic cave occupants in the treatment of the space they lived in and the rigidity of patterning of scatters of tools they left over the living floors (Freeman 1992a). Upper Paleolithic peoples subjected their living space to violent alterations, periodically performing a drastic “housecleaning,” artificially lowering and leveling the cave floors, and shoveling previous accumulations of debris out of the living area. They dug house foundations, graves, postholes, and storage pits into earlier levels, sometimes moving tons of earth and stone. While in a few sites Mousterian peoples did some of those same things, by and large they took the cave surface as a given, building atop it despite its irregularities, rather than digging into it to rearrange it and discard whatever got in their way. And last, while all Neandertals seem to have at least occasionally made some use of coloring materials, and some scratched regular marks on nummulites or bones (Freeman and González Echegaray 1983; Henshilwood et al. 2004), while a few Middle Paleolithic people may have perforated shells for suspension, or buried their dead, such behavior is remarkably rare in comparison with the abundance of remains of Paleolithic art, adornment, and even human burials that are regularly found once fully modern *Homo sapiens sapiens* appears on the scene. These bits of evidence all suggest that Neandertals were behaviorally quite different from their successors. But though it is suggestive, that, I insist, is only the very most tenuous and indirect evidence against their relationship to moderns.

WHERE DID WE GO WRONG?

Unfortunately, and this is particularly the case now that so much information is gleaned from the Internet, the great amount of true wisdom that has been accumulated is passed on with a lot that is not truly wisdom, but misinformation masquerading as such. In the last years there has been an explosion of both kinds of “data,” misinformation as well as truth. It has always been the case that we should reexamine or challenge past ideas, since much of the accepted wisdom from the past is mistaken, though from the stature of its authors it has often been accepted as unimpeachable law. It is the duty of every generation to test the “truths” passed on by our predecessors by comparing them to what our own experience has taught us. This is not just the task of the archeologist or the anthropologist, by the way; it is the task of everyone who pretends to make a real contribution to knowledge, whether that be in the arts or the sciences. We should learn from the past what it offers that is good, but never fear to test its conclusions, and we must reject those that in our experience do not ring true. In the course of development of Mousterian research, all of us have unfortunately been led astray by a number of errors that we should now do our best to eliminate.

Let me begin by enumerating those errors that are easiest to understand. Any decent archeologist probably knows at least one case of modern work that suffers from these mistakes. I must note that these potential pitfalls are not all unrelated. In fact, errors of one kind are often partly due to misconceptions of another kind; as in all aspects of human endeavor, the potential errors are complex and intimately inter-related. With a few exceptions, I do not intend to mention names of living archeologists who have fallen into blatant error, or to embarrass any of our colleagues, none of whom, I am sure, would ever be guilty of such gross blunders.

(1) First of all, it is deplorably still true that some excavations are not conducted with adequate care. Poor excavation technique and inadequate care for stratigraphic distinctions still plague the study of Middle Paleolithic industries. I do not mean to imply that there is only *one* way to dig a site, for sometimes it may be justifiable to ignore a good deal of minor detail in the interest of illuminating the grander aspects of prehistoric life.² It would make little sense to search for the outlines of an open-air settlement and the walls of the huts within it with the same painstaking dental-pick and paintbrush techniques and concern for microstratigraphy used in the excavation of a small-scale Paleolithic occupation in a cave or shelter. Nor would the agencies that fund our research be likely to support such slow and painstaking work at such an obviously complex site. But until more intact open-air Mousterian sites are found, the observations below will apply.

Unfortunately, even otherwise well-trained excavators may continue to follow the mediocre methods of traditional practice even while acknowledging that they are deficient. It is still common practice to dig a small window deep into the site sediments in order to “trace the evolution of climate and industries,” or to find the earliest occupation of a site, despite the fact that it is impossible to detect stratigraphic

inversions, or delineate the distribution patterns that might indicate discrete activity areas or the presence of structural features, from such areally limited exposures. It would be much better to forget all about finding “the oldest level in the site,” and to concentrate instead on exposing as extensive a surface as possible within each natural level excavated. In fact, my experience suggests that an exposure less than 20 meters square in a single level is too small to let us appreciate any single aspect of how people actually used a site, and even that area, larger than many current excavations, is too small to show us all the activities that they may have performed there.

To keep track of the positions of small recovered finds, it is essential not only to excavate and process the earth from each natural level separately, but also to subdivide a level’s contents into sublevels of about 5 centimeters’ thickness, and to subdivide the surface of each meter square excavated into 9 sectors (sizes that have been shown to be productive in practice). Earth recovered from each such unit should be processed separately for best results.

Too many excavators still seem to believe that in archeology the word “level” means “horizontal.” It does not! It simply means “layer,” and the natural layers of deposition may be highly irregular in thickness and orientation: they lie conformably at base with the surfaces on which they are deposited, and usually have upper surfaces that are also (though differently) irregular in their contours, as the thickness of the layer varies. There is no possible excuse for continuing to excavate horizontal “spits” in a site where such spits crosscut the orientation of the sediments.

In most sites, I have found that stratigraphic changes are more often identifiable from differences in the texture of the sediments in the levels than they are from differences in sediment color. Furthermore, the edges of dugouts or pits and the lots of debris in a shell-midden may be difficult to detect unless the excavator pays careful attention to the orientation of the bones and stone artifacts in the levels. They often follow slopes or lie oriented along ancient cuts into the sediments. Testing sediments regularly for differences in pH value (acidity) and other chemical characteristics can also help delineate such features.

Sad to say, although we know that very small finds may be crucial to the understanding of activities undertaken at a site (Freeman et al. 1998), most archeologists still make little or no effort to recover them, even where they are present, where water is readily available, and where it would not be difficult to wash sediments through ultrafine mesh screens. We know almost nothing about the plants used by Mousterian communities despite the fact that seeds are well preserved in some sites: they have been recovered sporadically from some Mousterian occupations, such as the horizons at Abric Agut in Cataluña, by simple water flotation.³ We now know that our approach to the recovery of small finds from Mousterian occupations at Agut and Morín was flawed: we should have processed *all* the sediment excavated from every level, rather than just that small sample whose characteristics suggested that it would be the most productive. In fact, the extra effort and cost of processing all the sediments would almost certainly have been amply compensated by the important new data recovered, as we later learned at el Juyo.

Last, the utmost care in applying the best modern principles of excavation is useless without extremely careful and detailed recording, using field notebooks, plans, sections, and good photographic documentation as well. Too often, this is where our practice fails.

(2) A related problem is often manifest in the improper “dating” of assemblages or of unusual specimens. An especially insidious example is the dating of surface finds by the assumed ages of the surfaces on which they are found. Many archeologists do not understand the complications involved in the formation of river terraces or raised beaches, and interpret them oversimply.⁴ Materials found in the various levels of a river terrace or beach do not all reflect uniform environmental conditions, and the sediments found in upstream deposits may have formed under climatic conditions that were the reverse of those responsible for contemporary deposition downstream. What is more, archeologists often confuse a date that establishes a *terminus ex quo* with a true, precise, and accurate age for finds.⁵ An item actually dropped by a human atop a river terrace is no older than the terrace but may be many thousands of years younger. (But it is also true that materials derived from older deposits may be eroded from them and deposited atop much younger terraces.) In fact, we should all be aware that artifacts of many different ages may be found intermingled in contexts that cannot be differentiated in or on terraces. Attempts to date assemblages by the nature of biological materials found in deposits with them can be problematic for other reasons. Postulated stages of microfaunal evolution or the development of vegetation are often based on unverified (or obviously flawed) assumptions.

One of our major problems is the lack of suitable radiometric dating techniques for deposits of Mousterian age. Decisions about the age of Mousterian assemblages are made instead on “relative,” indirect criteria, such as the supposed geological age of the sediments in or on which they are found, or on a few uncertain direct “absolute dating” techniques such as amino acid racemization, thermoluminescence, or electron spin resonance, which require us to make prior assumptions that may be totally invalid. Partly because of these uncertainties, we are inclined to discard what may prove to be accurate dates that do not correspond to our present preconceptions, either adjusting the parameters of the tests until the results “seem reasonable” or accepting only those ages that can at best be considered “consensus dates,” in better agreement with what we think we know. I remember that, when potassium-argon dating was first applied to them, there was a great debate about the age of Villafranchian deposits and the Plio-Pleistocene boundary in which many insisted that both had to be much younger than the new (and approximately accurate) dates showed. Then, too, we are still hampered by the idea that one can date specimens by the apparent crudeness or primitive characteristics of their form, which leads us to postulate a great age for hastily made expedient artifacts or pieces found as rough-outs that were discarded at quarry sites because of their imperfections.

Inaccurate dating is only one of the problems we must avoid. It appears so large only because we have been taught that to study an assemblage, one must know its age.⁶ That is only true for some purposes. Many important questions about the past

can be answered by studying well-excavated assemblages whose absolute or relative ages are unknown.

(3) There are several other potential traps for the unwary. One of the most insidious of these is our human desire for the simplest possible explanation of complex phenomena. We behave as though we believe that as “scientists” we should emulate physicists or astronomers, for whom, in our distorted caricature of them, simple explanations have historically proven more powerful than more complex ones.⁷ As archeologists, we try to explain such occurrences as the radiation of hominids out of Africa by the invention of fire, the introduction of a single miraculously powerful tool type, or some convulsive climatic change. It is currently fashionable to ascribe the origin of spoken language to the shape of a single bone (Clegg 2004), or to a single gene. While a defect in a single gene can undoubtedly prevent one from being able to speak, having that gene in normal form will not automatically produce articulate language. The ability of a Cervantes was certainly not the result of a single gene. Humans are popularly supposed to be killer apes whose genetically determined instinctual aggressiveness accounts for wars. In fact, the decision to wage war is not individual but collective; it is a political decision, not an instinctive reaction. Simple-minded environmental determinism is one of the most pervasive of these errors, mostly because so often the results attributed to environmental conditions are themselves complex and hard to analyze. Human behavior always involves too many interacting factors to be explained in simple terms. The explanations for complex cultural behaviors are *never* monofactorial. Sometimes it is easier to understand how silly such monofactorial explanations can be by reducing them to imagined modern examples. When Javier wins a public office, the outcome may seem to be triggered by a sudden event, but it always involves a multitude of contributory factors; it is *not* simply due to climatic change.

Now, let me discuss some problems that are subtler but no less dangerous.

(4) While the “hypothetico-deductive” model of archeological investigations, so important among archeologists trained in American and British institutions, rightly claims that the particular theory (preconceived working hypothesis) that one sets out to test determines the kinds of data one collects, it is especially important to keep in mind that the data one excavates may have nothing at all to do with one’s pet theory, in which case it would be the gravest error to discard or disregard the data as meaningless. They may well prove to be of the utmost relevance to some other, equally important but unanticipated, theory. As a corollary, one must never let one’s “theory” assume such importance that it leads one to deny contrary data where they are found. In the Middle Ages, no one knew how shellfish could become fossilized. But fossil shellfish existed despite that ignorance. I have been told that unless I can explain the formation of the soil pseudomorph recovered in 1969 at Cueva Morín, it simply can’t exist. I am unable to explain completely just how the Morín pseudomorph formed.⁸ Nevertheless, it is undeniably true that it *does* exist.

(5) An inadequate appreciation of the potential and limits of artifact typology brings as a consequence a number of particularly complicated problems.

- Many archeologists still fail to understand that the observations that can be made of artifacts are of only three kinds: (1) one can observe the physico/chemical attributes of artifacts and the traces they bear; (2) one can observe the positions in which they were found; (3) one can observe the relations (both in space and in number) between the artifacts themselves, and between them and all contextual information. The level of detail afforded by the study of their physico/chemical attributes and that of their positions of discovery provide a very limited amount of information in comparison with all that can be learned from relational data. But studies of relational data depend on careful excavation, detailed recording, precise and intelligible labeling of the finds, and their meticulous conservation.
- A major misunderstanding of the potential of artifact classification stems from a belief that it is easy to distinguish “stylistic” attributes—those that differentiate tools of the same sort made at different times, or by different societies, social subgroups, or individuals at the same time—from those “functional” attributes that reflect the artifact’s technological uses, a priori. Ordinarily, however, the distinction of “stylistic” from “functional” attributes is a late stage of interpretation that can usually not be done until after the classification and much of the study of assemblages has been completed. We do know a great deal about the influence of raw material on style. Much stylistic information is superficial decoration that does not affect artifact function, such as weaving patterns, carving in wood or bone, or painting, glazing, and texturing of ceramics. Textiles, wood, bone, and ceramics are media for the manufacture of tools whose functions are relatively limited, but whose stylistic attributes can be much more extensively and freely varied. That situation contrasts with stone tools that lend themselves to a range of stylistic modifications that is extremely limited, in comparison with their extremely varied functions.
- It would be possible to design a classificatory scheme for stone artifacts that minimized the role of stylistic factors, even where those theoretically exist but are hard to identify, and that is precisely what François Bordes did for Mousterian stone artifacts. In fact, he even went further. He insisted that one should always call a knife a knife, regardless of the “accidental attributes” that made a knife from one region such as China look different from a knife made in another such as France. In so limiting his type definitions, Bordes defined his types “functionally,” defining types in terms of their “knifeness” in this case, or their suitability for other tasks in others, and in so doing he virtually eliminated from consideration any attributes of the stone tools that might possibly prove to be stylistic. His classificatory practice, if applied to modern products, would have led him to ignore the very stylistic differences of which he was so proud, that set the folding knives made in Carsac that he used in excavation off from those made in the Charente, or Paris. Having done this, despite the implicit logical contradiction, he defined the Mousterian facies as *stylistically* distinctive: the products of different “tribes” of Mousterians. If type definitions are not “stylistic,” then the tool types they define can’t be, and neither can the artifact assemblages or “facies” types based on them. These implications of his

classificatory practice were obviously unanticipated and unintended, and are still misleading classifiers today.

- Two important alternatives to the Bordes artifact classification, using “traditional” approaches, have been proposed: that of Leroi-Gourhan and that of Laplace (a third approach will be dealt with in a later section). The first of the two is logically flawed, and ridiculously impractical. Leroi-Gourhan proposed listing all the possible variations in form that could possibly be taken by a tool or its edges, and then, after observing what variants actually occurred, to use those as the basis of one’s typology. The number of possible variants being infinite, Leroi-Gourhan will perhaps have enough free time between harp lessons to list them all, now that he is in heaven. The Laplace classification really yields virtually the same types as does the Bordes classification. But it has the disadvantage of disguising types whose names should be comprehensible to any citizen in the street under the cloak of pseudo-mathematical formulae. The meaning of his “equations” cannot always be remembered even by their inventor, who has had occasionally to refer to previous publications of their definitions in order to recall what particular combinations of symbols signified. If our field is to survive, and to find continued support from governmental or private donors, the results of prehistoric investigations must be made intelligible to the broadest audience possible, not just to an elite “priesthood” using an esoteric pseudo-scientific jargon for their description that can only be understood by other initiates.
- But until artifact classification appropriately considers the operational chain of artifact manufacturing—both the nature and staging of materials processing and sequences of core reduction—as well as the progressive changes of artifact edges due to continued use-wear and breakage, further progress in artifact classification will be limited. There have already been attempts, though not always realistic ones, to take these factors into consideration, and there will surely be more and better ones in future.
- Another source of error lies, paradoxically, in our current focus of attention on “whole” artifact assemblages. For a long time that was a strength of the Bordes approach to the classification of Mousterian industries. Artifacts found together in a single discrete archeological level constitute true assemblages. Assemblages, as we now understand them, are composed of the multitude of toolkits used by the occupants of a discrete level. Artifacts found together in museum drawers are always collections, but they can never be considered “toolkits,” and seldom do they constitute assemblages as the excavator found them. Collections are sometimes “split” between two or more museums, often must be “consolidated” due to lack of storage space, and can become detached from relevant information about their provenience, or may become mixed through movement during cleaning, or building reforms (rebuilding, adding a room, etc.), or during careless study. Consequently studies based on museum collections, particularly older ones from less than adequately controlled excavations, but even excavations conducted according to the best modern criteria only to suffer from careless warehousing and slipshod curation, are often overambitious and overoptimistic as to the significance of their results. My own doctoral dissertation was to some extent flawed because of such effects, but fortunately they did not alter its major conclusions.

- Another problem faces the student of assemblages from the best, most careful, and up-to-date excavations. We owe to François Bordes a pioneering insistence that the attributes of all tools in a whole assemblage should be considered by the classifier. While that sounds laudable, it is less so in practice. No assemblage is ever truly “whole,” in the sense of representing all the activities undertaken by a human group, or the tools used in them. In fact, because of the disappearance of perishable items, and the areal limits of excavation, they cannot be more than a sample of the materials made and used in a single archeological level. To regard them as “*complete* assemblages” is simply self-delusion.
- Bordes’s insistence led historically to a great improvement in our understanding of Mousterian assemblages and their differences, including the definition of the supposedly distinctive “*facies*.” But, we have now reached the limits of progress in that direction. To progress further, it seems clear that we must shift the focus of our efforts to attempts to understand the different “*toolkits*” that go to make up an assemblage, rather than on the “*study of assemblages as wholes*.” (Of course, our study must still take into account *all* the artifacts recovered from each level excavated.)

(6) One of the most important potential causes of error and one that is still very much with us is due to an inadequate appreciation of the random factors that always cause difference between assemblages. The use of well-tested statistical procedures is essential for their evaluation but those procedures are not well understood by most prehistorians. It is commonly held by a surprising number of prehistorians that a simple subjective comparison of lists of percentages is “*The Statistical Method*.” That is a naïve misunderstanding. Most such scholars believe themselves incompetent to understand any more complicated statistical tests, and so whenever they read a publication that includes mathematical calculations, they may reject its results as completely unintelligible and thus irrelevant (when in fact it may be quite correct and crucial to their own research). On the other hand, they may accept its results as totally accurate without trying to evaluate them (and in the latter case they seldom dare to contradict the results of such calculations even when they disagree with all the reader’s past experience or with common sense).

But those who publish such calculations often do not understand them any better than their naïve readers. Their work may excusably incorporate honest mistakes in the use or interpretation of tests, or simple errors in calculation. One glaring example from the past was a well-meaning but misguided attempt to alter random sampling procedures, to distribute the samples taken in a program of surface collection more regularly over the ground, so that they could be used both as random samples and as systematic samples for mapping purposes. The proposed modification, called “*Systematic Stratified Non-Aligned Sampling*,” actually violated the principles of the random sampling method to such a degree as to make the samples useless for statistical comparisons.⁹

On the contrary, it is inexcusable when results that make little sense are presented dishonestly, and when, if they contradict previous interpretations, they are falsely claimed to be “*complementary*” rather than contradictory. Unforgivably, a very few of our colleagues have tried to make their reputations on quantitative

obfuscation, assuming that most of the rest of us are too mathematically unsophisticated to catch them.

If you feel mathematically incompetent, but will at least *try* to learn a few standard descriptive and analytical statistical tests, you will soon be surprised at how easy they are. In my experience it is easier for a prehistorian to train him- or herself as a statistician than it is to get a professional statistician to understand the kinds of problems a prehistorian is faced with, and to select appropriate statistical procedures for use in our research.¹⁰ Unfortunately for those who are ill-at-ease with mathematics, the proper use of statistical testing is becoming an essential part of prehistoric research. That is because difference between archeological assemblages may be due to any of several factors, but among them one that is always involved is random sampling error.

Random error is partly responsible for differences *between* assemblages, but it is also one cause of difference *within* an assemblage: differences in content between groups of items found in different areas within a single archeological occupation. Prehistorians seem to assume that artifacts are usually distributed uniformly over an occupied surface, or if not uniformly, “at random,” and that collecting a “large enough” sample of artifacts from each level will somehow “compensate” for these differences in distribution. That is *not* the case. No matter how large they may be, no two samples drawn from a single population of artifacts are ever identical, any more than two handfuls of black or white marbles from a jar whose contents are mixed are ever expected to be identical. Only by the use of standard statistical tests is it possible to estimate the likely amount of difference between them that is due to “random sampling error” alone. That error is meaningless in cultural or depositional terms, and so of no further interest to the prehistorian. Many prehistorians (and other specialists, such as palynologists) attempt to compensate for the effects of random sampling error by restricting comparison to samples larger than some arbitrary size, such as 100 or 150 items. But this practice does not have the desired effect. Even differences between samples of 150 pieces that appear to be meaningful may not be significant, while it may be shown that *some* differences between samples that are much smaller (30–40 pieces, say) are highly unlikely to be due just to chance when they are evaluated by standard statistical tests.

This prompts me to mention yet another approach to artifact classification: what the late Hallam Movius and his students called “attribute cluster analysis.” Its use depends on having enough pieces bearing some particular combination of attributes to demonstrate that the cluster of attributes is not “accidental,” but statistically significant. But most clusters of attributes are not represented with sufficient frequency in archeological assemblages of “average” to large size to permit statistical tests such as chi-square to show that they are significant. Sometimes all that is possible is to prove that retouched pieces are significantly different from unretouched products, by-products of the flaking process, and waste. Though a valid approach, attribute cluster analysis would in the best case imaginable lead to descriptions of assemblages each of which would be quite different and none of which could be codified in the same conventional set of terms, making results hard to communicate

to other professionals. Artifact types that really do exist but are rarely represented in any assemblage would be impossible to define with this procedure. That leaves us on the “middle ground” of compromise classifications that combine attribute clustering analysis with both analyses of the operational chain of artifact production and use and traditional procedures for tool typology.

For the evaluation of the significance of differences between long lists of items arranged in a traditionally accepted order, such as the Bordes list of Mousterian artifact types, I have long favored the use of the Kolmogorov-Smirnov two-sample test, which the statistician Leo Goodman (1954) has shown to give results that are at least as strong and reliable as chi-square. Of course, the Kolmogorov-Smirnov test takes difference of sample size into account, and for long lists of different classes of items, it is very much easier to calculate than is chi-square or any suitable alternative.

Once the contribution of random error has been evaluated, the remaining difference is due to factors that are of interest for interpretation. Here, one *caveat* is called for. The larger the samples compared, the smaller the difference between them need be to reach “significance.” Consequently, statistically significant differences between some very large assemblages may be so tiny that they are essentially useless for interpretation. (It may be fortunate that archeological samples are seldom that large.) Archeologically interpretable “significant” differences can be due to phenomena at the time the artifacts were deposited, or to forces that only acted after the deposits were laid down. Then, one must eliminate from consideration those differences that are likely due to rolling or realignment by flowing water; gravity movement downslope; the action of scavengers, bacteria, and fossorial animals; or redeposition by human activity such as the construction of dugouts or “housecleaning” activities, etc. When that is done, one is left with differences due to phenomena contemporary with deposition, including geological and biotic agency, as well as past human behavior, whose reconstruction is of the greatest possible interest to us. Even in their study, standard statistical tests are essential. Instead of interpreting every item found in a grave as a deliberate offering to the dead, one must first show statistically that the items in question are not accidental inclusions. At Cueva Morín, a Mousterian cleaver flake was recovered from the sediments in a prehistoric grave, and consequently one famous prehistorian insisted that it was a grave offering and the burial must be Mousterian. But its stratigraphic situation proved that the burial was Aurignacian, not Mousterian. The cleaver flake was accidentally included in earth from levels into which the grave had been dug.¹¹

(7) After the contribution of random factors has been evaluated, differences within assemblages still remain, as do differences between assemblages. From the very remote past, our ancestors performed different tasks in different places. Mousterian peoples arranged the ground surfaces on which they worked to some extent to fit these different purposes, sometimes digging graves and pits, or building walls to divide the surface into different zones. Upper Paleolithic people altered the earth they lived on more drastically, shoveling sediments out of their living areas during

periodic housecleaning, or to smooth out irregularities, lower the level of their living floors, or build dugout structures. Nor do the forces of nature act uniformly. As one example, less buoyant pollen grains fall from the air currents that carry them closer to the entryways of caves than do more buoyant grains. Sometimes the differences in pollen fallout that occurred simultaneously in two parts of a cave vestibule can be as great as the differences that most pollen analysts would attribute to major climatic change. The implications of these factors contradict a widely held tenet of prehistory. One cannot assume that a single long section cut into the deepest part of its deposits ever contains all the important strata that are represented in the site, nor can one ever assume that the activities undertaken during each of the occupations exposed in such a section were identical. There is no such thing as a "typical stratigraphy" for an archeological site.¹² I recognize that this means that our current practice of leaving an intact "witness section" to be excavated by future generations with improved techniques, so that they can check the accuracy of our stratigraphic conclusions, is of dubious value.

Another, related error stems from the fact that the fauna, flora, or sediments recovered from a particular level are sometimes thought to be adequately "representative" of environmental conditions when the level formed. In other cases, there have been attempts to reconstruct prehistoric "diet" and hunting practices based solely on the faunal remains found in a level. Not only does this ignore other components of diet than animals, it also assumes that the bones found represent all the animals hunted. Obviously, this is wrong. Some sites are specialized hunting or butchering camps, where only the remains of those animals scavenged or hunted on a limited number of occasions are to be found, and then the bones are unlikely to represent all elements of their skeletons. Others are base camps or dwelling sites, or sites of other types. Some items such as berries or small animals may have been consumed on the spot, and not brought back to a dwelling site at all. A mammoth, whose bones are huge, is likely to have been butchered where it was found or killed, the bones left on the spot, and only the meat carried to the site where the hunters lived at the time. Until other indications than bones of the amount of meat represented are considered, this means that smaller, more readily transported animals will be wrongly thought the most abundant elements of diet when they are actually overrepresented in the faunal sample.¹³

Yet another related error is involved when one attempts to study potential land use by early prehistoric groups, based on the characteristics of the modern landscape (the procedure of "site catchment analysis"¹⁴). True, advocates of this procedure insist that one should first evaluate changes that the landscape has undergone, but how can one really appreciate the potential uses of different parts of the landscape in Mousterian times, let alone "walk over" any reconstruction of it as it was then? Several other assumptions involved in the procedure, such as the distances prehistoric people might have been able to travel in a given time, and the total time they might have been willing to travel for a particular purpose, are also unrealistic. This technique is best restricted to the study of post-Pleistocene prehistory, for which it was designed, and then "taken with a grain of salt."

Since one cannot assume that the assemblage from a single excavated level represents all the activities undertaken by a prehistoric group, different assemblages cannot ever be *assumed* to be related to the same range of tasks. What is more, because the artifacts the excavator recovers are found in different “activity areas,” because these areas are not uniformly distributed over the surface of any site, and because sites are almost never completely excavated in any case, the sites themselves cannot simply be assumed to be equivalent; the prehistorian must *prove* that they are before considering them to be such. The foregoing observations, incidentally, invalidate all unsophisticated attempts to reconstruct stages of the “microevolution” of environments and industries based simply on relative stratigraphic position.

As a corollary, if one cannot assume that excavated areas or assemblages are equivalent, it is always inappropriate to “increase samples” by combining materials from different discrete levels, since the materials may represent completely unrelated sets of site uses or activities. It is often assumed that one can simply add the faunal remains from different levels of Mousterian to determine the climatic conditions under which the deposits formed, as long as the levels “belong to the same facies.” But since the different levels in all probability witnessed different sets of activities, this practice is fallacious. Equally often, it is assumed that if one finds Denticulate Mousterian assemblages in levels indicative of a particular set of environmental conditions, and Typical Mousterian in other levels indicating similar environmental conditions, there can be no “functional” difference between assemblages of the two types. Since different activities can be and often are undertaken in a single environmental setting, that too is just wrong, and indicates an incorrect understanding of the word “function.”

(8) A most pernicious error is due to the lack among many of our colleagues trained in Departments of “Integrative Biology” in the United States or Faculties of Geology or Paleontology, or even Departments of Archaeology, in other countries, of any real understanding of socio-cultural anthropology, its terminological usage, or its findings. Most frequently, this is manifest in the equation of an industrial complex with “culture” or “a culture.”¹⁵

Bordes thought that all unmixed Mousterian assemblages belonged to one of a few “facies,” which he wrongly thought of as mutually exclusive, non-overlapping phyla of industrial development, the products of different groups of “tribes.” He wrongly thought that “people exchange their genes more willingly than their customary behavior.” His facies were unlike the facies of geomorphology that were his model, since different geological facies can and do intergrade. But the products of his different Mousterian tribes did not. “If a woman from the Quina-type Mousterian was carried off by a man from the Mousterian of Acheulean Tradition, she might perhaps have continued to make the thick scrapers of her own tribe (they are found sporadically in the Mousterian of Acheulean Tradition), but it is more than probable that after her death no one would continue making them.”¹⁶ In order to define the facies the way he did, however, Bordes had to ignore a number of collections from earlier excavations, dismissing them as mixed or otherwise contaminated, since they

did show considerable intergradation. Later, excavations by de Lumley in France and Ripoll and Freeman in Spain showed that even well-excavated assemblages whose stratigraphic position was adequately controlled often intergrade as though they represented two or more facies at the same time.

Bordes (like his predecessors Breuil and Denis Peyrony) also had a mistaken notion of the nature of “tribes,” or of how different socio-cultural groups that are aware of their distinctiveness differentiate themselves from their neighbors. When two individuals meet, it seems ridiculous to assume that the only way they will be able to determine whether they belong to the same “tribe” and should befriend each other, or to different hostile “tribes,” is by the proportions of different types of stone tools in the assemblages of pieces left on the floors of the caves they live in. Yet that is not much of an exaggeration of Bordes’s position regarding the distinguishing characteristics of the Mousterian facies. Even when “tribes” are defined as polities rather than linguistic units, anthropologists know that they use many other marks of their identity (some intangible, but others material and potentially visible archeologically), and different tribes may use tools that are largely indistinguishable. When their social identity is important to them it is signaled in more obvious ways than the make-up of their tool assemblages.¹⁷ Outward signs of identity are given by such aspects of cultural behavior as gestures and body carriage, body mutilation and decoration, clothing, hairstyles, house form, food, the use of flags and banners, and many others. Several of these signs can be distinguished at a distance.

A tribe is a multiperson social category whose activities require substantial space. At Cueva Morín, we have shown that large but partial artifact assemblages that would have to be classified as belonging to different facies are found together, occupying different areas, within one single occupation level, and the coexistence of two separate “tribes” occupying such small spaces would be a physical impossibility (Freeman 1992b). In this case, a *functional* interpretation of the facies differences is the only possible alternative. But what is more, once the arbitrary nature of the distinction between facies was recognized, it became clear that it had been a mistake to spend so much effort in a search for the reasons for the difference between them. If facies intergrade, the distinctions between them are arbitrary constructs; the facies as Bordes defined them really do not exist, and so their differences are trivial, and a search for the causes of those differences is fruitless. The facies concept should be abandoned.

(9) Last, one of the commonest and most dangerous misconceptions in prehistoric research is that artifact assemblages are equivalent to, and behave like, human types. Human bodies are human bodies, and tools are, after all, only tools. Tools have no genes, and they cannot reproduce. It was once believed that every human type used a different set of tools,¹⁸ so that European *Homo erectus* was coterminous with large Acheulean bifaces (choppers and chopping tools in Asia), now-debunked “Pitdown Man” made crude flake tools, while Neandertals made and used either small bifaces made on flakes (early) or such small tools as Mousterian points and sidescrapers (late), and “Cro-Magnon man” was the author of the Aurignacian. We now know that this was simply not so. Except at the very dawn of toolmaking some two and

a half million years ago, when the simple flakes and cores called “Oldowan” tools do seem to have been the only types made by the Australopithecines (or earlier hominids), it seems always to have been true that any human type can be found in association with more than one kind of artifact industry, and some industrial complexes such as the Chatelperronian, for a long time considered the earliest Western European “Upper Paleolithic” industrial complex, seem to have been made by more than one kind of hominid. Both Neandertals and fully modern people are now thought to have made the distinctive tools of that complex. In any case, it has been shown that there is no necessary one-to-one correspondence between artifact types and human physical types.

Consequently, when we ask what became of the Neandertals, the answer must be based on the nature of their physical remains and their DNA. When we want to know if they are related to fully modern men, and if so, how, any proof or disproof of relationship must rest on skeletal morphology and other direct evidence of their biology. Studies of the relationship between the Mousterian and Chatelperronian industrial complexes are irrelevant to that question. The fact that both Neandertals and moderns used backed knives (or other similar tools) must be regarded as absolutely meaningless in that respect. Continuing to rely on artifact comparisons to elucidate genetic relationships is to confuse the mechanisms of biological and cultural evolution.

Any kind of evolution is basically just adaptation, but when human bodies are involved its mechanisms or forces are very different from those at work when we consider the evolution of systems of belief and behavior. People who look different may each learn how to behave like the others. While the nature of their bodies can only change relatively gradually, by passing on changes or recombinations of their genetic material from one generation to the next, their behavior can change much more rapidly by the “inheritance of acquired characteristics.” We pass on what we have learned to others, and that process is almost instantaneous when compared to genetic inheritance, since we can and do learn new behavior not just from our ancestors, but also from our contemporaries (including those younger than ourselves) during the course of our lifetimes. François Bordes wrote that the evolution of Paleolithic industrial complexes was “ramifying evolution” (*évolution buissonnante*), in which the various branches crossed and split in complex fashion. His precocious conclusion was regarded as strange and little used by prehistorians, who preferred to think of cultural evolution as operating by mechanisms in general analogous to the “forces” of neo-Darwinian biological evolution: selection, mutation, gene flow, and genetic drift. Nevertheless, it should have surprised no one. Because of the possibilities for the rapid exchange of information between peoples whose backgrounds and experiences are very different, *all* cultural evolution is, in fact, “ramifying evolution.”

CONCLUSIONS

This list of pitfalls for the unwary omits many, but highlights some of the most important. Despite the fact that the list of potential causes of error we must still overcome

is daunting, the progress that had been made in understanding the Mousterian complex of industries nonetheless surpasses it.

While museum collections from earlier excavations throw considerable light on Mousterian adaptations, much more can be gotten from careful excavation with up-to-date techniques. Our experience shows that there is still much unexpected information that can be learned from the excavation of intact sediments in many sites that were discovered and first (partially) excavated long ago, and it is obviously economical to investigate sites that are already known. But that experience also suggests that more effort should be devoted to searching for and investigating new, previously unknown sites,¹⁹ and particularly those in the open air rather than in caves or rock-shelters. No doubt this search will be difficult, in light of the lush vegetation cover that characterizes the Cantabrian landscape, but road and railway cuts, foundations for new construction, wells, streambanks, beaches, and, in fact, any natural or artificial feature that exposes sediments of appropriate age should be thoroughly examined. This effort should be accompanied by an exploration of sunken undersea grottoes and caves that were partially drowned by freshwater lakes and siphons; French divers have shown that underwater Upper Paleolithic sites exist, and there is no reason to believe that changes in drainage and sea level will not have preserved some Cantabrian Mousterian sites as well. But careful survey and mapping are not all that is needed.

If Mousterian studies are to continue to progress, painstakingly careful excavation will have to become the rule. This will have to combine the best techniques of horizontal as well as vertical control: we must attend assiduously not only to vertical stratification, but also to those spatial variations in the strata that betray the presence of natural differences in or artificial interference with the "orderly sequence of deposition." Thorough recording, coupling detailed note-taking, precise drawing of maps and sections, and thorough photographic documentation, must unflinchingly accompany the excavation. New kinds of artifact classification will have to be developed. Subjecting their results to appropriate statistical procedures, we can first classify the toolkits they compose, discovering how they relate to all aspects of the contextual data found with them. Then we can compare them and the activity areas in which they were found. Working outward, we should be able to demonstrate that comparable areas exist in multiple sites, and perhaps that the functions of some sites are in fact equivalent. By means of comparisons of comparables, we should be able to reconstruct subsistence and settlement systems in relatively broad and accurate terms, and then to discover how functionally similar activity areas and sites differ stylistically. Attention to minor differences in artifact manufacture and wear may someday permit us to identify the different "signatures" of different members of a team and overlap in team membership.

The criticisms of past investigations made or implied in this chapter are not meant to denigrate the contributions of the pioneers of Cantabrian prehistory. As I said earlier, we have all been guilty of at least some of the errors I have outlined above. Despite this fact, each generation of investigators in the past has truly added something to our wisdom about the Mousterian. Cantabria has contributed

more than its share of this wisdom. Can we continue to make as much progress in Mousterian studies in the future as we have in the past? I for one am most optimistic. But in the last analysis, the answer to that question depends not on my own opinion, but on the behavior of the members of this and future generations of investigators.

NOTES

1. Of course, there are bones from Axlor and Lezetxiki in Basque country—a fragment of maxilla, several teeth, and a long bone—that have been diagnosed as Neandertal. They have, however, not helped in the determination of relationships between Neandertals and moderns. A fragmentary left parietal found at the cave of la Flecha and possibly from its Denticulate Mousterian levels is thin and fully modern in appearance (Freeman 1964: 269). More information on the Spanish Neandertals is given in González Echegaray and Freeman (1998). I have not yet seen the recent publication of the discovery of a large number of Neandertal remains in the site of el Sidrón in Asturias by Javier Fortea's team (Fortea et al. 2003). Because I only know them from the presentation to this workshop, I omit any discussion of these finds, although they promise to be of great importance. They were found with some 30 stone tools, a collection that is still too small for accurate diagnosis.

2. This point was made more elegantly more than a half century ago by Sir Mortimer Wheeler (1950).

3. A fuller description of recovery at Abric Agut is published in González Echegaray and Freeman (1998). A previous attempt was made to find plant materials at Cueva Morín, using cruder methods that produced no useful result.

4. "Each terrace corresponds to a period of warmer climate, to an *interglacial* period" (Leroi-Gourhan 1957: 17).

5. A clearly fallacious example was an attempt to establish the age of a Lower Paleolithic stone industry by dating the basalt of which its stone tools were made. The resulting age might be correct for the basalt, but obviously could be millions of years earlier than the date of toolmaking.

6. "Comme la Géologie et l'Histoire qui l'encadrent, la Préhistoire est avant tout une science de la chronologie. Les comparaisons et les explications ne sont valables que pour des faits insérés dans le déroulement général du temps et datés les uns par rapport aux autres" (de Sonneville-Bordes 1981: 5).

7. Such caricatures are wrong. The current state of quantum dynamics, gravitational theory, or current debates about cosmology, should make that evident.

8. That is, aside from analogies with the mechanisms of formation of other, later soil shadows found in Northern Europe (see, e.g., Clark 1957: plate 17b).

9. For an explanation of statistical requirements of "simple random sampling" programs and their uses, see any good text on basic statistics, or Deming 1966: a probability sampling program "is carried out according to a statistical plan embodying automatic selection of the elements . . . concerning which information is to be obtained. In a probability sample neither the interviewer [the one drawing the sample—LGF] nor the elements of the sample have any choice about who is in the sample. . . . [I]n a probability sample the procedure for forming the estimates is automatic, being laid down beforehand as part of the sample design. Unless these conditions are met, probability theory can not be used to appraise the precision of the results, and a survey can not be characterized as a probability sample" (p. 10). The regular spacing of sampling units that is needed for the best surface gradient mapping can never be

random sampling, since some set of elements of the sample determines what other elements are to be included.

10. One of the best introductory works that deals with tests of use to prehistorians is still Siegel (1956) (later editions are available, but not necessarily preferable).

11. Some of the causes of interpretive error related to statistical testing are not so obvious. There are limits to the use of the frequently used chi-square test. The data used should be raw counts, not percentages. When the data are cast in a table larger than 2×2 , no cell can have an expected frequency less than 1, and fewer than 20 percent may have expected frequencies less than 5. For the 2×2 case, all expected frequencies must be 5 or more. When these criteria are not met, the Fisher Exact Probability test must be used instead. Correlation analysis and principal components analysis are two of our most powerful approaches to the recognition of types that vary together in related fashion because they were used for related tasks, or for other reasons. Correlation coefficients calculated for the correlation test itself, or as data for principal components analysis, that are based on seven or fewer cases, are mathematically invalid. H. Harman's (1967) *Modern Factor Analysis*, though now old, is still to my way of thinking the best introduction to the theory of and approaches to principal components analysis. Harman additionally presents the rationale for the rotation of axes, mathematical transformations that do not alter the solution but do facilitate the interpretation of the resulting structure of the solution. It is often assumed that because assemblages often vary widely in size, large differences in the numbers of tools of any particular type in different samples will lead to false correlations. That is certainly a possibility. Several means have been proposed to "standardize" sample sizes in order to compensate for this problem. Many of them, such as the transformation of the raw counts to their square roots or logarithms, are essentially useless—they really do not eliminate the size difference between the samples. Some, such as the "chi-square" transformation, yield results that are hard to evaluate. Others, such as the transformation of the counts of tools to percentages of collection totals, "constrain variance" in ways that *always* produce spurious negative correlations, wrongly making it appear that some types take the place of others, when in fact no such replacement actually occurs. The easiest and most justifiable, rational, and effective way to transform data is the use of rank-order correlation (I use Spearman's r) instead of the Pearson product-moment correlation coefficient. If data are ranked, so that the most abundant type in each sample is given the highest rank and the least, the lowest, the formula for the ordinary Pearson's correlation coefficient automatically produces Spearman's r . However, the dangers of using untransformed raw counts and Pearson's correlation coefficient with archeological data are often more theoretical than real, and often very nearly identical results will be obtained from the use of either approach to correlation.

12. This affirmation is made as strongly and justified in greater detail in the chapter by J. González Echegaray (1984: esp. 265–67).

13. I have dealt with this topic in greater detail in my article "The Fat of the Land" (1981).

14. This procedure was pioneered by a group under the direction of Eric Higgs (1975: 223–24).

15. "Artifacts made by the same people are an industry. A number of industries belonging closely together in time and space are a 'culture.' Ideally, of course, a culture will consist of far more than just so many groups of artifacts, burial places, and settlement types" (Roe 1970: 27). Despite the fact that this definition includes more than stone and bone tools, such "enumerative" definitions of culture are now regarded as anthropologically outmoded. A more modern definition is that culture consists of a society's system of beliefs and behavior.

16. "l'Homme échange plus volontiers ses gènes que ses coutumes. . . ." "Si une femme du Moustérien type Quina était enlevée par un homme du Moustérien de tradition acheuléenne, peut-être continuait-elle à fabriquer les raclours épais de sa tribu (on en trouve

sporadiquement dans le Moustérien de tradition acheuléene), mais il est plus que probable qu'après sa mort personne n'en fabriquait plus" (Bordes 1968: 144–45). This quotation illuminates several misunderstandings, from one regarding the relative rapidity of exchange of information and genetic material (even the French wear Levi's™), to a strange idea of tribes, and the now discredited concept of marriage by capture.

17. The study of such signing is the subject of the field of semiotics. Specialists have identified many kinds of signs, nonverbal as well as linguistic, that are used to differentiate social groups. See, for example, Barthes 1970 (esp. 25–30). Bogatyrev (1971) is an exemplary study of clothing that attends to its signing functions.

18. This idea is still not completely dead. Derek Roe (1970: 28) observes that "on occasion there may even be evidence for correlation between a culture and some specific human physical type."

19. Exemplary work along these lines has in fact already begun. See such publications as *Arquenas*, *Sautuola*, the *Boletín Cantabro de Espeleología*, the *Revista Arqueológica*, and other publications produced by local speleological and/or archeological groups, such as the *Colectivo para la Ampliación de Estudios de Arqueología Prehistórica* and the *Grupo de Espeleología e Investigaciones Subterráneas Carballo/Raba*, for illustrations.

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P A R T



PALEOLITHIC ART

There are five chapters in this section, perhaps because it has been a more recent focus of my research than have the Mousterian and Lower Paleolithic. For much too long I resisted entering a field of study where it seemed to me that ill-informed opinions were as accepted as were well-grounded ones. The specialists in this field of study seemed as “fuzzy-minded” as their audiences. It took my colleague González Echegaray many years to convince me that one could approach the study of Paleolithic art in rigorous fashion and that doing so could be rewarding. With my colleagues, I have spent many hours in the site studying the Paleolithic depictions on the walls and ceiling of the famous painted cave of Altamira (see Freeman and González Echegaray, *La Grotte d’Altamira*, Paris, La Maison des Roches, 2001), and I have acquired a firsthand familiarity with many other decorated sites in the Franco-Cantabrian region. I believe that this experience justifies my right to my opinions, even when they differ from those of the majority of my colleagues.

There are two lamentable tendencies among those interested in Paleolithic art. The first is to expect that we will eventually find a single, universal explanation or drive that accounts for the production of all art, especially the art of early periods. The second is the unthinking acceptance by most readers, even those who are

themselves specialists, of the forcefully articulated bright ideas of a few influential scholars, even when they contradict the personal experience of the readers.

The first cave artists were members of our own species, whose bodies were as complicated as our own, and whose behavior we might expect may prove to have been as complicated as our own in many fundamental ways. That should be reason enough for us to pause when considering the possible motives for the production of Paleolithic art.

Just at present, the dominant theory seems to be that of David Lewis-Williams, whose studies of the production of San Bushman art led him to postulate that Paleolithic wall art is related to shamanism and that what has been depicted are generally shamanic visions (see, e.g., Lewis-Williams, *The Mind in the Cave* [London, Thames & Hudson, 2002], and elsewhere). Although Lewis-Williams is to be commended for his research and for his interesting conclusions, which might certainly apply to *some* Paleolithic decorations, it seems quite unreasonable to believe that they apply to *all* or even the majority of those depictions.

A French pioneer in the study of Paleolithic art, André Leroi-Gourhan, himself a reputed ethnographer, observed that we have learned what we know about shamanistic practices from the lips of contemporary informants, but there is no longer anyone alive who can provide us with firsthand testimony about Paleolithic art. If there is one thing sure, it is that in that art “the same image has embodied spiritual entities from radically different mythological contexts. That is why prehistorians cannot follow the trail of shamanism without changing their methods and at least provisionally giving up their desire to understand everything they study” (“Le Préhistorien et le chamane,” in *L’Ethnographie* 74–75: 19–25; my translation). My own perspective departs from Lewis-Williams’s and is much more like that of Leroi-Gourhan: I believe that there are serious problems in applying Lewis-Williams’s theory, and that there are probably as many other valid explanations for the production of the wall art as there are decorated sites.

In the first of these five chapters I have tried to show how one might approach the study of Paleolithic art and to indicate once and for all that certain assumptions—such as that there are no natural models for angular geometric forms in the natural world (either external or internal to the artists) or that prehistoric art is essentially a “proto-language”—are blind alleys that lead the investigator nowhere.

It seems to me that the next chapter, “The Many Faces of Altamira,” addresses an issue of considerable importance, and one that is usually ignored by most other students of Paleolithic art. In it, I show how preconceptions from modern life have influenced ideas about prehistoric art and past lifeways, and how important prehistoric localities have in turn influenced our modern world.

Techniques used by prehistoric artists to enhance particular figures are the topic of the next chapter, “Techniques of Figure Enhancement in Paleolithic Cave Art.” This is once more a subject that has been ignored by most scholars. The section concludes with two chapters on just what is meant by the term “sanctuary” when it is applied to prehistoric art, and speculation about how one such sanctuary, that at Altamira, might have been used. Although many previous authorities have treated

Paleolithic caves as “sanctuaries,” none has defined in understandable terms what is meant by that concept. My reasoning that Altamira was indeed a sort of sanctuary is by no means original, but I believe that I have gone further than previous authors in the empirical definition of the term and believe that I have offered a definition that is as reasonable and verifiable as it is replicable. Furthermore, I dissociate myself from those who think that *all* decorated Paleolithic caves were sanctuaries. The last of these chapters is admittedly more speculative. It discusses the way in which Altamira might have served as a place of initiation. In this case, however, my speculation is based on controlled conjecture, not on the free play of imagination, a constant plague in the study of prehistoric art and one reason I avoided entering the field for so long.

Meanders on the Byways of Paleolithic Art

From the very moment of first discovery of Paleolithic cave art, concern for its significance and the most appropriate techniques for its interpretation have caused great intellectual ruminations on the part of those scholars fascinated by mankind's prehistoric past. The broad general lines of the principal speculations on the subject have been summarized, and their substance subjected to a critique as hard as it was overdue, by Peter Ucko and Andrée Rosenfeld in their work *Palaeolithic Cave Art* (1967). Possibly due to the rigor of its authors, the publication of this admirable little book brought with it unhappy consequences for cave art studies that they probably did not foresee and certainly could not have intended. For more than a decade after they demonstrated how inadequate, inconclusive, and sometimes even stupid were so many of the ideas of the pioneers in the field, new investigators seemed reluctant to advance into such dark and treacherous terrain. Consequently, the study of cave art seemed in danger of degenerating into a debate focused exclusively on the most concrete and superficial aspects of the material, or of falling into the hands of fantasy-ridden dilettantes.

Fortunately, fresh discoveries and the irrepressible enthusiasm of a few very fine scholars both young and old have now breathed new life into what recently looked like a moribund inquiry.

The degeneration of the field would have been tragic. First, cave art offers us information as unique as it is invaluable about aspects of the evolution of the natural environment, the ecology (in the broadest sense) of communities of the past, and the lifeways of our prehistoric ancestors and relatives, including details of their deepest emotions and beliefs. Comparable information is simply not provided by the study of tools and their attributes, or of contextual materials—sediments, pollen, fauna, etc.—nor by the shapes of buildings, nor by the most scrupulous study of numerical and spatial associations of excavated materials. It is the obligation of prehistorians to renew their dedication to the extraction of all possible information from this inestimable source, and to elaborate new and sounder interpretive techniques in light of the critique presented by Ucko and Rosenfeld. That is especially urgent now that we know that the very existence of many of the precious monuments of prehistoric art is in imminent peril.

I personally don't pretend to see the whole form of the new "science of cave art" that must be shaped. But I can see the indistinct edges of some promising tracks that have not yet been sufficiently explored, and I believe that they might lead a certain distance towards our goals. What is more, given the fact that so many old and well-traveled avenues have ended in the foggy nowhere, I don't think that we can be blamed for exploring beyond the limits of traditional terrain. In trying to find one's way out of a labyrinth, what is important is to follow a systematic search to its conclusion, no matter what the direction chosen. Naturally, if we see that a track is a blind alley, no matter how wide, attractive, and well-traveled it may be, we should abandon it and set out in a fresh direction.

In this chapter, it would be impossible for me to follow any research avenue to its conclusion. My aim is more modest. For the moment, I am simply going to indicate or *re*-indicate some directions that have so far not been proven unproductive. Perhaps some of them will lead to new perceptions of the truth about the lives of our prehistoric forebears. The ground to be covered is immense—too large for the small number of explorers now in the field—and I hope that these lines will stimulate my readers to undertake intensive research along some of the lines that I am about to sketch superficially.

■ SOME DEFINITIONS

Before we begin our explorations, we need to know what it is that we are investigating. Here are a few (mostly borrowed) definitions and preliminary observations that may guide our quest. First of all, to me the word "art" bears no load of value, and rings no emotional bells. I like Arnheim's (1971) definition of art (the graphic or plastic kinds) as "visual representation," and much of what I must now write follows him closely. His definition excludes a great deal, but what it leaves us is nonetheless an immense field, for the phenomenon represented is never simply a part of the external surroundings or "environment"; it is always and at the same time an internal condition of the artist—a condition that is psychological, emotional, and intellectual (Croce tells us that art is "contemplated emotion")—and always reflects the artist's

training and position as participant in a cultural system and actor on a social stage. In fact, we might well say that the only reality that can be drawn, engraved, painted, modeled, or sculpted is an internal reality, since perception itself is, in the last analysis, a cerebral phenomenon.

Those comments also imply that no representation ever really replicates its subject. The only real replica of an object would be another exactly identical object. A painting of a horse is obviously not another identical horse, but its representation: the generalized structural equivalent of a horse in another medium. Now since the artist's perception of the horse is really a brain state or complex of brain states, rather than a bit of external reality, and since a representation can never be a replica, there is no such thing as completely realistic art in any medium—that should be immediately evident from a comparison of a three-dimensional statue, a painting, and a drawing or engraving of the same object. The sculpture is really no more a true replica than the painting, but it is a representation with an added (third) dimension, while paintings, engravings, and drawings (and photographs as well) are two-dimensional, and the most “realistic” of them is much more limited an approach to realistic rendition than is (potentially) a sculpture. In this sense, all art is abstraction.

■ THE RELEVANCE OF CHILDREN'S ART

Analysts of children's art have also provided important data bearing on our study. Some interpreters of prehistoric art have held that in art, phylogeny recapitulates ontogeny. They claim to have found in prehistoric art developmental stages that parallel the products of the child slowly learning to draw. Those claims are exaggerated. There are certainly forms and techniques in Paleolithic art with parallels in the products of modern children, but while on the one hand it is quite possible that some Paleolithic figures are the work of children, on the other the noted parallels are “universals” that are also present in the work of modern adults, including trained artists.

The sequence of developmental stages in the child's acquisition of artistic ability is well summarized by Arnheim (1971). When the child begins to draw, it attempts to reproduce its brain states not just in the medium of pencil, crayon, paint, and paper employed, but also in the movements of its body, some of which seem to us adults totally unrelated to the external object that the child may tell us it is drawing. The first artistic products of children are scribbles. Later, gradually, these seemingly chaotic scribbles begin to take on form. Almost always the first shape produced is a roughly circular outline; the child uses this as a representation of anything whatever—a house, a flower, a person, an animal, etc. What the child apparently attempts to produce is an outline closed on itself that represents the “wholeness” of the object without any specific details, which at this stage of development are still considered unimportant. Thus, we may say that infantile art is “abstract” in the sense that its subjects are not highly differentiated, although as far as the child is concerned, what has been produced is a satisfactory and totally adequate representation of the subject.

As the child continues to develop, its representations become gradually more and more differentiated. One may with some justice claim that in the last analysis, the major difference between the art of a child and that of a Leonardo da Vinci is that the drawings of Leonardo are much better differentiated. On the other hand, it is also certain that any artist may deliberately choose to produce an undifferentiated representation, and may even intentionally reproduce patterns or forms that replicate the *Gestalten* they produced as young children. This is one direction chosen by artists such as Klee or Miró; though they claim to be influenced by the art of living “primitive” peoples, the child-like element in their work is self-evident. But another important factor in their art is the intentional reduction by the skilled adult artist of complex details, that he is quite capable of rendering, to vastly simpler form. Rhoda Kellogg, a leading authority in the field, suggests that artists of all times have probably drawn on motifs familiar to them from their artistic activities as children (1970: 208–45). What is more, there is reason to believe that certain simple linear or geometric patterns have a species-wide aesthetic appeal that is rooted in common perceptual processes that are not necessarily related to any attempt to produce recognizable depictions of the external world. As a consequence of these observations, any theory of art history that postulates that either abstract or “realistic” representations have inherently greater intrinsic artistic merit, or that the two must represent successive chronological stages in the prehistoric evolution of adult art, must simply be wrong.

These observations lead to a further comment. Some authorities have suggested that the so-called macaroni or superficially chaotic interwoven squiggles, that at times suggest the outlines of an animal figure, represent the most primitive or “infantile” stage in the evolution of art. In reality, these play with visual depiction in a sophisticated and subtle way that is probably far removed from the most rudimentary “original” art, whatever that may prove to be. The artist’s dominance of medium and the sophistication of the resulting visual “puzzles” of suggestive interlaced meanders are well beyond the capacity of the infantile psyche. It is one thing to select from a battery of other techniques that have already been mastered the use of scribbles to produce (and conceal) depictions, and quite another to scribble simply because that is all you know how or are able to do.

■ ASPECTS OF ARTISTIC PRODUCTION: APPROACHES TO A STUDY

There are several directions from which the study of the documents of prehistoric art might fruitfully be approached. Let me briefly outline a few of them. For present purposes, the process of artistic production may be said to have four fundamental aspects, although the four are by no means always so different as my discussion will suggest, for in reality they are always found mixed and blended. In the first place, an artistic act requires an agent, the artist, whose consciousness in all its individuality inevitably influences his or her product, whether or not the artist was aware of or intended that to happen. In fact, consciousness is the result of the continuously developing activity of the brain in interaction with its environment. It is perfectly

legitimate to study Paleolithic art in this perspective, from the psychological point of view.

Most attempts at a psychological or psychoanalytic analysis of prehistoric art that I know of are unsatisfactory, and even ridiculous, though that need not be the case. They have two fundamental deficiencies. First, they are usually produced by authorities whose firsthand familiarity with the corpus of Paleolithic art is almost nonexistent. Works by specialists of the caliber of a Geidion, who knew many of the more important cave art sites well, are rare exceptions, and though I disagree with many of his interpretations, he was so familiar with the representations that his interpretations are not to be lightly dismissed. I have also found admirable passages in the work of Herbert Read and Anton Ehrenzweig and in that of Jung; though he himself was not a frequent visitor to the painted caves, his knowledge of the world's art (including Paleolithic depictions) was extensive enough to give him an unparalleled basis for comparative discussion.

The second failing that has characterized such analyses stems from a widespread tendency among psychologists to think of things in strictly ethnocentric terms, and to apply analytical criteria devised to deal with the performance of members of one society across cultural boundaries, as though they were universally valid. Depictions that would suggest particular complexes or psychological problems if produced by a late twentieth-century European or American of European descent might well have no such meaning when produced by a member of a prehistoric hunting and foraging society. Kellogg (1970: 204) found one eight-year-old's drawing suggestive of the work of a schizophrenic mind (to her credit she refrained from making that diagnosis without further information), and from a young American schoolchild, the imagery and detail in the figure are highly unusual. I suspect, however, that many European or American analysts might think the drawings of a perfectly normal Balinese eight-year-old, full of fierce beasts and threatening supernatural figures, all rendered in minute detail with surprising skill, show the same degree of imbalance. The Pygmies Turnbull studied in the Ituri forest are unused to seeing objects at any great distance. When taken from his native forest to the open savanna of Ishango, his informant Kenge could not believe that animals seen from afar are not really as tiny as they appear to be (Turnbull 1962: 249–60). Members of his society would be likely to confuse scale differences in art intended to show linear perspective with real differences in size. Members of other societies may pay more attention to the white background of a Rorschach card than to the colored blot. If contemporary societies exhibit such differences, and tolerate such widely divergent imagery that what would be normal in one indicates sickness in another, how much more caution is needed in interpreting the products of artists removed from us by an immense temporal gulf, that must coincide with an equally great cultural chasm.

There have been recent efforts to remedy these deficiencies. Dr. Gerard Neuman and his associates have had considerable success in bringing archeologists and experts in Paleolithic art together with authorities on children's art, art therapists, practicing artists, psychologists, and psychoanalysts in joint working sessions and symposia sponsored by his Institute for Psychodynamics and the Origins of Mind. As I have

mentioned, valuable insights may be gleaned from the published work of the few psychologists and art analysts who managed to acquire a familiarity with the documents of prehistoric art, and were mindful of the effects of cultural difference on artistic performance, choice of media, and subject matter. Though I cannot claim to have the background to follow this track myself, it is to be hoped that others, better prepared, will continue to do so.

If the artist cannot escape his own individuality, no more can he really escape the conventions of his society and the constraints his culture places on symbols he uses in thought, behavior, and communication. We now know that even manifestations of madness are culturally patterned. Art is almost always produced to be "seen" by an audience, whether that audience is alive and tangible or aloof and supernatural. Its success depends on its suitability for and intelligibility to that audience. From a historical and sociological viewpoint, one may try to identify significant symbols, isolate recurrent associations, and trace their temporal development, in hopes that they will lead us to recognize cultural conventions, norms, values, and stylistic traditions in Paleolithic art. The well-known efforts of the Abbé Breuil, Anette Laming, André Leroi-Gourhan, Francisco Jordá, Antonio Beltrán, Herbert Kühn, and Eduardo Ripoll exemplify this direction of research, and as the findings of Alexander Marshack show, it is far from being a blind alley.

Since art is representation, it follows that something is represented, and much of the time its subject matter is drawn from observations of the artist's surroundings. Consequently, we may legitimately approach the artistic product as a reflection of its subject matter. The study of art as a mirror of the artist's surroundings is of the greatest interest to anthropologists and natural historians, paleobiologists, and others concerned with the evolution of nature. Through such studies, we have come to know a good deal about the physical appearance and something about the behavior of animal species that became extinct millennia ago.

Finally, the representations are made of some material, using a specific set of tools and techniques. The study of the material and technical aspects of art can also be very enlightening. The careful study of order of execution of drawn and engraved lines and distinct masses of color undertaken by Marshack for some Paleolithic figures is a good example of the utility of this line of investigation. Identifying pigments and tracing them to their sources can inform us about the limits of the territory utilized by a prehistoric population, or about the existence of networks of exchange between areas far removed from one another. The comparison of the pigments used in paintings on the walls of an inhabited site with the characteristics of coloring materials found in the site's different occupation layers can provide useful information about the relative dating of the paintings and the occupations.

Eventually, it is even conceivable that some non-destructive means can be found to provide absolute dates for Paleolithic representations using the coloring material itself as datable material. The science of chronometry has made considerable progress in such directions in recent years.

In the rest of this chapter, I should like to discuss briefly each one of these four aspects of the study of Paleolithic cave art. The most logical direction for discussion

will be to proceed from the familiar and concrete to the abstract and unfamiliar. We begin by examining the decorations themselves from the standpoint of their techniques of realization and the interrelations between figures. This will be followed by a discussion of the art as a mirror on the outside world—a reflection of environments of the past. These are the aspects of our study with which the majority of readers will have most acquaintance.

LOOKING IN THE MIRROR OF ANOTHER'S EYES

But before proceeding, I must anticipate an obvious objection. If, as I have said, cave art shows us reality transformed by the perceptions and emotions of the individual artist and the beliefs of his or her social group, how can I pretend to understand its meaning? Didn't our early ancestors perceive things much differently from ourselves? Weren't the mental processes of Paleolithic peoples and their cultures and societies very different from our own? I admit that they were, in fact, different, but I believe that the available evidence indicates a degree of parallelism between the Upper Paleolithic artists and ourselves that justifies our cautious attempt to reconstruct aspects of the meaning of their art along general lines.

It is quite true that it would be dangerous to attempt to understand the art of beings whose perceptual processes were very different from our own. And, it is also true that other animal species cannot perceive the world as we do. Probably no one has made this point better than the pioneer ethologist Jakob von Uexküll did many years ago (1934). His germinal article opened the eyes of the world to the possibilities for interpreting the behavior of animals very different from human beings.

The basic point von Uexküll made is that a single environment is several different "worlds" as it is perceived by animals as different from each other as a fly, a snail, a dog, or a man. To illustrate, let me summarize an extreme example, von Uexküll's comparison of the perceived environments or *Umwelten* of a fly and a human. Though it may seem frivolous, its implications are really quite important.

For the human, the inside of a living room offers several surfaces upon which he can walk, sit, or lie, but all are located within a few feet of the floor, all are parallel to the earth's surface, and almost always all are below his standing eye level. He sees these surfaces and their edges in sharp focus, in three dimensions, and, if they are colored, in a wide range of colors. The floor has a particular significance for the man that is not shared by the walls and ceiling which he cannot walk upon.

To the fly, the same room, even if devoid of furniture, is a completely different world. In the first place, the empty room has six surfaces on which it can walk or rest, not one. In the second place, the fly does not see its surroundings as we do. Its compound eyes permit it to discern what must be less focused, more generalized contrasting blobs or splotches, rather than the sharper forms the human eye distinguishes. The fly's eye is sensitive to a narrower band of colors than ours, and it does not permit stereoscopic vision. Another significant point is that those aspects of the room which mean one thing to a man. Such as a piece of rotting meat on the ground, have entirely different meaning for the fly. If a fly could paint what it

perceives in its environment, it would be quite impossible for us to recognize what it intended to represent.

Naturally, our earlier ancestors, members of other hominid species, were not as different from us as are flies. Nevertheless, their skeletal structure, their cranial capacities, and the shapes of their endocranial casts all strongly suggest that their cerebral development was quite different from our own. There are good reasons to suspect that had they produced cave art it would have been unintelligible to us. This suspicion is strengthened by the observation that chimpanzees, our rather close anthropoid relatives, who can be taught to use fingerpaints and even to communicate with us and with other chimps using rudimentary nonauditory language symbols, have never produced any recognizable artistic depiction, though they do occasionally produce rudimentarily organized and apparently non-representational paintings that we may find aesthetically pleasant.

But, the converse of these observations is also true. Upper Paleolithic people were anatomically identical to living humans. Their anatomy, stature, cranial capacity, and apparently even their cerebral organization (so far as can be judged from endocasts) were just like our own, as far as we can tell. The fact that they produced tools, structures, and burials that we can analyze and understand, and even more important, the very fact that they produced recognizable artistic depictions of animals and other subjects in understandable patterns, indicates as surely as any evidence could that their mental processes and behavior were potentially as complex as those of living humans, and that their processes of perception may be considered for present purposes to be identical to our own. Whatever differences there may be between us are differences of degree, not kind. The very existence of recognizable figures and patterns in Paleolithic art is in itself a guarantee of the legitimacy of our attempt, based on the assumption of a considerable degree of continuity between the perceptual apparatus and mental faculties of the cave artists and ourselves, to analyze that art and seek to find its meanings. Theoretically, at least, major aspects of the meaning of the prehistoric representations and their organization should be accessible to us.

The same cannot be said with such certainty about the products of earlier hominids. We do not in fact know when in the trajectory of hominid evolution our relatives first were able to utilize perceptions and brain states that foreshadowed in complexity those of modern humans. It is entirely possible that some Neandertals, or even earlier hominids, had these capacities and exercised them at least occasionally. But if so, there is precious little evidence (one could say virtually none prior to the Neandertals) of the corresponding behavior in the archeological record. Perhaps our earlier ancestors demonstrated such complexities in the recitation of myths or epic poems, or in song, none of which would have left durable evidence for the archeologist. But for the moment we are best advised to avoid imputing modern motives, emotions, and feelings to *Australopithecus* and early forms of the genus *Homo*, relying instead on research techniques analogous to those used by ethologists working with other animals: we should restrict our interpretations to the limited durable evidence for past behavior recovered from early sites by excavation. In those cases, we do not

have the advantage of a corpus of contemporary art as supplementary evidence for behavioral complexity or as an indication that our interpretations might legitimately range further afield.

In contrast, Upper Paleolithic peoples, despite the temporal distance that separates us and the modal gulf that must separate their socio-cultural systems from our own, have revealed themselves, particularly through their art, to be our very close relatives, rivaling us in their capacity for complex cultural behavior. The cave painters have not hidden their world from us. Instead, they have translated the reality of their environments into terms that, in appreciable part, we should be able to understand.

On the other hand, the interpretation of Paleolithic art is by no means easy. The world reflected in the decorations seems strange to us, and penetrating its mysteries, even in their most general outlines, is not a simple task. The careless or untrained observer will have little success in producing adequate reconstructions of past environments from the works of art produced by prehistoric people. The study of Paleolithic art requires caution, scrupulous attention to detail, and thorough preparation.

■ THE STUDY OF CAVE ART: THE DESCRIPTIVE/CLASSIFICATORY PHASE

The requisite first step of any study is to find the representations themselves and examine them as carefully as possible. It is essential to produce an exact description of each figure, including its measurements and orientation, to describe the different stages involved in its rendition in order and the techniques utilized at each stage, to detect later secondary additions or alterations that may have changed its significance, to determine its precise position in any series of superimpositions, to define its place within any composition and the exact placement of the figure in the topography of its cave or shelter, and finally, to describe its associations with other representations in the site.

The depictions in any decorated site must be studied in their total physical, topographic, and artistic context. This necessarily means that a precise map must be made of the site and all its galleries, and the location of each figure noted in its proper position on the plan. As Leroi-Gourhan (1971: 82) has observed, one of the enormous advantages of the study of cave art is that the depictions are found exactly where they were executed, and where they were intended to remain and be seen; that substantially enhances the potential of studies of the relationships and associations of the figures among themselves, and between them and the rest of their surroundings.

A study of the site itself may prove highly informative. It is essential that the situation, size, relative and absolute elevation, and orientation of the galleries and of the cave mouth, or the direction of exposure of the rock face, in the case of a shelter, all be noted. In the same way, it is necessary to situate the site in the modern topography, and to determine its position in the reconstructed topographic and environmental setting of the period when its decorations were produced. The elevation of

the cave over the floor of the valley in which it is situated may indicate when it was formed and became available for human habitation. The orientation of the shelter or cave mouth with respect to the prevailing winds and the direction of the sun at different times of year may help determine whether or not its occupation is likely to have had a seasonal aspect. Its relationship to the present and prehistoric landscapes may provide indirect data about the reasons for which the site was selected for occupation and about the environmental resources available to its occupants. If the age of the site and its decorations can be determined within acceptable limits, it could prove possible to relate it to other more or less contemporary sites, with or without cave art, in the region. All of these kinds of information will help us reconstruct prehistoric systems of subsistence and settlement, and lead us to a better understanding of the position and function of decorated sites in such systems.

■ THE PRIMARY EVIDENCE

We now know enough about cave art to recognize that its depictions can be divided into several classes, based on subject matter and on technique of rendition. In the first place, there are those signs that can only be called enigmatic scribbles. Next, there are single lines, small irregular groups of lines, dots, and alignments of dots. There are also more or less regular geometric figures composed of arrangements of lines or dots that are generally unintelligible, though they can be subdivided into types called claviforms, tectiforms, scutiforms, etc. Occasionally, some of these figures can be further interpreted with some probability of success. This is the case, for example, for some of the depictions in the cave of Tito Bustillo in Asturias, that seem to represent vulvas (de Balbín and Moure 1981). It is also the case for signs that seem to represent darts, arrows, or other weapons, noted with some frequency at Castillo, Altamira, and many other decorated sites. Some of these are interpreted as throwing-sticks, analogous to ethnographically known specimens. There is also some similarity between certain linear geometrics from Castillo and “valve traps” recovered from Mesolithic contexts in Northern Europe. Nevertheless, such similarities, while striking, are by no means completely conclusive: there may be alternative interpretations (some “darts” could equally well represent “plants”) that cannot be negated, and for that reason we must admit that we do not know with certainty, and perhaps we shall never know, what kind of material object, if any, they were intended to represent.

Some such figures may allude to real objects from the cultural or natural environment, but others, as I shall explain later, might just as well be sketches of fantastic visual forms that proceed directly from the brain or the eye. Some “signs,” such as the series of red disks that line some of Castillo’s walls, may simply have been intended to mark a trail to be followed through the cave.

Representations of human hands in positive or negative are another well-known class of Paleolithic depictions. The fact that some of them, especially in French sites, appear to be mutilated, has brought them considerable attention. Janssens, for example, dedicated an article (1957) to the medical implications of these apparent mu-

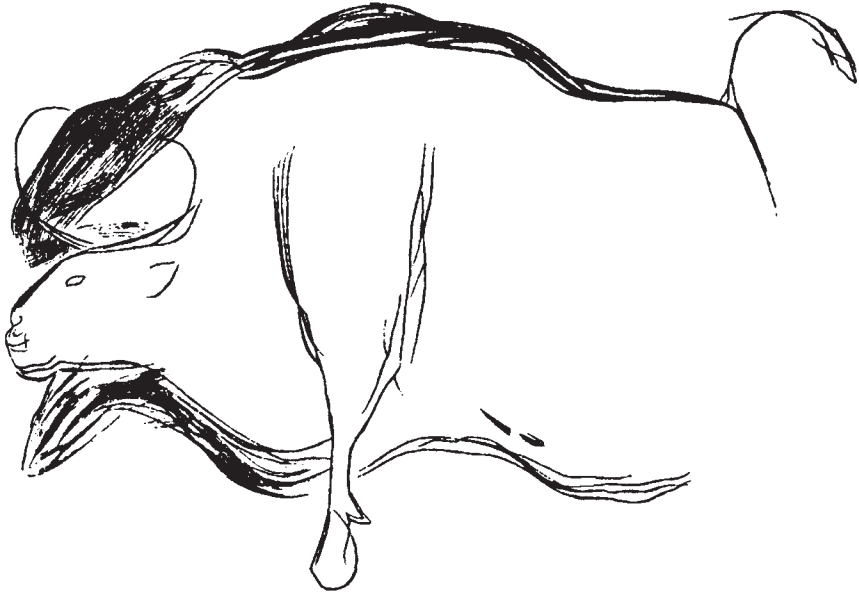
tilations. In other respects, they have received less attention than they should have. These figures must be carefully measured to determine the range and modes of variations in size. This can potentially tell us if the hands represented are the same in size as those of modern adults, or if they may include tracings of the hands of children as well. Bimodality in the size distribution of depictions of adult hands can show that hands of both sexes are represented. A still unpublished master's thesis taking these factors into account was completed by Christina Peterson in 1984. Her study of the sizes and finger proportions of hand depictions at Castillo suggests that there, while hands of both sexes are represented, women's hands predominate.

Depictions of humans ("anthropomorphs") are not common in any Paleolithic art, and when they exist, such figures are stylistically deformed in most cases. Sometimes, as in the case of the recently discovered figures from the Cueva de Hoz, they seem to wear clothing or more unusual accessories, whose study might tell us a great deal about dress and adornment. The distortion of facial features in the majority of cases (perhaps the portable plaques from la Marche are the least distorted examples) makes the anthropomorphs practically useless for studies of physical morphology.

Depictions of other animals are the most frequent representations drawn from the external world. Paleolithic artists were keen observers of nature, and sensitive to details of animal morphology and behavior whose significance often escapes the city-bred archeologists who have had little opportunity and less need to reflect on wild creatures and their ways.

The correct interpretation of animal figures in cave art was probably an easy, almost automatic task to any prehistoric hunter. For a prehistorian raised in an urban setting, attaining the necessary expertise demands much hard work, including a great deal of reading, and, even more important, as much knowledge gleaned by firsthand observation of the species represented as can be acquired. Most prehistorians, even those most interested in cave art, lack that preparation, and consequently their analyses are sometimes surprisingly naïve. I don't pretend that my own command of such subject matter is perfect—far from it. However, in some cases, even a little learning combined with careful observation and common sense can lead one to see where previous studies are lacking, and how those failures should be remedied in future.

The first and most fundamental problem presented by the study of animal figures is the correct identification of the exact species represented. A biological species is a genetic isolate. Its members cannot, will not, or, in their wild state, would never have the opportunity to interbreed with the members of any other species. For obvious reasons, this definition is not directly useful in studying cave paintings, but fortunately, as a consequence of genetic isolation, each species in a region is distinctive, and has a number of definite discrete morphological characteristics that, in the case of the large mammals of Europe, serve to differentiate it, even when examination is relatively cursory, from any other contemporary species in the same region. However, unless species-specific characteristics are unequivocally indicated in a representation, specific identification of the intended animal is impossible. No



■ FIGURE 11.1. Figure from San Román (bison) identified as a musk ox by Gómez Tabanera (after Hernández Pacheco 1919)

identification that is not based on those characteristics is reliable. Identifications of unusual species that are rarely represented, that are not based on unequivocal morphological characters, are especially suspect.

From time to time, animals that must have been rare in northern Spain, or whose presence in the region is surprising, have been identified from drawings, paintings, or engravings in Cantabrian caves. For example, Gómez Tabanera (1975: 28–29) has claimed that an engraved figure (Fig. 11.1) from San Román de Candamo in Asturias represents a musk ox (*Ovibos moschatus*). Debate about the accuracy of this identification still goes on, but is quite unnecessary. These animals are at present restricted in range to the arctic tundra. A few undeniable depictions of the species have been found in French sites, as have occasional bones of these creatures. No musk ox bones have been reported from any Cantabrian Paleolithic site. Therefore, any representation of a musk ox in Cantabrian cave art would be unexpected. We should insist that the engraving clearly show the species diagnostics of musk oxen before accepting the identification.

According to Van den Brink and Barruel (1971: 172–73, plate 20), the diagnostic characters of *Ovibos moschatus* include a peculiar form of flattened horns that sweep downwards across the forehead toward the eyes, then turn up and to the sides for a short distance, ending in sharp points. The animals' long, shaggy coats hang to their feet. The carpal region of the forelegs is very thick. Not one of those features is present in the San Román engraving, which shows the typical body build, head and

horn shape, limb structure, and beard of the European bison, a species frequently represented by drawings and skeletal remains in Cantabrian Paleolithic sites. There is no reason for continued debate about the San Román engraving. An identification of the figure as a musk ox cannot be reasonably sustained.

It is important to note that most species undergo characteristic seasonal changes in hair distribution, coat color and thickness, and body weight. Among deer, the males (in reindeer, the females as well) seasonally shed and regrow their antlers. The analyst must recognize these seasonal growth characteristics as such so that they will not be overvalued in classification.

Seasonal changes in coat pattern of *Equus caballus przewalskii* were studied by V. Mazak (1961), who provides a series of drawings that are most useful in analyzing depictions of horses. Similar work should be done for other species. Judging from Mazak's data, it seems probable that several of the horses from Lascaux, le Portel, Niaux, and las Monedas are shedding their coats. Ignacio Barandiarán gives a useful summary of this and other studies of coat pattern in an article published in *Santander Symposium* (1972).

Some authors have attempted to carry the study of depictions of animals in Paleolithic art still further, proposing subspecific distinctions based on the color and shape of markings on the coat (Blanchard 1964). This is particularly problematic. Since subspecies are only partial genetic isolates, there is always enough intergradation between different subspecies of a single species to make their consistent differentiation impossible among living animals. The only possible approach to the differentiation of subspecies from representations must use significant biometric measurements, although in many sites what are probably stylistic conventions may easily be confused with important similarities and differences of this sort, leading to erroneous conclusions. Madariaga (1963, 1969) and, later, Lión (1971) have applied biometry to Paleolithic art with suggestive results. When specific identifications are attempted, it is even more important that one not confuse stylistic conventions, or depictions of transitory and seasonal changes in pelage, or characteristics that more properly indicate the age, sex, or condition of the animals, with attributes that may have real genetic significance.

There can be no archeology worthy of the name without systematic and exact classification. The same may be said of the study of Paleolithic art. In the absence of correct classification, the study of animal figures in cave art can lead nowhere. The exact identification of the animals represented (to the level of the species wherever possible), and the correct recognition of characteristics that reflect differences in age and sex, are the indispensable prelude to any further study.

■ PALEOLITHIC ART AS A REFLECTION OF THE EXTERNAL WORLD: LEVELS OF STUDY

The study of animal figures on cave walls can be pursued with varying degrees of thoroughness. It must be understood that I believe that such studies should always be carried out as thoroughly as possible, within the limits of the time and human

resources available. But some concrete information of value can be obtained even at the more superficial levels. A rapid and provisional pilot study may prove highly informative.

Such studies are those undertaken within the first weeks of the discovery of a decorated cave, as a prelude or accompaniment to a more thorough and detailed investigation. The simplest such study is the identification of all the recognizable animals depicted and the production of a list of all the species represented. It may be possible to get an idea of the approximate age of the depictions from the extinct species that appear on the list. Since different species have different habitat preferences or climatic tolerances, such a list more often provides some information about the general climatic conditions that obtained when the figures were produced. Of course, a list that indiscriminately includes figures produced at different times can easily be misleading, but paradoxical inclusions often betray such mixed assemblages.

The next higher level of analysis is the separation of the animals into different groups, based on their different location in the cave, and the techniques used to realize them. In one gallery, all the engraved animals might be reindeer, and all the painted animals red deer. These two species prefer different habitats. From such information, it should at least sometimes be possible to conclude that the figures produced by one technique or found in one particular part of the cave indicate different environmental conditions than those in another medium or gallery. Where the differences indicate a marked climatic change, it suggests that a long time elapsed between the production of one series and another. When such information can be combined with the study of superimposed figures, it may be possible to derive a relative chronology of groups, techniques, or styles. This sort of study occupied a great part of the time of the Abbé Henri Breuil and other pioneers in the investigation of Paleolithic art.

In some cases, it will be possible to recognize true compositions, made up of several animals. Although it is frequently said that there are no true multifigure compositions in Paleolithic art, that affirmation does not correspond to the facts, as is abundantly proven by the recent work of Moure, Apellániz, and many others including the author. A composition is, after all, only an intentional, systematic arrangement of figures with respect to each other or to some preferential orientation or external feature. As examples of some less familiar kinds of composition, we may cite parallel alignments of figures, their disposition circumferential to or peripheral to the decorated space, and "organic" arrangements in which some particular figure or characteristic of the decoration (for example, the drawing of a river, or canal, or a central figure) is the focal point to which all the other depictions are oriented. A particular arrangement may be repeated several times in a larger composition. The figures may also be placed with reference to natural irregularities on the decorated surface. Although a disposition of this kind is partially constrained by the nature of the surface, there is no doubt that in many cases, such as that of the animals on the ceiling of the Great Hall at Altamira, the figures form a composition in the fullest sense of the word. There are also other more familiar compositions, eminently realistic in character, that reflect scenes taken from observations of the real-life be-

havior of the animals. That is the case, for example, for the well-known line of hinds at Covalanas, all following their leader, who is herself depicted looking backward vigilantly past the file.

Once different compositions have been recognized, the figures in each should be compared in the same way as the figures executed in different techniques, or found in different galleries. In addition to all the information mentioned above, these comparisons may reflect the different kinds of habitats represented in a region, social groupings of animals, and significant interspecific associations. Such associations have been given special attention by A. Leroi-Gourhan (1965, 1971) and A. Laming-Empeire (1972) among others.

At this point, it is essential for the analyst to know that certain species frequently travel together in the wild. An obvious example is the association of predators and prey. It is quite usual to find a pack of wolves on the outskirts of a herd of bison or reindeer, and their presence does not panic the herd or disturb it in any way (Haines 1960: 24); on the other hand, there are also associations between herbivores. Roe deer and red deer are "companionate" species one often sees in each other's company (Laurent 1974: 29, 47; Sire 1968: 152). Other species are mutually repulsive, each fleeing the presence of the other: examples are bison and aurochs, or chamois and reindeer (Hediger 1964: 152). The coincidence of mutually intolerant species in a composition is probably more a symbolic statement than a reflection of an ordinary real-life situation.

■ DRAWING ON EXTERNAL DATA: ETHOLOGY, ECOLOGY, OCCUPATION RESIDUES

As study progresses, more elaborate questions are asked of quantified data. First, from counts of figures, one calculates the proportional abundance of individuals from different species in each locality, medium, style, or composition (or in the site as a whole if there is some guarantee that all its figures were produced at the same time or over a very short period). The results are some indication of the relative importance of the different species in the minds of the artists, though they themselves were under no obligation to reproduce faithfully the real environmental situation in the vicinity of the site. On the other hand, the possibility that the artist might have given a reasonably accurate indication of the representation of species and habitats in the neighborhood of the site is worth investigation. This approach has been taken by González Echegaray in his study of the cave of las Chimeneas (1963, 1974) and by Ripoll in the cave of las Monedas (1972). I personally find these attempts among the most interesting and promising modern studies of Paleolithic art.

In some cases (perhaps more than one thinks), remains of probably contemporary human occupations have been found in near proximity to representations in a decorated site. That is the case at Altamira, Lascaux, Pair-non-Pair, Marsoulas, Ekain, and Hornos de la Peña, to mention only a few sites. Tito Bustillo, excavated by Moure Romanillo, is another very clear case. The comparison of pooled graphic data from many decorated caves with pooled excavated data from many other Upper

Paleolithic sites suggested to Leroi-Gourhan and others that the animals of most economic importance to prehistoric humans are not the species most frequently depicted. However, his conclusions in this respect are debatable. The samples he used are not comparable (occupation horizons have not been recognized in most painted caves) and the data should not have been pooled, but rather compared on a site-by-site basis, using only those caves with both well-excavated faunal assemblages and animal figures. The studies of excavated data Leroi-Gourhan used are mostly early and unreliable. Where occupation layers were recognized in painted caves, they were sometimes summarily studied, well before prehistorians were aware of the importance of a careful examination of faunal remains. What is more, the art was “dated” stylistically, and one suspects that often occupations that were really contemporary with them were dismissed as irrelevant because they were wrongly considered to be older or younger than the decorations on totally *a priori* grounds.

A careful comparison of decorations and excavated data from the same sites is always called for. There might be sufficient evidence in contemporary occupation residues to permit a relatively detailed reconstruction of the local environment and to identify the habitats or fauna locally available as resources. To mention just one recent study, our excavations of the Magdalenian level at Altamira already suggest that there may not be much difference between the proportions of different large mammals shown in the depictions and represented by excavated food debris, after all. The comparison of excavated faunas with animal depictions from the same site should indicate whether there is any marked difference in proportions of species represented and those expectable in the neighborhood of the cave; marked discrepancies might indicate that the artists were disproportionately concerned with particular animals, a factor that becomes even more noteworthy when the animals in question are either rare or only found at a great distance.

Such a study might additionally provide an approximate idea of the “mental maps” of those species and habitats that were important in the symbolic domain of artistic thought. While geographers have dedicated a good deal of time to the study of mental maps (Gould and White 1974; Ittelson 1973), as far as I am aware, investigators of prehistoric art have paid them all too little attention to date. Keeping in mind that every species is not equally easy to hunt with the same technological equipment and the same interpersonal organization of the hunters, the disproportionate representation of particular species might also provide important information on hunting methods and the organization of hunting groups. However, this kind of information is more readily obtained at the next stage of analysis.

I mentioned earlier that certain morphological characteristics of animals change over time according to regular and often cyclic patterns. For example, in summer, the pelage of the red deer stag is reddish, with spots over the flanks. In winter, it becomes gray, long, and shaggy. In September and October, the males have a mass of long hair extending from the neck to the chest. Hinds are antlerless, but males have deciduous antlers that begin to grow in spring. By the end of June or early July the antlers are completely developed, but still covered by a layer of very sensitive skin, permeated by a multitude of blood vessels—the so-called velvet. By end of

July or early August, the velvet peels away from the antlers, at first hanging in rags over the face, then finally falling away completely. The antlers themselves are shed in February or March and the cycle begins anew. Naturally, a careful examination of depictions of red deer stags might reveal some of those characteristics, permitting a precise estimate of the season of the year depicted. Analogous changes take place in other species at different times of the year, and for that reason a detailed study of such features for all depicted species can potentially provide a sort of “calendar” of the seasons chosen for representation in a site, gallery, style, or composition. González Echegaray (1974) and Barandiarán have used this kind of information in the study of Paleolithic art.

While the spectrum of movements and postures among humans is very broad and culturally conditioned, that is not the case for other animals. Each other species has a relatively limited range of “instinctive” customary behaviors and body attitudes that indicate the physical and emotional state of the animals. Those postures are absolutely characteristic of a particular species, and are shared by all its normal members. Among the most noticeable of these are the behavior patterns that accompany the breeding season. It seems inevitable that these stereotypical postures must frequently have caught the particular attention of the Paleolithic artists. That is in fact borne out by the depictions. The reasons are not far to seek. In the first place, such careful observers of nature were probably well aware from the behavior of the animals that certain stereotyped behaviors were related to reproductive activity, and it seems likely that they were able to make the connection between breeding and the “regeneration” of hunted resources. In the second place, during the rut, many animals lose their usual shyness and become highly visible in their surroundings. Wherever possible, red deer, for example, normally retire after an early morning feeding to the shade and shelter of heavy brush or dense woods, where they remain resting and inactive (and thus relatively well hidden) during most of the day. Aside from sporadic gentle and unobtrusive calls of hinds or the low bleating of young fawns, deer remain as silent as possible. However, during the September/October rutting season, the males engage in a ceaseless roundup of hinds for their harems. As part of their search, at all hours of the day or night, they emit a jarring and characteristic call (“belling”). Often, a stag lying at rest in the heat of the day will bell repeatedly without getting up. When the animal is up and about, he assumes a characteristic posture as he bells, with neck outstretched, head lifted, and wide open mouth (Laurent 1974: 10–20; Hainard 1949: 132–36).

This characteristic posture can be observed in Paleolithic representations, such as that of a stag from San Román (Fig. 11.2). More often than not, it is erroneously interpreted as a wounded stag bellowing in his death agony. Contrary to widespread opinion, wounded deer are usually silent. While an exhausted deer may pant hoarsely, and in flight may break underbrush audibly, neither I nor any hunters of my acquaintance have ever heard a wounded stag cry out or bellow. When such depictions are seen in Paleolithic art, then, what is represented is an animal in the thrall of passion, not of death. Where, as happens at San Román and elsewhere, the depiction seems to show a spear transfixing the animal, the artist probably intended



■ FIGURE 11.2. *Belling stag from San Román (after Hernández Pacheco 1919)*

to suggest that its belling has attracted a hunter to a careless prey, rather than that the hunter's lance has caused an agonized cry.

And that is another reason why the rutting season is a period of particular importance to hunters. In all mammalian species, including our own, males in pursuit of females lose whatever reserve and common sense they have, and behave like idiots in the heat of lust. During the rut, red deer will allow hunters to approach them much more closely than at other times, and that makes them far easier to kill (Hainard 1949: 134; Laurent 1974: 13). Many other mammals also lose their fear of natural enemies during the breeding season.

The information derived from a study of the condition and attitudes of animal figures can thus tell us a great deal about the season of the year that the artist wished to depict. Obviously, that need not have been the season during which the site was occupied and painted, since the painter could have worked from memory. But, if all the animal representations in a site are indicative of the same season of the year, the hypothesis that the paintings were produced during that season naturally suggests itself as a plausible explanation. However, other evidence, such as the nature and location of the site (whether on the coast or in high mountains, etc.), or the nature of

sediments, flora, and fauna from contemporary levels of human occupation debris in the site, is needed before the hypothesis may be evaluated.

In the next chapters, I shall show how a careful reexamination of the polychrome figures on the great ceiling at Altamira, along the lines indicated above, revealed aspects of their meaning that had not previously been suspected—that in fact almost all the figures depict a herd of bison during the rutting season. This theme is pervasive at Altamira, recurring in figures executed in different styles and techniques, and occurring in other galleries. Its repetition suggests that seasonality and reproduction were a focal concern of the artists, and that in turn leads us to consider the possibility that the human occupation of Altamira may have been seasonal, or even that Altamira might have been the scene of seasonal and cyclic rites related to the reproduction of primary natural resources that would have taken place at the end of the summer.

To strengthen such an interpretation would require as a first step that new data bearing on environmental conditions during the utilization of the site be gathered from the occupation layers. While excavated data might tend to make the interpretation more plausible, I cannot pretend that it would “test” a hypothesis about site function and seasonality. Even if the excavated environmental data gave a perfect indication that the site was inhabited at another season, or the whole year round, it would be possible to argue that the decorations were only produced during the late summer. Nonetheless, when it became possible to excavate the Altamira Magdalenian level in the early 1980s, we were especially attentive to the potentially informative relationship of “environmental” data from the midden to the decorations in the cave. At the moment, some of the evidence from the excavation has been analyzed, and tends to make the inference that the decorations depict the principal period of accumulation of the Magdalenian occupation debris somewhat more credible.

This brief resume and the studies to follow will, I hope, persuade the reader of the importance of a careful and minute study of decorated sites and the depictions preserved in them. From the study of art as a mirror of the external world, it is possible to gather information that is absolutely fundamental to any attempt to reconstruct the lifeways of our prehistoric forebears, and their adaptations to the environments in which they played out their lives. My choice of Altamira as an exemplary site for restudy was not fortuitous, though I did not foresee that I would actually be invited to participate so intensively in that restudy when it took place. Altamira quickly became the major thrust of my own research in Paleolithic art for three reasons: first, I believe that it is still in immediate danger of destruction; second, it was the first decorated Paleolithic cave recognized as such, and for that reason as well as the intrinsic quality of its art it is still one of the most famous Paleolithic caves, so that news of a demonstration of valuable new approaches to the study of its art would spread quickly to the profession and the concerned public; third, familiarity with parts of the site already indicated that there was a great deal that could still be learned. It made an ideal setting for the demonstration of just how much remains to be understood about even the most famous, best known, and most “completely” studied Paleolithic decorated caves. The fact that an Altamira, which has already

been subjected to detailed investigation by generations of the finest and most expert minds in the field, can still yield so much when approached with open eyes and new questions, means that almost everything about Paleolithic art is still to be learned. We have discovered a previously obscured avenue to the study of the lifeways of the past, and found it to be wide and promising.

■ ART AS A REFLECTION OF AN INTERNAL REALITY

Nevertheless, this is not the only avenue that we must explore. Studied simply as a mirror on the artist's surroundings, Paleolithic art could only provide a distorted glimpse of the past. It is also important that we examine the art as a reflection of the "interior" world of the artist's own psyche, and that we try to find in art's mirror glimpses of the society to which the artist belonged.

I must begin that examination with some brief remarks about the nature of the organization of the human brain, and the psychological processes involved in perception, as far as I am able to understand them (and I must admit that my knowledge in these fields is neither profound nor original). As prehistorians, we are most familiar with the concrete evidence for past human behavior that has survived in archeological deposits, or in the form of the depictions on the walls of caves and shelters. We are more than a little uneasy when faced with the speculations of those psychologists who deal with less tangible phenomena. We feel, with some justification, that any valid interpretation of Paleolithic art must always be based on solid empirical evidence of the artistic process: the paintings, engravings, reliefs, and sculptures realized by prehistoric artists. Those are the only primary documents available to us, and we cannot permit ourselves to engage in speculations that stray far from them. Nevertheless, it is clear that there are other relevant data at our disposition, proceeding from the study of the human psyche, and we are not only well advised but obliged to take these data into consideration to the degree possible and consonant with our aims and responsibilities as scientists.

It might seem at first glance that the human conscious is a nebulous, almost mystical field whose study is uncertain and has little direct bearing on the understanding of Paleolithic art. But that is not entirely true. Aspects of this study are as empirically solid and trustworthy as the cave paintings themselves. In fact, the findings of psychology actually set certain limits to our interpretation of the figures, and restrain the uncontrolled flights of pure fantasy that at times have characterized the work of some interpreters of Paleolithic art. Psychological data derived from living people and other animals illuminate subjects as different as the relationship between art and language, the origins of abstract art (as we have already observed), and many other dimensions of the meaning of cave art.

In agreement with other paleoanthropologists, I think that the authors of practically all cave art belonged as we do to the biological subspecies *Homo sapiens sapiens*. This is the subspecies to which all of the fossil humans of the latest Pleistocene should be referred, although they have commonly been assigned to different "races," with names such as Cro-Magnon, Chancelade, Combe-Capelle, Grimaldi, etc. There is no

reason to believe that the nervous systems and cerebral organization of these humans were significantly different from our own (see, for example, the work of Kochetkova [1978 and elsewhere]). Consequently, any attempt to understand Paleolithic art from a psychological viewpoint may and should use data obtained from the study of modern humans.

■ THE PROCESS OF PERCEPTION AND GEOMETRIC FORMS IN ART

There are many excellent studies of visual perception among vertebrates. The retina of the primate (including human) eye contains very large numbers of light-sensitive cells, whose function is the reception and “classification” of visual data. It seems that this retinal classification is quite simple, inasmuch as different groups of cells only respond to differences of light intensity oriented in specific ways or contrasts that move in particular directions across the visual field. Some cells only discharge their electrical impulses when presented with a vertical contrast, others are sensitive to horizontal gradations of light intensity, others to moving contrasts that form right angles, and so on. While a particular kind of contrast stimulates one group of receptors, at the same time it inhibits the activity of other groups. The total number of different kinds of photoreceptors seems to be relatively limited (Gregory 1966; Rose 1976).

The information organized by the retinal photoreceptors is transmitted to specific zones in the cerebral cortex, where cells responsible for the processing of information from determined parts of the retina are all situated in columns perpendicular to the cortical surface. It seems to be the case that within any column, there are vertically arranged sets of cells all of which are responsible for the analysis of data of the same kind, with neighboring sets processing different kinds of data. So, stimulating adjacent groups of columns by whatever means produces the visual impression of patches of flashing spots, sometimes arranged in a geometric network, sometimes in movement, situated in a particular part of the visual field. Even without the intervention of the eyes, these effects will be produced. This is the explanation of visual patterns, called “phosphenes,” produced during attacks of migraine, or fever, or by overindulgence in alcohol, ingestion of drugs, or blows to the head. Pressure on the eyeball may also produce apparent “visions” of spots and patterns of light (Fischer 1975; Harner 1973; Horowitz 1975; Klüver 1966).

Since the retinal photoreceptors lie at the back of the eye, the light that stimulated them must first travel the distance that separates the crystalline from the receptors. In this intervening space, there is an important network of blood vessels, and the intraocular space is also filled with fluid that often contains local concentrations of material of greater density than the surrounding medium. The outer surface of the eyeball is constantly bombarded with tiny dust particles while the eyes are open, and must be continually bathed by lachrymal fluid to prevent dust buildup and irritation. Normally, we are not aware of the existence of dust particles, “floaters,” or the network of blood vessels in the eye, but under certain light conditions they may become visible and bothersome or frightening.

These observations have considerable relevance to the study of art. I have previously said that art is representation, and that sometimes what is represented is the external environment—the world outside the artist. Sometimes, however, what is represented is inside the artist.

Some authors have claimed that such geometric forms as “tectiforms,” “scuti-forms,” and “claviforms,” that appear with some frequency in Paleolithic representations, have no counterpart in nature. For that reason (they go on) such depictions must necessarily represent artifacts made by prehistoric people, such as traps, huts, boats, or other built objects. It seems to me that we need not necessarily seek models for these representations outside the eye and brain of the biological organism that produced them. Complex and regular geometric forms do exist in nature, and can appear to us in our dreams, or even when we are wide awake, sometimes in striking fashion, provoked by stimuli that are always with us. I don’t mean to suggest that all geometric figures in cave art need necessarily have this origin—such a suggestion would be an abuse of the evidence—but I do insist that the *a priori* opinion of some authors that all such images are modeled on real artificial structures familiar to the artist is just as dubious, and is especially dangerous if it leads to analyses of postulated “cultural features” or if it comes associated with the idea that geometric art represents a developmental stage later than “realistic” representations of animals.

■ LATERALIZATION, ART, AND LANGUAGE

Another fundamental question concerns the relationship between Paleolithic art and language. Some specialists have always tended to see the two as related. Marshack (1976) is one of the authors who has dwelt on such an interpretation, suggesting that certain kinds of depictions reflect the beginnings of articulate speech. If that were so, it would be extremely important, since there is no other direct evidence of spoken language before true writing appears. Other authorities have proposed that Paleolithic art might be the root of written language, and consequently suggest that the designation “proto-writing” be given to some Paleolithic signs. Of course, one has to admit that the Paleolithic figures are graphic symbols that were almost certainly intended to convey some message, and in that sense they are a fruitful field for the student of semiotics and communication, but not all graphic symbols that convey a message are linguistic, and not all are properly called writing. Silent films, cartoon strips, or animated drawings may “tell” a story in an understandable way even without legends or titles, by imitating actions or figures that are more or less recognizable, but that doesn’t qualify them to be called “language” in any but an analogical, figurative sense. Not all communication is language.

Surprisingly, evidence from the field of psychology can be brought to bear on this question. As far as we know today, all higher mental functions are under cerebral control (Luria 1966). Complex human thought processes are related to the cerebral cortex. The cerebrum is composed of two semi-independent hemispheres that communicate by means of the *corpus callosum*. As the very small child learns to think and to speak, one of the hemispheres gradually comes to dominate its rational

processes and complex body movements. In normal right-handed people, it is the left hemisphere that becomes the more dominant, while in normal left-handed people, the reverse occurs. If the dominant hemisphere receives some damage, as long as the child is still very young, the damage may be compensated for by a transference of function to the other hemisphere, but if this occurs at a more advanced stage of development, the transference of function does not take place. If the *corpus callosum* is divided, communication between the two cerebral hemispheres cannot take place directly, though the individual may continue to behave in many other respects in a normal way, speaking, reasoning logically, and following a relatively ordinary way of life. Observations made of such “split-brained” individuals, of patients with lesions in one hemisphere, and some experimental data from normal subjects show that, in the immense majority of people, the dominant hemisphere is the locus of verbal communication; of analytic, logical, and grammatical functions; of mathematical ability; and of phoneme comprehension. This hemisphere analyzes discrete input sequentially, processing it in linear fashion (Ornstein 1972).

Apparently, rational, sequential processes function in a framework of binary digital logic, based on all-or-none signals along neuronal chains. Binary digital logic is that used in digital computers, and seems to underlie all language and mathematical calculation (Fisher 1975). All our conscious mental processes governing intentional actions are under the domination of the system of verbal communication and the dominant hemisphere, that may be called our “conscious, logical” brain.

The other, non-dominant hemisphere, normally the right in right-handed people, is our “unconscious, analogical” brain. It evidently plays little part in the organization of logico-grammatical structures and processes, but it is capable of a diffuse differentiation of some words and a general appreciation of their sense. Its functional organization is less differentiated. It is specialized for holistic, synchronous mentation, for the simultaneous processing of visual and relational data appreciated all at once, and for information about the state and orientation of the body in space (Luria 1973: 160–68). It recognizes familiar faces or objects, and their characteristics, but not their names. It is precisely in the non-dominant hemisphere that artistic production is localized. Musical, artistic, and craft ability can be lost if it is damaged severely, although the incapacitated individual can still speak and reason if the dominant hemisphere remains intact (Ornstein 1972). The non-dominant hemisphere discriminates tones and rhythms, but does not “hear” phonemes (Luria 1966). It is concerned with nonverbal information processing, with visuospatial *Gestalten* and fields, with multivalent metaphor, with what the Freudians call primary process, and with intuition (Fischer 1975; Dimond and Beaumont 1974; Geschwind 1974).

Split-brain individuals who are right-handed can draw, copy spatial constructions and complex two-dimensional geometric figures, using color to make pleasing and regular patterns, but only with their left hands. They are incapable of performing the same tasks using their right hands. However, they maintain their ability to write with their right hands (Ornstein 1972).

Similarly interesting observations could be multiplied, but there is no reason to go on. The point is already sufficiently clear. Visual, pictorial representation is

the function of one hemisphere, while logic, grammar, speech, connected narrative, and serially structured information such as is required for the production of history or myth are the function of the other. These observations have the profoundest implications for the interpretation of Paleolithic art. They imply, first of all, that we cannot expect the study of Paleolithic art to provide direct evidence of the origins and development of spoken language. On the other hand, that study does provide indirect evidence as valuable as it was unforeseen.

It is now known that mutual interference between the two cerebral hemispheres is as marked a hindrance to artistic ability as it is to the acquisition and reproduction of language. It is therefore almost inconceivable that works of art as masterful and well defined as are some of the animal figures in even the earliest Paleolithic art could have been produced before cerebral lateralization had developed to a state where separation of function of the two hemispheres had become as marked as it is among living humans. When the first art was produced in the caves, cerebral lateralization must already have been so advanced as to permit the use of spoken languages as complex as those that exist in our day.

ART AND WRITING: EVIDENCE FROM LINGUISTICS

Of course, the fact that Paleolithic art cannot be taken as evidence of a primitive stage in the evolution of spoken language does not necessarily mean that it could not provide evidence about the origins of writing. In at least some of the earliest writing systems, representational signs were certainly used (although popular ideas about the evolution of specific alphabetic signs from pictographs of particular objects are as often based on misinformed *a posteriori* rationalizations as on chains of reliable historical evidence). Alonso del Real (1974) is among those who have referred to some enigmatic signs as though they were “proto-writing.” If this appellation is ever justified, it is only in a very few special cases (possibly, for example, the small groups of points and lines that are found repeated in association with some of the animal figures at Lascaux; they do seem very like some individual “i.d.’s” or personal insignias known from ethnography). But even in these cases, it is doubtful that we should call the marks “proto-writing.”

What I have just called “personal insignias” are certainly signs, and might have served as a stimulus for the idea of using graphic signs to represent words. However, identity markers of this sort are a peculiar category of linguistic signs, without much relationship to the signs used in normal text. If, as seems possible, the Lascaux signs are personal expressions of the identity of the artists, attached, perhaps with some pride, to their aesthetic productions, there is more “affect” than “logic” in the semiological load they bear (Guiraud 1975), and that sets them apart from other forms of writing, just as the great affective load of family nicknames or pet names sets them off from other words in speech. With the sole possible exception of these signs that may be personal insignias, where the great majority of Paleolithic depictions is concerned—for the animal figures, tectiforms, and other geometrics, etc.—the interpretation of Alonso del Real is probably without justification.

Linguistic theory has something to say on this subject. Writing properly so called—that is, language writing—is a secondary graphic means of communication derivative from and modeled on already well-developed systems of articulate speech. Since its organization and structure are based on systems of articulate speech that are already completely evolved, the term “proto”-writing cannot properly be employed. While written language may take many forms, as different as syllabic writing, or alphabetic, or ideographic writing, and despite the fact that any particular written language changes through time, none of these manifestations can in any sense be called proto-writing. Language writing either exists full-blown or it doesn’t exist at all.

All linguistic writing systems share certain characteristics. In the first place, every script consists of a limited (though sometimes large) set of ultimate, discrete, and clearly patterned components. Second, those components are systematically arranged into recurring hierarchical structures, in compulsory patterns, so that in any specified context, certain signs must always precede or follow others. In art, on the other hand, as Roman Jakobson has pointed out (1964), there are no ultimate discrete components, and even where hierarchical structure occurs in an artist’s work, it is neither compulsory for the rest of that artist’s contemporaries nor is it necessarily systematic. With the exception of the pieces called “tally-marked” objects, where evidence of patterned notation (counting) does exist (even though Marshack is certainly wrong in identifying so many of them as lunar calendars), Paleolithic signs do not satisfy this criterion. There is no systematic, compulsory arrangement of small, indivisible, and easily recognizable units into larger, recurrent, hierarchical wholes. Consequently, for the moment it seems that the overwhelming majority of Paleolithic signs must be regarded as art, not writing.

The late Annette Laming-Emperaire proposed another interpretation of art as narrative (1972). She postulated that individual figures may be mnemonic devices recalling elements of the mythic history of the group. Some such narrative significance, at least in embryonic form, is virtually implicit in Laming’s and Leroi-Gourhan’s earlier treatment of painted caves as sanctuaries containing associations of symbols whose meanings are in complementary opposition. But in that case, the “message” was always thought to be so short that it was possible to represent it integrally on a single panel, in contradistinction to the more extensive narratives Laming’s newer theory implies.

I find that the suggestion that the corpus of decorations in a cave might represent long narratives on the order of Kwakiutl myths or medieval romances is still unconvincing. It is impossible to deny that in some caves, that might be the case, and no one familiar with certain sites like Altamira can escape the impression that they are indeed sanctuaries that would have been appropriate settings for the recitation of myths of cyclic death and resurrection, or the performance of initiatory rites. Nevertheless, the methodology of Leroi-Gourhan and Laming is ultimately subjective and impressionistic, and the proof of such assertions, even in those cases that seem subjectively to be most suggestive, must derive from much more consistent, rigorous, and systematic methods than have ever yet been applied.

I admit that in some caves, such as Altamira, one can demonstrate a pervasive theme that unifies different figures, panels, or galleries. But the revised hypothesis of Laming, postulating that all the decorations in a cave may represent the detailed exposition of an extensive mythic cycle, seems especially weak to me. Where unifying themes have apparently been detected, they are manifest as redundant and forceful figurations of simpler notions of symbolic equivalence or contrast, and the complexity that one would expect from an involved narrative is simply lacking.

There is another, equally important obstacle to Laming's theory. In the case of written language, elementary symbols are apparently mentally converted to speech sounds and then processed by the dominant hemisphere. In similar contexts, the same phonemes are always represented by the same set of elementary signs. Though there is some variation between one representation of a sign and the next, enough similarity must be present so that identical sounds or concepts can be recognized when they occur. The signs have to be remembered until the message makes sense. Simplicity and replicability make it possible for a reader to keep a string of elementary language signs in memory long enough so that whatever meaningful patterns it contains can be decoded. Long written texts look very different from long pictures on this account. Students of Paleolithic art are not the only scholars who have failed to recognize this fact. The number of early abortive attempts to decipher unknown scripts, such as Egyptian hieroglyphics, that failed in part because would-be translators tried to read the pictures on monuments as well as their inscriptions, is absolutely amazing.

When a non-linguistic visual depiction is examined, its "meaning" is derived from the relatively simultaneous perception of the visual field. For the meaning of a composition to be evident to the viewer or producer, its elements must be relatively close to one another in space, and perceptible from a single viewpoint. Many of the associations discussed by Leroi-Gourhan and Laming do not conform to this requirement. That makes it unlikely that any viewer could have grasped the significance of the complementary oppositions portrayed.

There is, of course, a way to make sure that a spectator receives a unified impression from a set of discrete, spatially separate compositions. That is to link the symbols in sequence by a verbalized logical structure that has previously been committed to memory, or that is recorded in writing elsewhere. That is the technique employed, for example, with the stations of the *Via Crucis* in a Roman Catholic church. The "Way of the Cross" is a sequence of conventional graphic symbols linked to a mnemonic structure by means of the repetition of one or more of the symbols in recognizable form in several of the different panels that make up the whole. (Usually the panels or stations are also numbered in sequence, and they may have short written legends as well.) The repeated focal symbols are individualized by means of a well-defined and obvious conventional complex of invariable primary attributes that remain recognizable despite changes in position, attitude, and other secondary characteristics. Christ's beard and nimbus, the crown of thorns in the latter part of the series, and the shape of the cross are just a few of the primary characteristics found repeated in different stations.

I know of no series of figures from Paleolithic art that conforms to this description. The Paleolithic figures that would play the role of mythic symbols in Laming's interpretation look so very dissimilar that any conventions for their individualization would have had to be completely anarchic—key figures whose repeated presence could serve as reference points for the thread of narrative would have had no “recognizability”—a manifest contradiction in terms. That may be the most telling objection to Laming's thesis.

■ OTHER INTERIOR REALITIES, INDIVIDUAL AND SHARED

Until now, aside from this digression into the byways of linguistic theory, my discussion has only dealt with the characteristics of the universal, genetically inherited perceptual apparatus, and not with other, learned, aspects of conscious mental activity. Now I must enter less solid ground. Several clinical psychologists and psychiatrists have made important observations that apparently indicate a certain psychic unity in the development of individual thought processes, and these observations are in agreement with data from other fields. In fact, unless there were certain natural similarities in the mental processes of all peoples, there would be no basis for the structuralist position that Lévi-Strauss, Piaget, Leroi-Gourhan, and Laming represent, and despite my specific reservations, there is no doubt that their approach has led to more profound insights in the social sciences and in the study of Paleolithic art.

One of the greatest contributions that clinical psychology has made to our understanding of the human condition is the recognition that people see the world as they want it to be, not as it “really” is. Many philosophers before Freud, and some at present, have taken the stand that there is a real, external environment, whose characteristics may be objectively and correctly appreciated by our senses: they are unalterable truths that are always with us. For those philosophers, it is imagination that is plastic, not our surroundings or our perceptions of them. It was Freud who showed convincingly that stimuli from the external world can be ignored, avoided, or altered according to the internal demands of the unconscious. That means that our perception of the external world around us is not an immutable given, but instead is always shaped and modified by internal processes such as projection. Recognition of that truth has altered the course of scientific investigation in a myriad of ways.

One need not be a strict Freudian to appreciate the truth of this fundamental observation. The dichotomy between the external universe and the inner psyche is never complete, and in studying Paleolithic art, we must constantly remind ourselves that a naïve belief in that dichotomy would be an error. In fact, our perception of reality is the result of a complex interaction of individual conscious and unconscious frames of reference with the “real” environment. Einstein commented that in physics it is always the theory that decides what we can observe (Heisenberg 1971). Ethnographers are now aware of the extent to which the values of their own societies usually intrude, distorting their analyses of other cultures and societies (Sturtevant 1964). Historians tell us that the biases of the present affect our understanding of the past, producing what Fischer (1970: 135–144) calls the fallacies of

“Presentism” and “Tunnel History.” But these are only special cases of what Freud showed us to be a more general condition of all understanding, the basis of all intellectual endeavor. While we may and in fact must minimize these distortions in our understanding by making ourselves aware of their nature, extent, and causes, we can never eliminate them entirely.

From the foregoing, we may say with some justification that there are several distinct kinds of reality, each of which is of interest to us. First, there is the reality of physical objects, although we are never able to grasp them exactly as they are. Second, there is the shared reality of perception capacitated by our biological inheritance, which we have discussed. Every normal individual of our species perceives reality by means of a mechanism common to us all, determined by the structure of the human eye and brain, and subject to the same set of basic illusions. Third, there is the reality which has been filtered, modified, and structured by the emotions and feelings of the individual. Fourth and last, there is the reality that is interpreted and structured according to shared, learned frames of reference, such as language, myth, and religion. This kind of reality is the exclusive domain of the human species.

Only the first kind of reality is truly unstructured; there is no human observer in the equation. The moment a human observer is introduced, structure becomes essential. Without the imposition of some structure, no matter how arbitrary, by the spectator, it would be completely impossible for us to understand our surroundings, to grasp “reality” at all. Nature is not copied in the mind by perception and thought; it is instead sorted out and interpreted according to schemata, some of which are shared, while others are peculiar to the individual. It is clear that Paleolithic art is a material reflection of part of this process of structuring and sorting out of reality, and that presents us with the fascinating likelihood that the study of Paleolithic art can tell us something about those individual and group differences in reality structuring that obtained among peoples of the prehistoric past. This presentation does not provide sufficient space for me to more than allude to the potential of such studies in the most general terms.

■ ART AND TRANSITIONAL MODES OF BEHAVIOR

As is well-known, there are several theories about the purpose and function of Paleolithic art, such as that the figures were used in hunting magic, that they represent myths, that they were produced solely for the aesthetic pleasure they yielded, that they are somehow related to rites of initiation, and so on. The variety and superficial incompatibility of these suggestions prompted Peter Ucko and Andréé Rosenfeld to comment (very sensibly, in my opinion) that there is no single reason why the art was produced; there may be as many motives for its production as there were artists or decorated sites. In fact, if we were to accept the reservations of Ucko and Rosenfeld as the last word on the subject, we would have to admit that the search for the motivation of most Paleolithic art is a fruitless and futile venture.

Although I believe that their conclusions are generally correct, I also believe that the external diversity of possible motivations for artistic production may overlie

and obscure a deeper, more basic uniformity. That uniformity has apparently been detected by certain ego-psychologists who have studied the processes by which children develop to become functioning individuals and members of society. In particular, the work of Winnicott (1971 and elsewhere) and Modell (1968) is both relevant and fundamental to our inquiry.

Their observations may be summarized as follows. At birth, the individual is immersed in his environment. Despite the fact that we have learned that infants may respond at some level to stimuli while they are still in the womb, there is no convincing proof that the newborn child is aware of its distinctness from its mother. On the other hand, this immersion of the individual in the environment would be an unthinkable situation for a rational, functioning adult, and as a consequence, there must be a gradual differentiation of the self from its surroundings. For some time after birth, whatever the child needs is ordinarily provided as soon as the need is noticed, by an adequate mother. As the baby becomes gradually less helpless, more competent, and more conscious, its demands on its mother are increasingly ignored—we think of a mother who responds too readily to its whims as “spoiling” the child, and in fact there is a real danger that being overly solicitous will hinder normal development. Through this process, the child learns that it is someone other than the mother, an individual separate from an environment that it is not omnipotent to command. It is essential that the child undergo this differentiation or individuation process successfully. Eventually, the moment must come when the child cannot be protected from the environment by its parents, so it has to learn to deal with that environment independently in rational fashion.

As soon as the beloved and satisfying object—the mother—begins to withdraw her attention with some frequency from the subject, and as the child becomes more aware of the inevitable shortcomings of any parental treatment, no matter how good, feelings of rage and retaliatory destructiveness are provoked in the child. The guilt that those sensations produce causes anxiety—fear that there could come a time when mother might never respond, no matter how insistently she is called.

The anxiety that the environment might someday cease to gratify the subject, or even depart never to return, leads the child to create a private inner world more consonant with its wishes. As this happens, a part of the affect normally associated with the parents is almost always projected on a so-called transitional object. The transitional object is something real, such as a piece of cloth or a doll, but at the same time it is invested with certain qualities that come from the child's inner world, and thus, paradoxically, it is simultaneously part of the external world and part of the child's imagination. It gives the illusion of protecting the child from the dangers of the environment and it may be carried about so that it is always there and always gratifying. (Linus's security blanket in Schultz's *Peanuts* comic strip has long since become the most famous example.) In some degree it substitutes for the parents, and thus counteracts the effects of physical separation of the child from the real, gratifying, but imperfect environment. As long as the child is in possession of the transitional object, it is easier for it to tolerate an increasing separation from the parents.

As the child continues to develop, the old transitional *object* is eventually abandoned, but the transitional *mode of relating* to the outside world persists as play. In play, which may be either a solitary or a socially shared experience, children create a magical world, neither entirely within themselves nor entirely outside, where they can symbolically work through the continuing problems of psychic development. Play and creativity persist among adults, where they continue to have similar functions, but now the activities tend mostly to be shared, and the symbols employed are not developed by each individual, but are instead defined by the society.

The psychic tensions and anxieties which inevitably arise from the vicissitudes of social interaction or the unpredictability of nature are allayed to some degree by dreams, daydreams, and fantasies, and to an even greater extent by art, music, dance, literature, myth, and religion. They, too, substitute a created proximate world for a distant real environment, and in the process provide at least the illusion of participation and control as well as some amount of gratification, whether real (for the participant or performer) or substitute (for the observer), or both. Additionally, they permit the psyche to consider the world from several different perspectives, working through the consequences of alternative behavior, before initiating any real action.

It seems to me that these observations have some real, fundamental explanatory validity for the study of art of all times and places. The production or viewing of Paleolithic art would have served an important educational function, as it helped prepare individuals for action in their real environment. The representation of possible, immediate, or frequently recurring situations must have helped hunters deal with those situations as the need arose. The act of producing or viewing the representations in itself provided a degree of intrinsic satisfaction, and so helped relieve the tensions produced by a concern for the possible failure of the hunt, or by the fear that the game might fortuitously disappear. In response to anxiety that the hunt would not be successful, or that there might be too few prey to permit group survival, humans created a symbolic environment where the spatial separation between the hunters and their prey was denied. The representation of desirable but unlikely or impossible situations would have provided some gratification withheld by the real environment or only obtainable at rare intervals, or after long and arduous exertions. By this reasoning, the production and stabbing or other mutilation of animal figures (well documented at Altamira as elsewhere) would have had a measurable influence on the outcome of the hunt and the well-being of the human group, not because of any magical effect on the real prey or external environment, but because of their real ultimate effect on the hunters and their neighbors and kin. A basic function of Paleolithic art, we may say, was the reestablishment of the balance of psychic forces in the artist and the viewer.

It is also important to note that whenever groups of people made or viewed cave art together on ceremonial occasions, or as part of other rituals, the effect produced by the representations would have been heightened, while at the same time the act would have helped to strengthen the social bonds holding the group together. As group solidarity was augmented, individual anxieties allayed, and the self-confidence of hunters strengthened, Paleolithic art would have helped hunters in a reasonably

benign environment to greater success in their daily endeavor. It is because of their efficacy, not on the world of animals, but on the world of men, that such practices are self-perpetuating. The fact that art is a transitional mode of behavior provides an adequate and convincing explanation of some of its most important functions among hunting and gathering peoples. It is a more generalized explanation for the production of Paleolithic art than the specific motives examined and rejected by Ucko and Rosenfeld, but its very generality lends it strength.

■ ART AND SYMBOLICALLY STRUCTURED SPACE

Nevertheless, that is not the only valid explanation for the production of Paleolithic art. Paleolithic art undeniably had an important and far-reaching effect on the real world outside the artists and their audiences in another way. It served to organize and domesticate the cave environment.

At all times, caves have held a strange fascination for human beings, while at the same time they have filled them with unease and fear. Caves themselves are liminal zones: they are the passageways between our surface world and the abysses of the terrestrial womb. On the one hand, they offer us the benevolent protection of mother earth: in their interiors, we are sheltered from the inclemencies of weather and the fickleness of the seasons. Interior cave environments are not subject to the marked changes of temperature and precipitation that affect the surface world. They remain relatively warm at the height of the winter's cold, and cool on the hottest summer days, and their respiration fills them with gentle breezes. They may conceal enchanting formations that when lit sparkle like jewels, or glow with soft colors. They often contain crystalline pools or rivers of clearest water. I have never felt more secure and protected than I have often felt in caves. In many respects the inside of a cave is the ideal home.

But their nature has at the same time a threatening, terrifying aspect. I have never felt more helpless, alone, and unprotected than I have sometimes felt in caves. The darkness of their depths is impenetrable, indescribable, unimaginable. A cavern may be filled with hidden dangers—bottomless holes and fatal precipices. Their bowels were frequently the lairs of fierce carnivores, the lion and bear, sworn enemies of hunting groups from memoryless time, and the treacherous hyena, who robs by stealth but will turn and kill when he has the advantage. The descent into an unexplored cavern is a completely unnatural act for humans, beings from the sunlit surface. Once we have emerged into the light at birth, to enter a cave is symbolically to submit to reabsorption. As psychologists of the Jungian school hasten to remind us, caves are the grave in the mother's belly (Jung 1965: 125–38; 1964: 56; Neumann 1972: 45); they are wombs that seek to devour us.

But that grave is also the gateway between the world of the light of the conscious and the unknown underworld of darkness. To enter it is symbolically equivalent to a descent to the lowest depths of the subconscious, to explore the intimate secrets of our own individuality, to search for new syntheses of our personalities, always with the possibility that we may be reborn illuminated and triumphant. In that

subconscious allegory resides the attraction of caverns for the majority of people of all times and places.

The interior of a cave seems intolerably strange and disorganized to the human surface dweller. On entering an unfamiliar cave, we find ourselves at first wholly disoriented. Its galleries seem unbelievably twisted, and broken by completely irregular rock formations—columns, fissures, cascades, stalagmites, and stalactites—that when seen from different directions take on totally dissimilar aspects. Consequently, the impression produced is one of absolute chaos—a situation so far removed from the culturally ordered world of our everyday lives that we find it almost unimaginable; we feel thoroughly lost in it.

Ordinary humans cannot long endure such disorganized space. Before it can be put to human use, we must first make it orderly, and give it “sense.” Paleolithic people accomplished this with their art. Representations of animals, geometric figures, hands, splotches of color, and engravings on the walls and ceilings miraculously transformed the chaotic natural environment into symbolically structured space—a cultural environment or “symbol-milieu” that to a much more comfortable degree domesticated and humanized the cave interior.

The principles of organization of the new cultural environment would have reflected the ideology of the group, its beliefs about its place in the natural and supernatural worlds, and the conscious and unconscious concerns of its members. In places that served as sanctuaries, the symbolic load would have been even more concentrated and better differentiated than in mere domestic space, but both would necessarily have reflected the same basic principles (Eliade 1957).

■ SOCIAL DIMENSIONS OF SYMBOL CONSTRUCTION

If, as I have said, Paleolithic art reflects the cultural beliefs and values of the societies that produced the artists, we may hope to find there a reflection of some of the broad outlines of the classificatory principles and patterns of symbolic structure they conventionally employed. Since no two societies use exactly the same range of structural principles in identical ways, artists from different groups must have patterned their symbolic representations in ways that can betray their group affiliation. When we are able to identify the underlying principles of symbol construction in a corpus of parietal art, and couple their analysis with a search for constellations of co-occurring principles and an examination of the spatial distributions of such constellations, we will have another important tool for the detection and delineation of distinct territories occupied by different contemporaneous social groups in the prehistoric past. This is another important motivation for the intensive study of the art on the cave walls.

■ CONCLUDING OBSERVATIONS

In this chapter, I have tried to show how multifaceted is the study of Paleolithic wall art, and how an examination of some of its aspects that have previously been

little appreciated by most experts can lead us to a deeper understanding of the motivations, beliefs, and behavior of our prehistoric relatives, and of the relations that obtained between them and their natural, non-human surroundings. It is my belief that the study of Paleolithic art must be a two-stage operation. First, we must obtain a complete and scrupulously exact description or redescription and classification of each and every depiction, composition, panel, or assemblage, paying due attention to the positions and attitudes of the figures, the relations among them, and the relationships between the figures and their physical surroundings. Once this stage of the study is complete, one may proceed to the second, or analytical, stage, bringing to bear as wide a range of relevant observations as possible from ethnology, aesthetics, psychology, and ethnology, and examining the figures and artistic compositions in light of those external data. Naturally, it is essential that the comparisons undertaken in this process be meticulously controlled, to avoid baseless or fantastic interpretations. Elsewhere I have shown how these principles may be applied to the study of art from the Paleolithic and Medieval periods; the reader will judge whether or not their promise is justified by the results.

I am quite aware that the exploration begun in this chapter has only given an imprecise sketch of the lay of the land and the approximate position of some of the pathways that lead to its interior. Nevertheless, our journey to this point has given us glimpses of an unknown landscape with breadth and texture, not the well-mapped, even boring topography we might have expected. Too long dominated by armchair speculation and dilettantish fantasy, the study of Paleolithic art invites the attention of the serious, objective scholar willing to immerse him- or herself in its firsthand study. The task of understanding Paleolithic art is as demanding of systematic application and analytic rigor as any aspect of prehistory. Approached correctly, it will be found just as informative and rewarding.

NOTE

This chapter is in essence the combined text of two lectures presented during a short course on “The Present State of Paleolithic Art” at the Menéndez Pelayo International University, on June 12 and 13, 1976. The original manuscript, in Spanish, was lost while the article was in press, and so it never appeared. Several years later, it was rediscovered and returned to the author. The English translation is new. Since 1976, much praiseworthy work has been done, and many published studies have examined some of the themes touched on here in greater detail. The one that has had most impact is certainly the restudy of Lascaux by Glory, Leroi-Gourhan, and others (Arl. Leroi-Gourhan, Allain et al. 1979: *Lascaux Inconnu*). Nevertheless, I believe that publication of this paper in essentially its original form is still useful. I have not changed any of the substance of the text, but have added a few newer references where they make points better than the citations available to me in 1976.

The reference section also incorporates a number of sources that are not cited in the text. I consulted them extensively in preparing the course, and they were suggested reading for the students. I believe that today’s readers will still find them

useful. My Spanish colleagues, engaged in the firsthand study of Cantabrian wall art, insist that the existence of Paleolithic compositions is an established fact, that should not now need to be stressed as strongly as I did in the 1970s. I see, however, that the horse is not dead at all (Halverson 1987: 67); one more beating will do it good.

I did not specifically discuss the origins of art as such in the short course. Fascinating as I find the current debate concerning the origins of representation (see, for example, Davis 1986), I am not sure that it leads to any resolution, and in any case I believe that the subject stands quite apart from the issues I have addressed.

The most important modification of this text that recent research might have called for is the recognition—certainly very satisfying from my point of view—that French and Spanish investigators, many of them representing a younger generation, have already carried empirical studies of cave art in Paleolithic sites to considerably greater depths than those attained before this chapter was written. Their work is in refreshing contrast to a troubling resurgence of armchair speculation on this side of the Atlantic.

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The Many Faces of Altamira

INTRODUCTION

It has sometimes been asserted that archeological research lacks contemporary relevance. On the contrary, cases of archeological discoveries that have practical value today are not hard to find; take for example the rediscovery of dew irrigation and more recently Kolata's reconstruction of the ingenious and productive raised field system of Tiwanaku (Kolata 1993). They have other, less practical, dimensions of meaning, as well. Prehistoric monuments themselves have been turned to use by the modern world in many ways, acquiring an overlay of meaning that is seldom explored by prehistorians. That seems to be particularly true for two kinds of sites: those with human interments, and those with important assemblages of wall art—the major painted caves of the Franco-Cantabrian region. Most discussion of Altamira and the other painted caves centers (as it rightfully should) on the meaning of the decorations as cultural manifestations from the prehistoric past. With my colleagues, I have published several articles trying to interpret Altamira's decorations from that standpoint. Such interpretations only tell one part of the story. Other dimensions of meaning are also important.

One example of present uses of the past is well-known to any prehistorian who has worked in the field. Very often, the countrymen living near an important

prehistoric site have fabricated fanciful tales about it. These we generally smile at and ignore. They may be as imaginative as the stories about Christian saints that have grown over the ages in popular tradition—for example, the idea that St. Cecilia played the organ and sang hymns of praise as she was being martyred. There is probably more relationship between the two domains than is ordinarily suspected.

The study of legends about the painted caves is just one interesting aspect of a much broader field, the investigation of the contemporary “meaning” of prehistoric monuments. This topic is huge, involving as it does the ways in which prehistoric sites and materials, or concepts about the past, whether correct or misguided, are integrated in the countries in which we work into modern systems of belief and action by governments, political movements, art, religious systems, cults, legends, etc. In some cases, the modern uses of the past may be as interesting and relevant to our work as the meaning of our documents for prehistory.

It is an undeniable fact that in certain cases, traditional archeological concerns about age, artifact classification, manufacturing techniques, and functions may be less enlightening than information about how the documents from the past have been interpreted and used in the ages since their production. Well-referenced examples are not hard to find from later periods. The “Shroud of Turin” was produced at a particular time using a specifiable set of techniques. However, its age, the manner of its production, in fact all the details concerning its possible authenticity, are, in the case of that particular artifact, of considerably less importance and interest to anthropologists than the ways in which the shroud has served as a condensation and validation of belief, a stimulus to behavior, and as a nexus of interpersonal and intergroup relations through the centuries.

Like the Turin shroud, many prehistoric sites continue to have an important meaning that has little or nothing to do with their importance as scientific documents about prehistory. It is my belief that as professionals we are obliged to study and report that information. It is an aspect of our documents that may prove of the greatest importance in reconstructing and understanding the origin and transmission of folk belief, or of our own preconceptions and motives as prehistorians. There may be significant patterns and trajectories of belief and behavior that can best be seen—or can only be seen—in the many uses of the past in the present.

Prehistorians themselves have not generally made much systematic attempt to gather information about this topic or to analyze and understand it. Even those who do routinely gather and use such knowledge regard it as somehow trivial and certainly peripheral to more central archeological concerns. This “insignificant” information seldom appears in monographic reports about Paleolithic sites. The subject deserves more serious attention: it is relevant not just to prehistorians, but also to other social scientists of a variety of persuasions. No knowledge is ever trivial; supposedly peripheral or unimportant information of this sort frequently has practical implications for research, facilitating easier relations between the archeologists and the local populace, regional bureaucracies, or national governments. Prehistorians who have given it due attention have found their interest rewarded with a better understanding of the milieu in which they operate.

The following outline sketches several aspects of the present uses of the past more specifically, using Altamira as a prime example.

■ THE PAST IN THE PRESENT

The Past Is Politicized

Ideas about the remote past serve as wellsprings of ethnic or national identity. Often, these ideas are condensed on particular prehistoric monuments, just as monuments truly associated with more recent and historic figures in U.S. or Spanish history (say, Independence Hall or the Alcázar de Toledo) have served to focus patriotic sentiment. Particular monuments are regarded as part of the local heritage, to be locally venerated or exploited without interference by others, even by the central government. Where the sentimental charge is great enough, control of these monuments and associated symbols may become a focus of contention between locality and locality, region and nation, or nation and nation. As we are all aware, the interpretation of prehistoric monuments has often been forced into conformity with political doctrines concerning the evolution of society, or used to justify those doctrines and programs based on them.

Some prehistoric sites are the obligatory loci for civil validation ceremonies; unless the sites are used, the ceremonies lack legitimacy. Better-known examples include the triennial Ad Montem festival at Eton, the annual reading of the laws by the Manx parliament on Tynwald Hill, or the use of the Pontprydd Rocking Stone as a site for political rallies (Michell 1982).

Altamira is used as a conceptually legitimizing source of identity in a related way. Any Spaniard writing a general history of Spain is almost subconsciously and irresistibly compelled to discuss the cave, as though it were a prefiguration of current Spanish character and values. Spanish histories devoted to more specialized topics, such as the Reconquest, the Discovery, or the Spanish American War, often make at least a passing reference to the cave. Latin Americans, too, may find Altamira an essential reflection of their Spanish heritage (see, for example, Fuentes 1992). There is usually no earthly reason why these works need mention Altamira—the cave is not in any way illustrative of their major argument—but its use as a sort of touchstone seems to be felt as a moral obligation.

Territorial claims may be justified by reference to antiquities, real, imagined, or invented. Basque nationalism has used the painted Paleolithic caves of France and Spain to justify claims that Basque territory extended much further previously than it does at present. Apellániz's fine treatment of Paleolithic art, *El arte paleolítico en el país Vasco y sus vecinos* (1982), gives so much space to Altamira that it has been cited as supporting this contention (though Apellániz himself certainly made no such claim). Some non-Basques have uncritically accepted these territorial assertions: Isidro Cicero's otherwise excellent juvenile history of Cantabria, *Vindio* (1979), seems to suggest that Paleolithic residents of Cantabria spoke a sort of proto-Basque.

Imposing Archeological Monuments Serve as Landmarks

Where, as often happens, they are prominent features of the landscape, monumental buildings or archeological monuments give cultural order to the mental maps (and often to printed maps: see for example the British Ordnance Survey series) of those who have to travel about what may otherwise be conceived to be a relatively “featureless” landscape. The Castillo hill in Puente Viesgo is a relevant, though natural, example. Physically prominent archeological monuments have even been used to direct artillery in modern warfare.

Prehistoric Structures, Including Caves, May Still Be Used or Inhabited Today

Some sites have served as byres for animals or human shelters or dwellings relatively continuously since the Paleolithic. Inhabited structures built into caves or shelters are common in the French Dordogne, and in time of war, troops have been billeted and weapons, explosives, and supplies have been stored in prehistoric and historic archeological monuments. Altamira itself served as a powder magazine during the Civil War.

Many structures that survive from antiquity saw extensive practical service. One thinks particularly of walls, roads, bridges, and aqueducts. Many of them have needed periodic attention and repair for continued functioning. Economic utility has been the impetus needed to stimulate restoration in such cases, ensuring their survival.

Archeological Investigations and Famous Ancient Monuments Often Have Great Economic Importance

It has been rumored that it is possible to make a decent living by teaching prehistory at the university level, or by doing research in the field. That seems to be just another modern myth. But archeology may be economically important to non-specialists in many ways.

The University of Chicago’s Paleolithic excavations at Torralba and Ambrona (Soria) during the 1960s were seasonally the largest employer of local labor and the largest single source of cash income for farmers in an area including a dozen hamlets. In the 1980s at Ambrona, excavators found themselves in a tricky labor-management disagreement (one that was finally resolved to the full satisfaction of the workmen’s delegation). In their naïveté (particularly since they were paying higher salaries for “unskilled labor” than anyone else in the province of Soria) it had not fully struck the field directors that they could be defined as a “management” with economic interests opposed to those of the workmen the project employed.

With increased tourism and a growing market for souvenirs, the manufacture of modern forgeries may become an important cottage industry. So, deplorably, may the illegal and clandestine sale of real antiquities and the legitimate antiquities trade: one is as pernicious as the other. Where laws about treasure trove permit individual

finders to keep a portion of the antiquities they discover, even where there is a cash reward to the finder when excavated remains are turned over to responsible scientific agencies, clandestine excavation and the antiquities market are encouraged. Many years ago, important visitors to Altamira sometimes received small “souvenirs”—pieces of bone, shells, even stone tools, dug from the wall of the Altamira “cocina.” I have seen some nondescript pieces purported to come from Altamira in private hands.

Archeological monuments have been much used in trademarks and advertising. The sale of cigarettes called Bisontes, using as a brand-symbol one of the late Abbé Breuil’s copies of an Altamira bison, was the subject of litigation eventually resolved in the cigarette company’s favor. In the late 1980s, Ashton-Tate used the Altamira polychromes in an advertising campaign promoting one of its graphics programs for personal computers.

Admission fees to prehistoric monuments can be a substantial source of income. Altamira is a site with the greatest economic potential. At the height of unrestricted public access, between 400 and 500 tourists visited the cave *each day* in the two-month peak tourist season (*100 años del descubrimiento de Altamira 1979*). Though admissions were not charged at the time, concessions for the sale of refreshments and souvenirs, books, and postcards were very lucrative.

The accident that a population is located near a priceless archeological monument may give local peoples and institutions the impression that what is in fact the heritage of all humanity is instead their particular birthright. Were it conceded that one individual, population, ethnic group, or corporation were the sole heir to the cave and its decorations, that entity could theoretically exploit the site for its own short-term gain, and there would be no way to prevent damage to, or even the final destruction of, the site. Some important sites are known to have been damaged or destroyed for economic gain in Cantabria (principally by quarrying, as at La Pila). Altamira itself is still not completely out of danger.

Sometimes, local polities give up their economic “rights” to antiquities in their territories only after the central government agrees to pay a substantial regular compensation. That is the case at Altamira. This compact is all that has saved Altamira and its depictions from destruction. Nevertheless, there are periodic outbursts of local resentment about the agreement, in the political arena and the popular media. The fact that the Spanish central government placed Altamira under its protection by declaring the cave a part of its National Museum system—it is the only cave classified as a museum in Spain—has provoked some acrimonious exchanges. It is still possible that political pressure could reverse measures the national government has taken to protect the site.

Archeological Tourism Stimulates Culture Change

Tourism, both internal and (in the case of the most important monuments) foreign, brings substantially greater economic benefits to local food and lodging establishments: to *pensiones*, bed-and-breakfast establishments, hotels, bars, and restaurants.

Foreign tourists who visit prehistoric monuments are on the whole better educated, wealthier, and used to a higher standard of living than the average. National governments may find that the provision of adequate facilities or protection for tourists requires them to provide those facilities at reasonable rates, competing with locals, or at least to oversee the treatment of visitors directly. The pull of Altamira, more than that of the Gothic town itself, has had that impact at Santillana del Mar.

As chains of national hostelries spread, they bring with them a standardization of facilities, prices, customs, and language that would otherwise be slow to find reception. Advanced education and cosmopolitanism become increasingly common where multilingualism, formal commercial training, and an ability to deal diplomatically with educated foreigners are requisites to the operation of sites and museums. The dress and comportment of well-to-do tourists have an undeniable effect on local modes, internationalizing them.

The Ancient and Enigmatic Exercises a Special Appeal, Particularly Where It Is Aesthetically Pleasing

Handsome and intriguing antiquities or prehistoric monuments have exerted a particular fascination through the ages. They have profoundly attracted later architects, artists, and landscapers, influencing their products.

A symbolic return to the beautiful forms and styles of the past as they were known or imagined was a hallmark of Renaissance artists, of the Neoclassic Revival, of Romanticism. Paleolithic art has a substantial and economically rewarding attraction for collectors today. For several years, Douglas Mazonowicz has made his living selling masterful lithographs, etchings, and serigraphs based on Paleolithic paintings from Altamira and other sites. His work has a broad appeal, though some of his reproductions enhance or complete details that are difficult or impossible to see in the originals. (The modernist architecture of Gaudí is a related example: it self-consciously and ingeniously adapts the shapes, textures, and imaginary beasts inhabiting the travertinous caves and shelters of his eastern Spanish homeland.) Remote antiquity has a two-edged charm. The other edge of the blade, the dark chaos of the cavern, is reflected in “Grotesque” imagery in Western art (so named because excavated Roman ruins where frescoes and statues of such strange creatures as fauns were found were mistakenly thought to be caves).

In early eighteenth-century England, no wealthy aristocrat could really gaze with pleasure on his properties unless their romantically tailored landscape included a ruin. A landlord with a good ruin might have a go at restoring it to his own or his lady's taste, to make a more pleasurable showpiece. The rich who were not lucky enough to own a real ruin built artificial caves or tunnels to make up for the lack, decorating them with crystals, shells, and statues of savage beasts. The grotto at Ascot Place, Berkshire, is an excellent representative of the type (Crawford 1979; Piggott 1976).

■ PREHISTORIC SITES AND RELICS BECOME THE THEMES AND SETTINGS OF LOCAL LEGENDS

These seem to fill the need of the folk for an accounting of their presence and “functions.” Local folklore has incorporated many of the more visible Paleolithic caves in northern Spain. Most of the cave legends from Cantabria, such as those involving the Ojáncano and Ojáncana (Cyclopes) or the Anjana (Nymphs), are rooted in classical antiquity. Passed along the generations, such stories acquire the power of common knowledge, and despite their implausibility, it is hard to shake them with contrary evidence. A widespread legend speaks of a golden Moorish treasure, wrapped in a bullhide and hidden away in a Cantabrian cavern. Caves bearing names that sound to the popular ear like references to Moors (e.g., la Mora, Morín) reinforce such myths, despite the fact that Cantabria, a wellspring of the Reconquest, never fell under Moorish domination.

We have heard from dozens of local people that the cave of el Juyo (where we have worked for many years) has galleries that go on for miles, and contains a subterranean stream that emerges kilometers away in another village. The site, opened in the 1950s, has now been completely explored, and neither of these things is true; it is a small cave with a subterranean stream whose emergence nearby has been satisfactorily demonstrated with colored tracers. Yet adults we have shown the whole cave say that when they were children (that is, before the cave was discovered) they personally visited the site and saw what they could not possibly have seen, and we are convinced that they are not deliberately lying.

Such tales must not be disregarded as aberrations of the uneducated. There are erudite myths as well, such as the seventeenth-century tale that the village of Igollo was the site of a palace built by Prince Astur, son of Isis (Io) and Osiris (Io = Iollo = Igollo). The ruins of the “palace” are in fact a natural rock outcrop, not a prehistoric site, but the story is nonetheless illustrative. A heterodox school of local scholarship perpetuates such tall stories—and even wilder ones, about extraterrestrials and Atlanteans in the painted caves—today.

Even professional prehistorians are not above such fantasy. Many otherwise reasonable professionals stubbornly entertain misconceptions that are just as improbable as are popular folktales about the caves. These include the unshakable conviction that the commonest way of applying color to the cave walls was as a paint mixed with grease, blood, or marrow (no greasy or oily base would penetrate damp walls or adhere as well to them as would dry pigment or a water suspension), that animals depicted are not the ones whose bones are found in the food debris in Paleolithic levels at the sites (at Altamira, the mammals on the walls *are* the same ones found in Magdalenian deposits), that *all* Paleolithic depictions are finished masterpieces—neither children nor unskilled doodlers had any part in their production (like other sites, Altamira has its share of poorly executed figures), and that Paleolithic art is always located on inaccessible surfaces—the highest ceilings or deepest recesses of the remotest cave galleries (the polychromes on the Great Ceiling were close to the cave entry, and the ceiling was very low). While each of

these affirmations may correctly characterize some particular site or group of paintings, exceptions outnumber the “rules.” The most perplexing aspect of these beliefs is their endurance in the face of so much contrary evidence.

Prehistory is a surprisingly conservative discipline. Its practitioners make every effort to sustain outmoded ideas until the last possible moment. Misinterpretations created, perpetuated, and disseminated by prehistorians often originate in statements (sometimes out of context) by accepted authorities that incorporate unacceptable oversimplifications or overgeneralizations about very complex phenomena. Some of these fixed ideas persist as the result of didactic oversimplification by teachers trying to drive home a few easily remembered principles; they are passed on from one generation to the next as convenient *aides-mémoires*. Others are harder to explain.

■ PREHISTORIC MONUMENTS AND POPULAR CONCEPTIONS ABOUT PREHISTORY ARE OFTEN USED BY FRINGE CULTS, ESOTERIC SOCIETIES, AND OTHER VOLUNTARY ASSOCIATIONS

This is not the case for Altamira, probably due both to the relative recency of its discovery and the fact that access has been controlled. Other sites have been less fortunate. Mounds, stone circles, and gallery graves are particularly frequent victims of these practices. Not too long ago, periodic meetings of local antiquarian societies traditionally took place at famous and imposing archeological sites; unhappily, some damage to the monuments inevitably ensued. Groups of speleologists still hold reunions at caves, including prehistoric sites, and to commemorate their visits will sometimes set a plaque into the rock, or chisel the group designation or members' names into gallery walls. Fortunately, most speleologists who work in the caves of northern Spain collaborate intimately with prehistorians or include prehistorians in their ranks; those groups are among the first to condemn such vandalism.

A splinter branch of the Rosicrucians, founded by S. I. MacGregor Mathers, was called the Temple of Cromlech, and there are evidently similarly named subdivisions in the parent organization. A tunnel-like rock chamber intended, I presume, to suggest a cave or passage-grave was an important ritual symbol for that rite (Mathers 1988).

The use of Stonehenge as a ritual site by the so-called Druid Revival is probably the most familiar example of the cult use of an archeological site. In recent years, Stonehenge has been fenced by the British government, to prevent vandalism and incidental damage. The reconstituted “Cornish Bards” are another group assembling periodically at stone circles (Michell 1982).

■ ARCHEOLOGICAL FINDS AND MONUMENTS MAY BE TURNED TO USE BY ESTABLISHED RELIGION

This has not been the case at Altamira, probably because it was only officially discovered quite recently. There has been insufficient time for the site to become in-

corporated in pious legend in relevant ways. However, other examples are not hard to find. An elephant bone from one of the Acheulean sites near Medinaceli was venerated there as a relic in the Catholic church of San Román, and annually carried in religious procession. It was thought to be a bone of the giant camel who pulled a wagon in which the relics of four Christian martyrs were miraculously transported to their final resting place. A striking case of the association of a Christian saint with a prehistoric monument is a sixteenth-century French painting now in the church of St. Merry in Paris, showing St. Geneviève using as her sheepfold a now destroyed prehistoric stone circle at Nanterre (Michell 1982: 110).

Caves were used in cult and served as models for early religious “architecture.” The occurrence of early Christian relics in some caves suggests that they may have served as places of worship. Caves served as the refuge of hermits. The earliest Christian churches in northern Spain are the *Iglesias Rupestres* (mostly circa ninth century)—rock-cut churches like that at Arroyuelos in Cantabria (González Echegaray 1969). These tiny churches were excavated from the living rock following the model of a natural cave. Some of the sacred grottoes of the classical period became shrines of the Virgin in Christian belief. Apparent references to worship in caves are other evidence of the practice. The followers of Priscillian seem to have celebrated initiation rites or other secret ceremonies in caves, a practice finally forbidden under pain of anathema by the First Council of Zaragoza in 380 AD, “*nec habitant latibula cubiculorum ac montium qui in his suspicionibus perseverant . . .*” (“those who are obstinate in these beliefs should not utilize hidden chambers in sepulchers or hills” [for their reunions]).

Human remains found buried in Roman ruins underlying modern churches are often venerated as Christian saints. It is well-known that pagan religious buildings and shrines were frequently converted to Christian use, and that new Christian temples, with associated interments, were built atop older non-Christian religious foundations. Only exceptionally is there documentary proof of the identity of the bones, and where claims are made that the remains are those of a particular individual, the basis is most frequently nothing but pious speculation.

■ THE PRESENT IN THE PAST: DISCOVERY AND VALIDATION OF ALTAMIRA

Other interesting aspects of the past in the present are revealed by a close examination of Altamira’s history as a monument of Paleolithic art. The story of the discovery by Sanz de Sautuola and authentication of its paintings is a rich field for exploration, with facets whose understanding is important to anthropologists, prehistorians, psychologists, folklorists, and theologians. As is well-known, Altamira’s paintings were the first to be recognized as Paleolithic. The cave was found relatively recently—it seems that it was first known to the countrymen around Santillana in 1866–68. Because of its late discovery, legends of classical antiquity are not attached to Altamira. The legends about the cave are more recent. With other caves, mysterious passageways from the known, everyday world to the fascinating and dangerous

underworld, Altamira shares in a certain symbolic mystique. There are other equally deep dimensions to the symbolic value of this cave as a monument of Paleolithic art.

Altamira's paintings vividly display the sophisticated symbolic and expressive capacity of our early ancestors. They reflect the antiquity of behavior very like our own, suggesting our own indestructibility—a comforting and appealing thought indeed. Like the tomb of the pharaoh Tutankhamen, Altamira seems to evidence immortality. Like the bodies of some saints, its sanctity is certified by its incorruptibility. The public does not want to hear that the paintings at Altamira are deteriorating, and when they are so informed, they react in disbelief, sure that the community of scientists is trying to sequester the site and its paintings for financial gain or other nefarious ends. (These attitudes could be overcome with an appropriate educational campaign, but the government has as yet not understood the need to mount one.)

Like the pyramids, or the Dome of the Rock, Altamira produces reverential feelings in its visitors. It is no accident that, when referring to important decorated caves, students of Paleolithic art inevitably resort to the undefined term “sanctuary,” even though most non-trivial definitions of the word do not seem to fit the empirical evidence from the caves well. Despite that fact, there seems to be general agreement that the term is appropriate. This ill-defined concept strengthens quasi-religious feelings of awe that have an unconscious influence on prehistorians who study and evaluate the depictions, even at the level of their basic description. If the caves are sanctuaries, it follows that their figures must be supposed to illustrate themes of fundamental importance to prehistoric people—magico-religious themes that somehow affect the reproduction of the game. As Ucko and Rosenfeld (1973) have pointed out, while that may be true in some cases, in just as many others it may not.

Only Leroi-Gourhan (1967 and elsewhere) and Laming (1959 and elsewhere) explicitly specified the evidence they believed would support the claim that decorated caves were sanctuaries, and their procedures for recognizing the complementary oppositions on which they based their conclusions are not rigorous enough to be replicable. Nevertheless, the idea continues to dominate interpretation. The reasons why this is so may run deeper than most prehistorians suspect. They can be seen in operation in great relief in the story of the discovery and authentication of Altamira's paintings. The treatment given to the site shows remarkable point-for-point parallels with the treatment of Christian religious shrines and sanctuaries. I believe that is no accident.

The Discovery

William Christian's book *Apparitions in Late Medieval and Renaissance Spain* (1981) analyzes legends about visions and the establishment of religious shrines. With surprising frequency, they involve the discovery or disinterment of a sacred image by an animal, often a herdsman's dog. The dog is a creature standing astride the threshold between the natural and the cultural worlds. A child or countryman may be

taken to the image or led to a place of apparition by the animal. The ecclesiastical investigators considered poor rustics, particularly men or young children, to be the more reliable reporters; they were apparently believed too simple and honest to deliberately try to deceive. Reports by women or the well-to-do were less likely to be credited. More than a third of the cases examined involve the discovery of an image underground or in a cave, and another eighth is associated with springs. Caves and springs are themselves liminal places. It is of course a fact that caves were frequently used as hiding places for “valuables,” including church paraphernalia, and dogs will dig in disturbed ground or enter crevices. Nevertheless, too many of the shrine-foundation tales involve such behavior. Christian undertakes a fascinating analysis of the contexts and symbolic meaning of apparitions, but the part of his work that is most relevant to this essay is the evident parallelism between the stories of discovery of religious shrines he documents and those about the discovery of our painted “sanctuaries.”

Obviously, some of the caverns a dog or sheep might enter could contain Paleolithic decorations. The proportion of painted caves that are said to have been discovered by animals is small, because so many had accessible entries that were well-known to all the locals. However, this proportion increases when one considers just those principal painted caves discovered in recent years whose entrances are stated to have been previously closed or hidden from sight.

In fact, the two most famous Paleolithic art sites, Lascaux and Altamira, are supposed to have been revealed in just this way, and in both cases, there is reason to think the story is not literally true. At Lascaux, on September 12, 1940, four boys—Ravidat, Marsal, Agnel, and Coencas—wandering over a hillside saw their small dog “Robot” enter a burrow. Trapped inside, the dog began to bark, and in rescuing him the boys tumbled into a prehistoric wonderland. This story has been widely popularized and is still generally believed. But it is known to be untrue: the youth of the discoverers is usually exaggerated; the first entry was on September 8; only two of the four official “discoverers” were present on September 8 (Ravidat and Coencas); the dog story is apparently apocryphal; although the cave was still unexplored, its entry had been known to the locals since before the First World War, and perhaps for centuries; last, the formal discovery of the cave was not accidental—the youngsters set out deliberately to explore it, with a lantern Ravidat, an apprentice mechanic, had built just for such explorations (Delluc and Delluc 1979).

The outlines of the Altamira story are strikingly parallel to the legend of Lascaux. It is said that Altamira’s discoverer, the countryman Modesto Cubillas, was out shooting with his dog in 1868. The dog chased a fox down a hole, and unable to retreat, barked until its master released it by pulling some fallen boulders away from what turned out to be the entrance to the cave. Now, two decades had passed before any part of this story was published, and the name of the hunter was added in only the 1960s. One suspects that the tale may have been embellished, particularly since the site was locally known as the Cave of Juan Mortero, and it is reported that before Sautuola worked there, the cave entry had been used to store traps. Of course, there is no necessary contradiction here—all this information may possibly be true—and

after all, these are relatively meaningless details that seem to have nothing to do with the meaning of the art. On the other hand, if everyone thinks the story about the hunter and his dog is really trivial, why is it so insistently repeated?

Though there is too little evidence to establish this as anything more than a crude working hypothesis, I personally believe that such strict parallels as those in the discovery legends about religious shrines and painted caves suggest that we may find other parallels between them in popular belief. Certainly, we ought to look for such parallels. If found, their presence and content may help us understand just what so many prehistorians, including specialists in the study of Paleolithic art, mean when they call the painted caves “sanctuaries,” and just what that otherwise indefinable set of qualities that indicates “sanctuary” may be to them. One might perhaps discover that decorated Paleolithic caves are regarded as a subset of a more readily definable category of religious sanctuaries, or perhaps more likely, that both are conceived as subsets of a more general symbolic category of locales at a deeper structural level.

The Process of Authentication

Further parallels between the careers of painted caves and religious shrines are found in the long process by which the Altamira paintings were finally authenticated. It is so similar to the process through which claims of authenticity for new religious shrines are validated by the ecclesiastical hierarchy that the resemblance can scarcely be coincidental.

The most usual explanation offered by today’s prehistorians for the doubts cast on the age or authenticity of the Altamira paintings is that they were thought to be too masterful for their apparent great age. When the Altamira paintings were discovered, the French School of Anthropology was still dominated by its founder, Paul Broca. The doctrines of established prehistory were sustained by a hierarchy of French authorities, under the primacy of Gabriel de Mortillet. His followers, among whom Émile Cartailhac was one of the foremost, explained, expanded, and defended the orthodox line. This influential archeological establishment, convinced Darwinians all, is supposed to have decided that the artistic quality of the polychromes was too evolved for the mental and aesthetic abilities of hominids who were still primitive “Cave Dwellers.”

In fact, that explanation is by and large incorrect. It is both incomplete and anachronistic. By no means all who called themselves anthropologists or archeologists in the 1880s were confirmed Darwinian evolutionists: such an illustrious and accomplished prehistorian as the Marqués de Cerralbo, much of whose best professional work was devoted to finding the remains of the earliest peoples and cultures of Iberia, in association with the remains of ancient elephants and other extinct fauna, was a catastrophist who long after Sautuola’s death maintained that the world was only 6,000 years old. Ideas about the trajectories of cultural and biological evolution were by no means as resolved and crystallized as we now think they must have been, and opinions that today seem obviously inconsistent or mutually contradictory were in the past often seriously and simultaneously entertained by sound and

reputable scholars. While some who could be called “Darwinists” opposed the paintings’ authenticity (Lubbock, for example), others of that school did not. Even more to the point, among the most vocal opponents of Altamira’s paintings were some outspoken anti-Darwinists: Rudolf Virchow, a principal and influential opponent of the Altamira discoveries, was just as strongly opposed to the theories of Darwin and Haeckel, or to the idea that there were “Ice Age” people at all. Allegations that the Altamira paintings were too accomplished for prehistoric cave dwellers were evidently *a posteriori* rationalizations, used by a minority of critics.

Other evidence shows that the mythic account must be at least partly wrong. By 1880, human skeletons from Upper Paleolithic levels were known to be quite modern, so the fact that cave-dwellers should have been like us in other ways was not anticipated by most authorities. In fact, when the Altamira paintings were discovered, art was already a well-known aspect of the orthodox picture of Upper Paleolithic behavior. Engraved bones were first found at Chaffaud in 1834, and other specimens had been gathered by Lartet at Massat in 1860. Lartet and Christy’s *Reliquiae Aquitanicae* (1865–1875) reported many more. Worsaae, an authority in world prehistory, announced his acceptance of the Chaffaud finds as early as 1869. By 1883, the Museum at St. Germain held 116 engraved and sculpted Paleolithic objects in bone, antler, and ivory.

Émile Cartailhac himself, later their bitter opponent, was at first enthusiastic about the Altamira polychromes; on December 30, 1880, he wrote to Sautuola: “Your site is in every way like those we attribute to the Reindeer Age . . . I don’t believe that there has been any discovery in Spain more important than yours from the viewpoint of prehistoric archeology. . . . It would be unusual if the cave painters hadn’t also sculpted or engraved animals on bones and pebbles” (letter quoted in Madariaga 1972: 86). It certainly seems that at the time the discovery of the paintings at Altamira was first announced, Cartailhac did not feel his Darwinian tenets were challenged in the least. It was only later (and for other reasons) that his opinion changed.

Nor was the argument over the Altamira paintings originally based on the supposed fact that the sophistication of the art did not fit de Mortillet’s notion of mental and technical progress. It fit his ideas relatively well, as he himself explained in 1881: “art is not a special attribute of certain isolated populations, but one of the general characteristics of the Magdalenian period.” But this statement is part of his rejection of the authenticity of the Altamira paintings. When Cartailhac sent copies of the drawings to de Mortillet, the latter immediately rejected them, saying he suspected that Altamira was a fraud designed to discredit practitioners of the infant science of prehistory: “just a glance at the copies of the drawings you send me in your letters is enough to show that this is a farce; a simple caricature. They were produced and shown to the world so everyone would laugh at the gullible paleontologists and prehistorians” (1881 letter to Cartailhac, cited in Madariaga 1972: 83). The Altamira paintings were rejected by the establishment at the Lisbon Congress of 1880, not because of their sophistication—many thought them naïve rather than terribly sophisticated—but because rumor had it that they were forgeries. A debate that began as a relatively trivial interchange between Sautuola and a few opinionated provincial

literati had become intertwined with a politico-religious battle between rival doctrinal authorities. Altamira's advocates were on the losing side, and consequently Altamira too suffered, at least temporarily.

In many respects the debate about Altamira's authenticity had less in common with scientific investigation than it did with attempts to expunge heresy and the resolution of religious disputes. There is a relatively formalized set of procedures that is generally followed in the validation and recognition of an important religious shrine by the Church establishment. New shrines, the places where apparitions or miracles regularly occur, have such potential to support or undermine official doctrine that their claim to authenticity must be received initially with skepticism, followed by an on-site inquiry to establish that they are not simply delusions or fabrications. Once this phase is passed, prosaic explanations of the associated phenomena are sought. If the phenomena are inexplicable as purely natural or accidental occurrences without supernatural significance, one must next ensure that they are not traps set by the forces of evil to seduce the unwary from the paths of orthodox belief. Those involved must be questioned, and all apparitions, or other apparently supernatural phenomena, must be examined to determine that they are truly beyond the realm of everyday experience, and that they are consonant with the rest of orthodox doctrine. A shrine that passes these tests is sanctioned, but at the same time it is invaded and controlled by the ecclesiastical authority—and in this respect religious validation differs from ordinary scientific verification. These stages of authentication have striking parallels to the peculiar validation process to which several of the most spectacular assemblages of Paleolithic art—not just Altamira—have been subjected.

The announcement of the discovery by Sautuola of Paleolithic paintings at Altamira was at first met with accolades at best, and at worst, no more than the respectable reserve novel evidence usually excites. Members of the Sociedad Española de Historia Natural congratulated Sautuola when they received his communication and a copy of the paintings; they urged the Ministry of Patronage to underwrite intensified investigations in the Santander caves. Immediately, however, Sautuola found his conclusions about the great antiquity of the figures assailed locally. Principal among the critics was his Cantabrian compatriot, Ángel de los Ríos.

At the end of the eighteenth century, there were still in Spain many respected scholars and literati who took both the Bible and the legends of classical antiquity to be valid sources of literal truth: de los Ríos was one of these. Ignorant of the findings of prehistory, he used a fine classical background and knowledge of the Bible to argue, with vigor and skill, that no true prehistory could exist, and that all the paintings could have been produced in historic times.

He observed, for example, that peoples who made stone implements need not have been ignorant of metals, since Tubalcain worked copper and iron at a time when stone knives were still made (Madariaga 1972: 211, 214). No matter how silly or trivial such arguments seem today, many at the time found them quite convincing, when they appeared in the *Eco de la Montaña*. Finally, waspish tongues claimed that the polychromes Sautuola had admittedly not seen during his excavations in 1875 had actually been painted between then and 1880; the evidence advanced was

the fact that Sautuola had hired a French painter, M. Ratier, to work in the cave in 1879. (Ratier was of course making copies of the depictions, not painting the figures himself.) Others accused some unknown North American, who would of course presumably know more about bison than would a French or Spanish painter. It is especially noteworthy that these detractors almost universally belittled the artistic quality of the paintings: while their shading and proportions are thought too “mannerist” for prehistoric art, the polychrome figures were nonetheless characterized as “primitive” and “about what one would expect from a mediocre student of the modern school.”

Had it not been for its coincidence with unrelated events in French prehistory, this debate might have remained a local one. In 1880 the death of Paul Broca sparked a bitter factional fight for control of the French School of Anthropology, aligning de Mortillet, an opponent of the Altamira discovery, and his colleague Cartailhac (recognized as the foremost French authority on the anthropology of Spain) against others, among whom were the defenders of Altamira. Altamira sadly became embroiled in the war for succession. De Mortillet’s faction finally won the day, establishing themselves as the most influential of anthropologists in France, and him and Cartailhac as the two most influential prehistorians.

The “official” authentication of Altamira coincided with the onset of the battle. To resolve questions raised about their authenticity, the French anthropologists sent E. Harlé to examine Altamira’s paintings in person. Harlé, apparently at first inclined to consider the paintings authentically ancient, heard the local calumnies circulating about Sautuola and forgery, and, deciding that so much smoke must indicate some fire, finally concluded that the figures were recent products. His 1881 report (hasty and full of errors of fact) rejects claims to antiquity for the paintings, but does exonerate Sautuola, making him an innocent dupe rather than a complicit criminal. From the date of that report until 1902, Cartailhac reversed his field, refusing to admit Altamira’s authenticity, without ever himself examining the figures at first hand. He feared, as he said, that they were falsifications by the Spanish “Jesuits” to make the world laugh at the credulity of the new priesthood of paleontologists and prehistorians. A friend had told him: “Watch out! they are about to play a trick on the French prehistorians. Don’t trust those Spanish priests.” The phrasing is illuminating (letters and articles by Cartailhac quoted in Madariaga 1972: 186–89).

Cartailhac stuck to his contrary position even after the discovery of other Paleolithic painted caves in France after 1895, particularly Rivière’s work at La Mouthe (whose authenticity he accepted apparently by 1896 or 1897) and Daleau’s (1896) discovery of engraved animals covered by Perigordian strata at Pair-non-Pair. He maintained his negative attitude about Altamira despite the urging of other accredited prehistorians who had visited the Spanish site with open minds.

A careful evaluation of Cartailhac’s position puts a different light on his resistance to Altamira, one that has nothing to do with disjunction between the paintings’ quality and current evolutionary theory. It is no accident that Cartailhac envisioned his motives in disbelieving Altamira in terms of a battle with a rival group of ecclesiastical authorities, the “Spanish Jesuits,” who represented heterodoxy from

his perspective. The debate was in a real sense a religious dispute, based on faith, not experiential evidence. (In fact, Cartailhac himself refused to examine the evidence at first hand, despite reiterated invitations to do so.) The title Cartailhac chose for the 1902 article in which he finally renounced his former position, admitting that his doubts were misplaced, vindicating the (by then) deceased Sautuola and admitting Altamira to its rightful place in the revealed truths of orthodox prehistory, sets an appropriate tone for the recantation of heretical religious beliefs: the “*Mea culpa* d’un sceptique.” It is, to say the least, ironic that subsequently it was Cartailhac himself, aided by his young protégé, the Abbé Henri Breuil (later, and only partly in jest, nicknamed the “Pope of Prehistory” by his admirers), who undertook the restudy and monographic publication of the Altamira site. Cartailhac and Breuil legitimized the “sanctuary” as they placed it under the control of orthodox (French) prehistory.

I am not the first to have recognized the religious overtones of the Altamira controversy. In 1902, Luís de Hoyos Sáinz referred to Cartailhac’s apology for disbelief in the following terms: “this is another example of religious and irreligious jealousies at work. Cartailhac himself admits that was the origin of the process, as I had already heard from lips that may well have influenced his judgment. The criteria framed by the opponents were too narrow, and the specter of clericalism disturbed the tranquil course of scientific investigation, as on other occasions it has been disturbed by the irreligious. There should be no such thing as a Catholic archeology, any more than there are atheist or Buddhist mathematics, physics, or engineering. If those who write about archeology do so in an attempt to attack dogma, the result, besides being non-scientific or anti-scientific, will probably be in bad taste, and certainly superficial and stupid” (quoted by Madariaga 1972: 189).

Had this series of events happened only at Altamira, it could be called an accident, a unique coincidence from which little can be learned. But very similar stories of quasi-religious validation can be told about the forced vindication of La Mouthe, Rouffignac, and some other painted caves; such stories continue to unfold today. Both the discovery legends and the process of validation of major Paleolithic “sanctuaries” parallel those characteristic of newly invented religious shrines. It is important to note that these phenomena are not the rule but the exception in prehistory, and their exceptional nature underlines their importance. Ordinarily, the discovery of a new archeological site, or the recognition of a new tool type or a new industrial complex, is not challenged in a similar way. We customarily assume that our colleagues are responsible scholars, who would never intentionally mislead us. We commend new discoveries without much question (and sometimes regret it). We do so, that is, unless those discoveries involve important “sanctuaries” with Paleolithic art or Paleolithic burials. Then the machinery of inquisition jerks ponderously into motion, sometimes with salutary effect, but on occasion (and for almost two decades at Altamira) with outrageous results.

A special conjunction of feelings about the mystery of caves and notions about the romance of art privileges the study of Paleolithic decorated caves. Those special beliefs and feelings are held by the professional prehistorian as well as the average citizen. Neither is particularly good at self-analysis. In fact, most of us are not even

aware that we have such notions. For the layman, it may not be important to understand them. For the professional, on the contrary, understanding motives, attitudes, and ingrained preconceptions is an essential step in the direction of freeing research from unconscious bias. One possible route to that understanding lies in an examination of substantial disjunctions between the tenets and behavior of investigators working on such sites and the ordinary attitudes and usual procedural standards that are applied by competent professionals. When fixed ideas about prehistoric art, or about decorated sites themselves (or sites with Paleolithic burials), run counter to experience, there is such a disjunction. Where stricter standards of validation, or very much different standards, are demanded for one class of prehistoric data than would ordinarily be applied in the best research (as is the case for the authentication of such decorated monuments as Altamira) another area of disjunction appears. A careful examination of these situations, in an attempt to understand the basis of disjunction, is surely one of the obligations of those who study Paleolithic sites. For, unless we understand why the “special” sites are “special,” and why we treat them so differently than we treat other archeological evidence, we cannot study them dispassionately or analyze them without unconscious bias.

I realize that I have outlined a rather remarkable story about Altamira. I have claimed that fabricated tales about the discovery of new Paleolithic sites with monumental assemblages of Paleolithic art, and the way those assemblages are validated by the archeological profession, are formally and substantively so analogous to the circumstances associated with the discovery and validation of newly revealed Christian shrines that it can be no accident. There are reasons to believe that the behavior associated with the Paleolithic sites is not directly modeled on that surrounding Christian shrines, but that these two manifestations of belief, reverence, and validation of experience have the same origin at a deeper structural level. I still cannot pretend to understand that origin; I believe it to be promising material for further serious investigation.

■ CONCLUDING OBSERVATIONS

In this exercise, I have tried to explore some dimensions of the uses of the past in the present. I have not just tried to pour old wine into new bottles. In fact, I fear that we prehistorians sometimes overlook fine old wine in its own bottles, that would be easily found if we looked hard enough. I believe that the study of prehistory must be more than the recasting of old data in the framework of a new narrative with contemporary appeal. It must try to understand both the past and what the past means today to laymen and prehistorians alike.

The present undeniably impinges on the past. As prehistorians we interpret our data in ways that are conditioned and limited by our backgrounds, our preconceived ideas, and the settings in which we work. But that does not mean there can be no “truth” about the past. Our task is not to write new fairy tales about the past; we have a responsibility to be faithful to our documents. An interpretation not consonant with our evidence is worthless—a “feigned hypothesis.”

As scholars, we have an obligation to add to knowledge and understanding wherever we can. An appreciation of the ways in which the prehistoric past, rightly or wrongly construed, is made to serve the present, and the present affects our views of the past, cannot help but provide useful and interesting information on the generation of myths, the development and spread of popular traditions, and the functions of folk belief (whether those beliefs are sustained by the uneducated public or by professional anthropologists). By careful investigation we may hope to understand how delusions come to have the force of tradition and how the processes of occupation-related mythogenesis operate. These are important fields to all interested in folklore and belief. Such explorations add new dimensions of texture and relevance to the study of prehistory. They have immediate practical value, helping us see how we may smooth our relationships with the public at large, and with civil and religious authorities in the areas we study. I firmly believe that the exercise may make us aware of the constraints of the present on the past, and move us closer to a real understanding of the past in all its complexity.

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Techniques of Figure Enhancement in Paleolithic Cave Art

INTRODUCTION

Not so very many years ago, the primary aim of those studying Paleolithic art was to catalogue it, to define different styles, and to arrange them (based on superposition and the “logic” of stylistic evolution) in a developmental sequence (Breuil 1974). Sometimes, artistic depictions were convincingly interpreted as faithful reflections of the external environment (González Echegaray 1974) or, less convincingly, as enigmatic representations of religious symbols (Luquet 1926). From the totally *a priori* premise that the Paleolithic artist was only rarely capable of conceptualizing multigure compositions (groups were explained as simple juxtapositions), the isolated individual depiction was ordinarily the datum for investigation (Breuil and Lantier 1959: 245; Hawkes 1963: 197). Most prehistorians recognized that one aim of Paleolithic art was to convey information to the artists’ contemporaries. But it was only belatedly, after the stimulation of Annette Laming-Emperaire (1962) and André Leroi-Gourhan (1964, 1965: 110, 194), that there was any general awareness that Paleolithic figures often occur in meaningful associations, or that information about the complementarity or opposition of meaning might be gleaned from a study of grouping and the spatial relations between figures or compositions. As Leroi-Gourhan himself observed, despite the difficulties in dating parietal art, the

figures on cave walls are still in the places where prehistoric men put them, and their placement and associations imply something about their meaning (Leroi-Gourhan 1964: 8, 82).

Most prehistorians who have tried to get at aspects of the significance of prehistoric art from a firsthand study of its documents will have noticed that certain figures, by virtue of their anomalous treatment, stand out from the rest of the depictions accompanying them. It seems very likely that these figures have been singled out to call particular attention to their special meaning. They may even be foci towards which other information encoded in a set of figures is directed. But, whatever its motivation, the enhancement of particular figures deserves the especial attention of the investigator, even if the analysis does not lead in any obvious way to clearer conclusions about the depictions as a whole.

A discussion of Paleolithic figure enhancement will at least provide a basis for more detailed comparisons of artistic conventions at different times and places. We may even hope that such a study might potentially add to a corpus of evidence of eventual utility in comparing and contrasting the mental processes of particular Paleolithic and modern groups.

To the best of my knowledge, Paleolithic figure enhancement techniques have not before been catalogued, although most interested specialists have a more or less impressionistic feel for them, and certain have been discussed by others in some detail. The most meticulous and thorough treatment of some aspects of the topic to date is contained in André Leroi-Gourhan's *The Dawn of European Art* (1982), though enhancement as such is not specifically discussed in that work.

This chapter presents a rough sketch of some commonly observed devices used by Paleolithic artists to give particular features special impact. Though it enumerates the techniques I am aware of, it must still be incomplete; it is published in the full expectation that it will be criticized, amplified, modified, or rejected by my more knowledgeable colleagues. I hope that in the process we will all be led somewhat closer to an understanding of Paleolithic art.

Figure enhancement is a process that involves both the artistic depiction and the perceptual apparatus of the viewer. It does not exist without the participation of both. Consequently, the recognition and appreciation of particular techniques of figure enhancement can never be entirely objective; an element of subjective judgment is always involved. Provided that the judgmental element is informed, the process is not stigmatized. There are many complex problems of qualitative evaluation and pattern recognition that are more rapidly and effectively done by the human analyst than by the most elaborate "objective" electronic computer in existence. It is interesting to note that the creative aspect of mathematics, too, is the discovery of systems or theorems by plausible reasoning or informed guesswork. Only after the creative phase can one proceed to quantitative proof of the theorem by the objective, rigidly formalized procedures of demonstrative mathematical reasoning (Polya 1973). Pattern recognition and evaluation are among the many areas where the dichotomy between "art" and "science" proves false, where scientist and humanist are one.

Many figure-enhancement procedures are notable only when a single figure or group is examined in the total context of its relationships to other depictions, or to the topography and surface conditions of the walls, ceilings, and floors of the galleries where it is found. Thus, for definition of enhancement techniques, three elements are always involved: the depictions viewed, the surroundings in which they are viewed, and the impact they produce on the viewer. It might reasonably be objected that a depiction would have meant very different things to a Paleolithic observer than it means to a modern one. Animals must have meant something quite different to those who depended on the hunt for their daily survival than they mean to the prehistorian raised and trained in today's urban industrial world. There is no doubt that some aspects of the meaning of Paleolithic figures can never be recovered. But it is a fact that many Paleolithic animal figures are recognizable and even judged realistic by a modern viewer. What is more, as the student learns more about the depicted animals and their behavior, seemingly irrelevant or inexplicable details of the Paleolithic figures are seen to be reliable indications of coat condition, or stereotyped activity appropriate to a particular season, a temporary phase of development, or cyclic behavioral phenomena.

These observations show that no wide gulf separates the perceptual apparatus of Paleolithic artist and modern viewer. When certain figures stand out strikingly from the ordinary run of depictions in a site, gallery, or composition, from our point of view, we may assume that the artists also recognized their unusual character, whether they consciously intended the figures to stand out or not, and regardless of the deeper symbolic meaning of the enhanced depictions. It is very possible that on occasion the nature and relationships of an enhanced figure and the technique of enhancement might indicate aspects of its symbolic content or directions of further research that would lead to future understandings of its value. For present purposes, however, that possibility is of no immediate concern; my principal aim is the categorization of the techniques, not the elucidation of motives and meanings.

■ A PRELIMINARY CATALOGUE OF ENHANCEMENT TECHNIQUES

Isolation

No one can fail to have noted that viewing figures that have been hidden away in inaccessible nooks, pits, or galleries, or unexpectedly coming upon figures after passing through long passageways that are completely devoid of art, heightens the viewing experience and our appreciation of the art. It strongly suggests that the figures were intended to be out of the ordinary, even though they may be sketchily rendered or technically and stylistically average for the site. The line drawings in the *puits* at Lascaux draw our attention by their isolation, reinforcing the impact of their unusual subject matter (Leroi-Gourhan and Allain 1979; Laming 1959). The same may be said of the figures in the cupola at La Pasiega and the narrow scutiform-lined passage at the end of a gallery at the same site (Breuil, Obermaier, and Alcalde del Río 1913). (Many other cases are known, but I intend here simply to give one or a few

illustrative examples of each technique, rather than attempting the kind of exhaustive listing that only someone far more expert in the specialty could provide.)

Size

Figures that are either very much larger or much smaller than other depictions nearby attract our interest because of the size contrast. Figures such as the polychrome hind on the ceiling of the Sala Grande at Altamira stand apart from the rest of the polychromes in relative or comparative size; the polychromes contrast in relative size with the smaller outline and monochrome figures on the same ceiling and adjacent wall.

The size of the polychrome figures is also impressive in a more nearly absolute sense, however. They are quite large, and when viewed from the contemporary gallery floor, which brings the viewer's eye very close to most of the paintings, give an impression of much greater size (Breuil and Obermaier 1935). The viewer can feel almost overwhelmed by their scale. From most observer positions, complete overviews of figures in their integrity are difficult or impossible to attain, since one cannot easily stand back far enough from them to take them in. Some of the human heads at the Cueva de Hoz (Barandiarán et al. 1981) are monumental in scale in an absolute sense; considerably larger than a human viewer, they produce a similar, awesome effect. The disproportionate size of the large bulls in the main hall and axial gallery at Lascaux (Bataille 1955: 50–90; Leroi-Gourhan and Allain 1979) is another obvious example.

Attitude

Certain animal figures are arranged in postures or attitudes that contrast notably with other figures in their surroundings or with what the viewer supposes to be the "normal" pose of an animal at rest. Sometimes, as in the case of the "leaping" cow above the frieze of little horses in Lascaux's axial gallery (Bataille 1955: 85), an animal may be pictured at the height of some exaggerated action, legs violently doubled or stretched and extended. Other examples are the galloping horse at Font de Gaume (Breuil 1974: 82) and the galloping bison, formerly misidentified as a wild boar, on the great ceiling at Altamira (Freeman 1978: 171).

Sometimes animals are shown upside down, feet in the air, or in other unusual postures, suggesting that they are falling. There is such a "falling" horse at Lascaux in the axial gallery (Bataille 1955: 81), and a head-down, vertical bison at Altxerri (Altuna and Apellániz 1976: 63; Leroi-Gourhan 1982: fig. 68). On the other hand, there are depictions of attitudes that are less agitated but nevertheless striking. At San Román de Candamo (Hernández-Pacheco 1919: 61, 62) two stags are shown with necks outstretched, open-mouthed, in the stereotyped "belling" posture of the rut. One (perhaps both) is shown transfixed by spears. Another stag, apparently riddled by wounds, turns his head to look back, possibly at his pursuers (Hernández-Pacheco 1919: 64). At Covalanas, a group of does is shown with heads raised and ears pricked

up expectantly. One turns her head to look behind her (Alcalde del Río, Breuil, and Sierra 1912; Apellániz 1982: 72). Although there is no indication of agitation in these figures, an attitude of tense vigilance prior to flight is perfectly conveyed.

In the case of the stags from San Román, just mentioned, the mooring polychrome bison cow on the ceiling of the Great Hall of Altamira, shown with back arched and tail upraised (Breuil and Obermaier 1935), or the engraving of a female bison mounting a male in the final gallery at the same site (Freeman 1978: 175), the attitudes depicted are stereotypical postures that characterize animals in breeding condition. In this case, the “enhancement” of the animal figures conveys information to the viewer about their condition. Since they are seasonal breeders, one aspect of this information has implications about seasonality, suggesting that the artist was concerned with, and particularly intended reference to, a specific temporal period. Of course, the season depicted need not necessarily have been the season when the work was executed.

Omission

Some figures are made more noticeable by the omission of part of the body. Jordá has described a composition of headless deer and bovids from Los Pedroses in Asturias (1977: 75, 124–26), unusual in that it includes so many figures subjected to this treatment. Several other painted caves contain one or a few headless animals. Bodiless heads are also well represented: the small black outline horse’s head from Las Chimeneas in the Castillo complex in Santander, the finger-engraved heads of deer and bovids on other panels at the same site, are familiar examples (González Echegaray 1963). Sometimes, just the forequarters or hindquarters of an animal are depicted; the former technique is also represented at Las Chimeneas as well as elsewhere.

We must distinguish between three kinds of incomplete figures, however. Deliberate omissions, such as those just discussed, must be separated from figures which were originally complete, only to lose parts by fading, leaching, or surface alteration of the rock; they should also be differentiated from figures produced by the following technique.

Shadow Completion

In this case, the figure of an animal is incompletely indicated by painting or engraving. The missing parts are supplied, or better suggested, by shadows from irregularities of the natural rock surface, under appropriate illumination. The technique is more common than is usually supposed. The dorsal line and hindquarters of a large deer or bovid at Covalanas are shown in this way (Alcalde del Río, Breuil, and Sierra 1912: plates 13 and 14). At Castillo, a fissure forms the back of a black outline bison and irregularities on a stalagmitic column form the hump, back, tail, and hindquarters of another almost sculptural bison, partly engraved and partly painted, and depicted in a strange vertical attitude (Alcalde del Río, Breuil, and Sierra 1912: plates 84–86). A bison at Tito Bustillo is formed in this way. Virtually its whole outline is

suggested by the shape of the natural rock surface, and decoration serves only to fill in detail (de Balbin and Moure 1981: plate 1). The dorsal line of a bison at Ekain is suggested by the same technique (Altuna and Apellániz 1978: photo 14B). The beak, eye, and chest of a bird at La Pasiega are also suggested in like fashion.

Sometimes, irregular projections from cave walls or ceiling suggest animal heads or grotesque “masks,” like those at Castillo and Altamira (Alcalde del Río, Breuil, and Sierra 1912: plates 85 and 86; Breuil and Obermaier 1935). At Niaux, three cup-shaped natural depressions suggest wounds on a bison’s flanks (Breuil 1974 [1952]: 192), and a cavity, “completed” with black antlers, suggests the head of a deer (Leroi-Gourhan 1982: fig. 37). Perhaps the use of rounded bosses to give a three-dimensional quality to the polychrome bison at Altamira should be considered a variant of this procedure.

The technique, wherever it occurs, couples a mastery of form and the media of execution with an admirable economy of means. Its use was extremely widespread, as I have already noted.

Caprice

This is the first of three categories of deformed figures. Under this rubric I include deformations that produce unrecognizable animals or nonexistent monsters of all kinds and hybrids. To be judged a caprice, an unrecognizable animal has to be shown in sufficient detail to permit its recognition were it a real, living creature, so its ambiguity springs not from omission of relevant detail but from distortion. The “Licorne” at Lascaux is doubtless the best-known example (Breuil 1974 [1952]: fig. 89; Bataille 1955: 30, 49, 62). I find the suggestion that this is actually intended to be a Tibetan antelope totally unconvincing—its horns project forward, not backward as in *Pantholops*, body shape and coat color are all wrong, and the chunky, square snout is totally unlike the graceful, elongated muzzle of the antelope (see Walker 1964: 1464 for an illustration of *Pantholops*).

Hybrids combine in one depiction the features of two or more recognizable animals. The most familiar of these are anthropomorphic bodies with animal-like heads, such as the so-called sorcerers at Les Trois Frères (Breuil 1974 [1952]: 164, 166) and Gabillou (Leroi-Gourhan 1965: plate 58). The three published “masks” of Altamira apparently depict bison, but each is ambiguous enough to suggest human features at the same time (Breuil and Obermaier 1935: fig. 32, plate L; Ripoll 1980: 48; Leroi-Gourhan 1965: plates 402–4). The recently discovered stone face at el Juyo (González Echegaray and Freeman 1981, 1982; Freeman and González Echegaray 1981) is a human-feline hybrid, interesting in that its two natures are laterally differentiated. The so-called ornithocephalic anthropomorphic figures that appear with relative frequency in Paleolithic art should be included in this category if they are convincingly bird-headed, but the beaked appearance of at least some of the figures, like those at Addaura (Leroi-Gourhan 1965: plate 710), may simply result from a desire to make human features unrecognizable by summary rendition of the face. The head of the anthropomorph from the pit at Lascaux is so similar to the head of the

bird on the staff or spear-thrower beside him that he, at least, must be classed with the hybrids (Bataille 1955).

Caricature

This second category includes those figures which, though deformed, are neither hybrids nor anamorphoses (see below) and remain generally recognizable though distorted. It is often hard or impossible to tell, when considering a distorted figure, whether it was simply clumsily executed or really intentionally distorted. If the latter, it may be difficult to tell whether the distortion is a manifestation of a widespread stylistic convention or a means of singling out specific figures for emphasis.

There are, however, two rules of thumb for recognizing stylistic conventions that are valid for more recent artistic products, and might be expected to have validity for Paleolithic products as well. First, a stylistic convention for the depiction of a particular creature should be similarly represented each time that creature is portrayed by artists sharing the convention. So, one might expect that when distortion is used as a stylistic convention one would find that several animals of the same sort in a site are distorted in the same ways. If that is not the case, stylistic convention cannot be absolutely ruled out, but seems a less probable explanation. Secondly, while stylistic conventions unify the members of the artistic community sharing them, they simultaneously serve to differentiate that group from others. Any artistic phenomenon that has near-universal distribution and extreme longevity is not likely to be a stylistic convention.

The frequency with which the facial features are distorted in Paleolithic depictions of humans (Abramova 1966) strongly suggests a conscious or unconscious resistance to “naturalistic” rendition of those features. Despite claims to the contrary, there is no convincing portraiture in Paleolithic art. All human depictions of the time are noticeably distorted. Human depictions thus form the clearest, most convincing set of caricatures.

The frontal view of a human face from Marsoulas (Leroi-Gourhan 1964: plate 59) is an evident caricature. Features sketchy, malformed, and “cartoon”-like, it is nonetheless perfectly evident that a human visage was intended. Certain human faces from La Marche (Pales and Tassin de Saint-Péreuse 1976: plates 5–6) and Trois Frères (Breuil 1974 [1952]) are caricatures in similar fashion, as are the human heads from Fontanet (Delteil, Durbas, and Wahl 1972).

Convincing examples of caricatured animals are not difficult to find. But, the near-universality of caricature in rendering human features makes anthropomorphic figures the clearest and most obvious manifestations of caricature as a figure-enhancement device.

Anamorphosis

This is a most interesting and potentially informative category of deformations. Anamorphosis is the systematic distortion of a figure to make it appear either unrec-

ognizable or extremely deformed, but is different from other deformations in that when examined from one particular angle or when viewed with the appropriate apparatus, such as a curved mirror, the distortion disappears and the figures resume a more naturalistic appearance (Baltrusaitis 1969; Leeman 1976; Lanners 1977: 52–55). For present purposes, cylindrical anamorphoses and others intended to be viewed with mirrors may be ignored, since there are no known examples in Paleolithic art, nor are there Paleolithic mirrors.

Two-dimensional linear anamorphoses may be regular or progressive. In regular linear anamorphosis, measurements of the figure depicted are greatly exaggerated in one dimension, compared to the other. When the depiction is tilted, to bring it more closely parallel with the line of sight, and viewed along the exaggerated dimension, the distortion apparently vanishes. In progressive anamorphosis, distortion progressively increases in parts of the depiction that are further away from the intended viewpoint. Regular linear anamorphoses will appear normal from either of two opposite viewpoints, but a progressive anamorphosis is designed to be viewed from just one position and in one direction only. The polychrome hind on the ceiling of the Great Hall at Altamira (Leroi-Gourhan 1964: plate 109) seems to be a progressive anamorphosis. The degree of distortion in the figure is relatively small, but the hind looks much more naturalistic when viewed from a position in front of and below her elongated muzzle. Among the anthropomorphic figures engraved on small objects from La Madeleine are at least two that seem anamorphic (Capitan and Peyrony 1928: fig. 30, no. 3, fig. 30 bis; Leroi-Gourhan 1964: plates 440 and 442). Several of the human heads from La Marche are convincing anamorphoses (Pales and Tassin de Saint-Péreuse 1976).

Some years ago, in a conversation with John Pfeiffer, I mentioned that I thought these La Marche faces and the Altamira hind were anamorphoses, and in his recent book *The Creative Explosion* (1982: 42), Pfeiffer extends that interpretation to the figure of a horse at Tito Bustillo and the head of a bull at Lascaux. In fact, several of the Lascaux paintings seem anamorphic, particularly the black-headed red cow in the axial gallery and the black horse with engraved outline in the nave (Bataille 1955: 73, 97). There is some justification for suspecting anamorphism whenever an animal is depicted with anomalously small or excessively elongated body parts, particularly if the body and the hindquarters are very large and robust while head and neck are elongated and small.

The recognition of anamorphoses in Paleolithic art came to me as a shock, since I had been taught that, like recognition of the vanishing point and the discovery of linear perspective, anamorphosis was an artistic innovation of the Renaissance and that the oldest preserved examples are Leonardo da Vinci's sketches of an eye and a child's face from the *Codex Atlanticus* (ca. AD 1480). There are, of course, differences between Paleolithic and Renaissance anamorphoses. Da Vinci and his successors apparently constructed their anamorphoses with a proportional grid, developed from an understanding of regular geometric constructional principles, incorporating precise mathematical rules for perspective depiction. No such rigorous system was utilized by Paleolithic artists, nor was one needed. Perfectly effective anamorphoses can

be constructed entirely by eye, by a lone artist if the decorated space is small enough, or by an artist and an assistant if it is larger. In the latter case the artist, occupying the position that the viewer will later be obliged to take, simply projects his vision of the figure against the background, and has an assistant mark a number of points that when united will determine the outline. Small, handheld slabs can be poised vertically, nearly parallel to, rather than perpendicular to, the line of sight, and kept in this position while they are decorated with a depiction that seems naturalistic from that perspective. When such decorations are viewed normally, they will prove to be anamorphoses.

Renaissance anamorphoses often distort the subject so as to render it completely unrecognizable from any viewpoint other than the correct one, while figures anamorphosed by Paleolithic peoples are always recognizable, though distorted. Nonetheless, the principle of anamorphosis, as a perspective distortion that can be normalized by the perceptual apparatus under specifiable conditions, remains the same, whether we are concerned with Paleolithic or Renaissance examples.

Pfeiffer's summary mention of anamorphosis as an odd diversion in Paleolithic art misses the point. It is more than an exotic kind of visual punning. It is an ingenious technique of figure enhancement, for a properly constructed and viewed anamorphosis seems to float free in space, at right angles to the decorated surface. Since proper viewing places the spectator in a specific position, anamorphosis is also direct evidence about the direction from which a figure was intended to be approached, and in combination with other clues provided by fixed depictions and their surroundings, may permit the reconstruction of the Paleolithic "itinerary" for viewing a series of figures. Once direction of approach is determined, the order of viewing of a sequence of figures may also be established. Last, because the figure is viewed at an acute angle, it is always presented to one side of the visual midline. This fact may tell us much about lateralization of function in the two cerebral hemispheres of Paleolithic artists and their audiences, about the kinds of "meaning" the figures were intended to convey, and about the ways in which the information they contained was processed in the cerebral cortex (Springer and Deutsch 1981: 30, 64). Anamorphosis is potentially one of the most informative techniques in Paleolithic art.

Positioning

A figure may gain in impact by being displayed in a position that dominates the spectator's viewpoint. Depictions on cave ceilings, such as the polychrome figures at Altamira, may overwhelm the spectator by their relatively large size, as mentioned above, but this effect is exaggerated by the fact that they are executed on the ceiling and force the viewer to take an awkward position, concentrating his attention on the polychromes and excluding other stimuli from the surroundings. Sometimes the viewer is "anchored" in a particular spot to view a depiction. In the so-called throne room at La Pasiega, a natural rock formation, polished by use, forms a seat. When used, this almost automatically directs the spectator's gaze to a large painted bison.

Viewing the animal figures in the cupola, or the tectiforms in the narrow fissure ending Gallery A at Pasiega (Leroi-Gourhan 1964: fig. 134), similarly fixes the spectator's viewpoint.

Framing

Some figures, even some groups of depictions, are enhanced by enclosure within a natural frame. They are thus separated from other figures and, when illuminated properly, stand out from their surroundings with striking clarity. Perhaps the best known example of this technique in Spain is to be seen in the "Camarín" in the Peña de Candamo. The Camarín is a cavity with an ashy bluish background bordered by stalactitic formations. Several figures populate the cavity, but most impressive is a horse, in sienna, that contrasts markedly in color with the background. When the Camarín is illuminated from inside, its figures suddenly, magically appear "outside" the dark cave environment as though one were looking out to a sunlit exterior, and the horse almost seems to come alive (Hernández-Pacheco 1919: 51, 52, plate 10; Gómez Tabanera 1975). The effect must be seen to be believed.

Discovery

The sudden discovery of a depiction which is hidden by obstacles from all but one particular point of view can greatly augment the impact the figure causes. The final corridor in Altamira is so narrow, tortuous, and low that the visitor must shuffle along, often bent nearly double, continually glancing from the ceiling and walls of the gallery to its floor to avoid blundering into a projecting rock, slipping, or stumbling. At one point he must pick his way over and around several large fallen blocks, as the tunnel veers left. Suddenly, within inches of his face, a grotesque mask, half-animal, half-human (Leroi-Gourhan 1964: plate 403), pops into view at eye level on the left. The experience even startles those who are intimately familiar with the gallery, if they come upon the mask while distracted. To the neophyte, it can be nearly heart-stopping. The trick is obviously intentional—it is repeated in similar fashion several times in the final gallery.

Counterpoise (Counterposition)

Any of the techniques listed earlier could serve perfectly well as a means of adding impact to a single isolated figure. Counterpoise and all the techniques that follow require multiple figures or a combination of a figure and a scenic ground for their execution.

In counterposed depictions, similar figures are opposed to one another in a balanced composition. The similarity displayed may be one of color, size, "style," or kind of animal represented. This technique is treated somewhat differently by Leroi-Gourhan (1982) in his discussions of "symmetry" and "partial overlapping." The red-brown cattle on opposite sides of the axial gallery at Lascaux (Bataille 1955: 76)

are a fine example of counterposed figures, one of which is further counterposed, head to head, to another red bovine on its own side of the gallery. In the main hall at the same site, the opposition of two large black and white bulls on the “left” wall and the two bison shown tail to tail in the nave are similar cases (Bataille 1955: 46, 50–52, 105), as is the pair of polka-dot horses on the large panel at Pech-Merle (Lemozi 1929; Breuil 1974 [1952]: 270–71). At Le Portel, a pair of bison confront each other in such a counterposed composition (Breuil 1974 [1952]: fig. 216). The number of other similar cases is too numerous for detailed discussion of this point.

The spatial symmetry of a counterposed composition need not, of course, be absolutely perfect, nor do the animals or signs depicted need to be absolutely identical.

Complementarity

This technique is similar in some respects to that of counterpoise, except that here, instead of having a spatially symmetrical repetition of figures, an element of dissimilarity is injected. Moreover, the spatial arrangement of complementary figures need not be symmetrical at all; the figures need only be close enough to one another to be grasped by the viewer as related, though their complementarity need not be immediately perceived. In some cases, complementary figures are united by a relatively evident theme—that is to say, a theme that would have been recognized by prehistoric hunters familiar with the behavior and stereotyped attitudes of the wild animals they pursued. Though rare, there are some Paleolithic depictions of sexual complementarity—males and females are occasionally shown together, in stereotyped courtship postures. One convincing example is the pair of bison from the final gallery at Altamira, mistakenly identified by Breuil as a paired mammoth and bison (Breuil and Obermaier 1935: 85; Freeman 1978: fig. 4). Denise de Sonneville-Bordes reports an example in the frieze of horses from la Chaire-à-Calvin (1963: 187–90, plate 9). Several decorated bone and stone objects from La Madeleine and one from Abri Morin depict paired animals or heads, sometimes different enough in size to suggest sexual dimorphism (Capitan and Peyrony 1928: figs. 20, 38, 54, plate 15; de Sonneville-Bordes 1975: figs. 27 and 28). In the Morin case, a bison family of male/female and calf seems to be depicted.

Annette Laming-Emperaire (1962) and André Leroi-Gourhan (1964, 1965, 1982) are to be credited for having called attention to another kind of complementarity in Paleolithic art. This is the statistically detectable relationship between different animal species and between specific animals and specific “signs,” more particularly the complementary opposition of horses on the one hand and wild cattle or bison on the other. Leroi-Gourhan notes that this opposition may be underlined by the relative size and number of depictions of the kinds of animals involved, the dominant type of creature in a panel being stressed by the average size or abundance of its representations (1982: 60). Other kinds of animals are often shown in marginal positions near groups of these fundamental species and still others are often hidden away near the ends of assemblages or in nearly inaccessible interior corridors, in even more

peripheral situations. Such observations as these have made Leroi-Gourhan's work the central stimulus to the study of spatial and numerical relationships of Paleolithic parietal figures in recent years (this chapter is, of course, just one more in the series of offshoots from that trunk). Leroi-Gourhan's thesis, and the documentation provided by his sketch-maps and the superb photography of Jean Vertut, are too well-known to need further discussion here.

Repetition

There are a number of cases in Paleolithic art of the repeated depiction of figures of the same "kind" (whether animals, anthromorphs, or geometrics) in close proximity on a single panel. The fact that the polychrome figures on the ceiling of the Great Hall at Altamira are broadly similar in style and technique of rendition (though certainly executed by several artists) as well as size (between one and a half and two meters in maximum diameter), position, and orientation (the reclining figures all with heads in the same direction, backs more or less perpendicular to the backs of the standing bison nearest them, the ground line for the standing figures more or less in the same general direction, rather than random, and never completely opposed), and the lack of overlap or superimposition of adjacent figures are the strongest possible arguments that the Paleolithic artists intended them to represent a single integrated composition whose impact would be reinforced by sheer repetition. I have argued that at least two of the so-called wild boars in this group are mistaken identifications of other bison (Freeman 1978: 168–71), and that opinion has been accepted by others including Moure (1981) and Apellániz (1982: 55). The reclassification of these figures makes repetition even more important to the composition, of course.

In this case, repetition is an incentive to the viewer to search further for a unifying meaning of the figures. The makeup of the group of bison, incorporating as it does both adult males and females, indicates that the artist intended to depict a herd of bison at the season of the rut (Freeman 1978). This meaning has been overlooked by scholars who have treated the animals as so many unrelated individual depictions.

Some examples of repetition in Paleolithic art are even more obvious. The line of little horses on the right wall of Lascaux's axial gallery or the five stags' heads along the right wall of the nave, postures suggesting they are swimming in a stream indicated by a linear discolored patch on the wall into which their necks merge (Bataille 1955: 69, 95); the sculpted friezes of horses at la Chaire-à-Calvin (de Sonneville-Bordes 1963) and Cap-Blanc (Roussot 1965, 1972); the painted friezes of mammoth and rhinoceros in the Breuil Gallery at Rouffignac (Nougier and Robert 1959; Leroi-Gourhan 1965: figs. 535 and 536); that of bison at Font-de-Gaume (Breuil 1974 [1952]: fig. 39; Leroi-Gourhan 1965: fig. 527); and the economically depicted herds of reindeer and horses on small bone objects from Chaffaud and the Grotte de la Mairie at Teyjat (Barandiarán 1972: 345) sufficiently illustrate the popularity of repetition as a figure-enhancement device.

Such scenes are so widespread and so clearly compositional that one wonders how so many authorities before Leroi-Gourhan could have glibly and totally denied the existence of true compositions in Paleolithic art (Hoernes and Behn 1928: 44; Pittioni 1949: 60; Clark 1961: 56; Clark and Piggot 1965: 92; Hawkes 1963: 197).

Outlines of human hands are also frequently massed together, as, for example, at Castillo (Alcalde del Río, Breuil, and Sierra 1912: plate 65). At Pindal, sticklike figures, some ending in hand shapes, are arranged in two groups, one above the other (Alcalde del Río, Breuil, and Sierra 1912: plates 24 and 27). The occurrence of other kinds of geometric signs in masses of repeated shapes, as in the gallery of discs at Castillo, the dots at Trois Frères, the group of teardrop shapes before a fish at Pindal, the series of six claviforms beneath a bison at the same site, or the repeated tectiforms at Pasiega and Castillo, is more the rule than the exception (Breuil 1974 [1952]: fig. 124; Alcalde del Río, Breuil, and Sierra 1912: plates 39, 43, 69, 77, and 78).

In some cases, figures are repeated over such a large area that the grouping cannot be viewed as a whole from any single place; such series more properly exemplify the next technique.

Progression

Progression also involves the repetition of similar depictions, but ordinarily the repeated figures are separated from one another by some distance, so that instead of being grasped nearly at once, the series of similar figures is revealed by degrees. Recognition of the similarity of separated figures sharpens the viewer's attention, arouses the anticipation of other related depictions, stimulates a more active scanning of the visual environment, and creates suspense. It is the same device used in so many horror films, to such effect.

The masks in the final gallery at Altamira are a superb example of the successful application of the technique; the discovery of six previously unreported masks in 1981 brings the total of these figures to nine. The first examples encountered after entering the gallery are a pair of masks that confront each other across the narrow corridor. However, the left-hand mask is hidden from the entering visitor's view. After negotiating a hairpin bend and some meters of undecorated corridor, another mask springs into view on the visitor's right. A few meters further on, another hybrid visage peers out on our left. Then one comes to a widening in the corridor, where two masks are visible on the left (though, like the last, these are better seen on the return). As the passageway narrows again, another mask is seen on the right. On exiting through the wide chamber, the visitor sees two more masks that were previously hidden and, just before leaving the corridor entirely, he finds the formerly invisible member of the opposed pair before him. The mysterious quality of these three-dimensional faces, half-animal but still eerily human, is sufficient by itself to awe the viewer.

Combined with the technique of discovery, the masks appear as startling and monstrous apparitions. Suspense built by the use of progression can make a visit to this gallery a not-to-be-forgotten adventure. On leaving the constrained environment

of the final gallery, one continues to fancy that still more visages lurk among the irregularities of the cave in other areas (in fact, some probably do).

In the preceding section I mentioned that some continuous (or discontinuous) repetitions of signs extend for very long distances, as at the gallery of discs at Castillo (Alcalde del Río, Breuil, and Sierra 1912: figs. 109 and 110, plate 69). Sometimes, similar geometric forms are repeated where there are important projections (as at Pindal), or at the entries of narrow, or dangerous, or important passageways (Alcalde del Río, Breuil, and Sierra 1912: 69). Such uses of progression seem to me different from the cases discussed earlier. One wonders whether these may not be “indexical” signs, like danger signs or direction indicators along a highway. (That interpretation has been suggested independently by other authorities.) Only a small amount of additional research would be needed to establish or reject the suggestion for particular caves on empirical grounds.

“Landscaping” (Use of Natural Formations as Scenic Supports)

Natural formations on cave walls and ceilings are frequently incorporated into compositions as scenic devices. The case of the San Román “Camarín” has already been discussed. Probably the commonest device of the type is the use of a natural ledge, crack, concavity, or discoloration to suggest a ground line, along which animal figures walk. The technique is used at Font-de-Gaume, Lascaux, Rouffignac, and elsewhere (Leroi-Gourhan 1982: 27–28, plate 69). Sometimes the naturally suggested ground line is tilted rather than horizontal, and the animals are tipped to follow the slope—as, for example, at Las Monedas and Le Portel (Ripoll 1980; Leroi-Gourhan 1965: plates 64 and 66).

More unusual and more striking is the use of the edges of voids produced by cracks, hollows, and corners to suggest cliffs over which animals fall, or pits into which they tumble. A bison at Monedas slips into a crack (Ripoll 1980: plate 10). At Lascaux, a horse at the far end of the axial gallery seems to fall hindquarters first into a concavity suggesting a cliff edge (Bataille 1955: 81, 89), and in this case the part of the figure that disappears over the edge was never completed. The technique adds considerable tension and movement to compositions.

Sometimes, a negative, empty space is inventively used to suggest a positive obstruction from behind which a figure emerges. At Las Chimeneas, this is suggested by a horse’s head that appears as though from behind a shadowy rock (González Echegaray 1974: plate 22). There is a very similar interplay of isolated horse’s head and void at Rouffignac (Nougier 1966: plate 15).

Concavities and voids formed by irregular limestone surfaces also seem to have been an inordinately frequent stimulus to cave occupants to execute other kinds of manipulative activities. At Niaux and Chufin, cavities have been outlined with red coloring material (Leroi-Gourhan 1982: plate 112; Almagro 1973). In the final gallery at Altamira, some fissures have been packed with gobs of clay, and more clay smeared around them. This enigmatic behavior would suggest rather obvious interpretations to a psychoanalyst of the Freudian school. Though they would probably

recoil from being called Freudians, many prehistorians agree that a sexual interpretation is appropriate for these phenomena. The behavior responsible is not related to the “scenic” use of voids.

Embedment

Suitable limestone surfaces in caves have sometimes served as the canvas for generations of artists. Decorated panels may be palimpsests of layer upon layer of superimposed figures. Prehistorians often assume that much time must have elapsed between successive superimpositions. But that need not be the case. In portable art objects, we know of the existence of engraved cobbles, stone slabs, or bones with many intricately superimposed figures, that most would agree were probably produced over a short period of time: cobbles from Colombière, slabs from La Marche and Parpalló, scapulae from Castillo, to cite just a few examples (Leroi-Gourhan 1965: plates 480–85; Pales and Tassin de Saint-Péreuse 1976; Almagro 1976). In these cases, to see a particular figure the viewer must often work through an elaborate visual puzzle of crossing, interpenetrating lines and unrelated depictions in which the figure is concealed and embedded.

The modern human brain seems to be stimulated positively by such exercises: the search for and eventual recognition of isolated depictions is rewarding in itself; the perceptual game is self-motivating. There is no reason to believe that such activity was beyond the mental capacity of the skeletally modern Upper Paleolithic populations who are our near ancestors or close relatives. To the contrary, Upper Paleolithic palimpsests would seem to show that those populations enjoyed the perceptual exercise as we do.

The cave walls contain wonderful examples of apparently chaotic masses of interwoven lines that conceal individual figures of animals, humans, and hybrids. The viewer may stand for hours, enthralled by some of the more labyrinthine entanglements, working to disengage individual figures from the background of overlain webwork that hides them. There are fascinating panels of this sort at Trois Frères, Gargas, Pech-Merle, and Lascaux (Breuil 1974 [1952]: 160–77, 254–57, 273; Vialou 1979). Such exuberant panels are quite common. Enigmatic panels like that at Las Monedas, containing no recognizable depictions, nevertheless stimulate the viewer to project imagined forms into the disordered array (Ripoll 1980: plate 25). While it is generally supposed that such palimpsests as those from Trois Frères resulted from the repeated redecoration of a single panel at widely separated intervals in time, there is absolutely no proof of the assumption, and no inherent reason why the delineations could not have been produced over a very short time instead, *pace* Marshack. Whatever the history of their accumulation, their effect on the viewer is in any case the same.

DISCUSSION

I have identified and discussed a list of 17 special conventions in this chapter. I have no doubt that aspects of my list are not completely satisfactory, nor do I doubt that

many other conventions like these may exist. I hope that the study will provoke further discussion and investigation of these techniques, and will stimulate the refinement of the list or its entire substitution by other, more appropriate, formulations.

Ucko and Rosenfeld's thorough, scholarly treatment of cave art (1967) largely demolished facile and simplistic notions about its motivations, and dampened the ardor of armchair theorists for universalistic explanations for its production. At the same time, though I am sure this was as unintended as it is unfortunate, their work seems to have slowed the search for empirical information about more concrete and particularistic meanings and motives of specific artistic assemblages. There is nothing in their book that compels such reluctance, and in fact Ucko himself did not abandon the quest. True, we may never thoroughly understand any artistic assemblage or why it was produced, but the careful and objective search for such understanding is incumbent on any prehistorian who deals with Paleolithic art. To conduct a responsible search for particular motives and meanings, we must examine the depictions and their settings and relationships more thoroughly than ever. That requires a more complete and rigorous analysis of themes depicted, materials utilized, techniques of execution, artistic conventions, and styles than has been produced heretofore.

The special conventions just discussed single themselves out for our particular attention. Where they occur we cannot help but be struck by the fact that the figures involved have somehow been elevated above the common level of the mass, as underlined or italicized words stand out from a text. The special stress placed on these figures may indicate that they bear a central part of the meaning of the panel or assemblage, that they recapitulate it or condense it. Of course, they may prove to have no such focal importance, but it would be irresponsible not to consider that possibility at all. The soon-to-be-published results of new research at Altamira suggest that there, at least, the techniques of isolation, size contrast, attitude, omission, shadow completion, caprice, anamorphosis, positioning, framing, discovery, counterpoise, repetition, complementarity, and progression are all used (often in combination) to enhance figures with special relevance to a set of interrelated themes that pervade the cave, that may now be recognized as central aspects of "meaning" of the art at Altamira. Since the conventions for figure enhancement are so many and varied, it may be suspected that the conventions themselves may have differed in their meaning to the artists and their contemporaries, and that the choice of one set instead of another equally practicable set is a significant datum.

There is no doubt that specific conventions convey particular kinds of information to the modern analyst. The study of unusual attitudes has already suggested that depictions in certain sites have seasonal significance, and reflect an interest in the reproductive condition of selected species (Freeman 1978). The techniques of omission and shadow completion and the use of natural formations as scenic support show the extent to which Upper Paleolithic people's mental processes, like our own, tended to supply the missing parts of familiar percepts, simplifying and reshaping complex or irregular forms to construct recognizable figures. In both anamorphism and discovery, the figure is first presented or presented in most recognizable

form to a particular portion of the visual field. Examples of such figures designed to be presented to the left side of the visual midline seem to me to be more common than others presented to the right of the midline, though I have still made no attempt outside Altamira at the systematic collection of empirical data needed to verify this impression. Among living peoples, where information is presented directly to the cerebral hemisphere best specialized to process it, response is quicker and more accurate than otherwise. Holistic recognition of complex visual stimuli is accomplished most rapidly and efficiently when the data are presented on the left of the visual midline, directly to the right hemisphere. Where a startling effect was desired, grotesque hidden figures popped into sight on the viewer's left, so that the complex visual data they presented would be channeled directly to the right hemisphere for immediate holistic processing. On the other hand, presentation from the viewer's right to the left hemisphere should facilitate logical consideration of symbolic content. If the stimuli of the depictions in particular caves were predominantly designed to fall on one side or the other of the visual midline, it would tend to suggest that cerebral hemisphere function among Upper Paleolithic artists and their audiences was differentiated as it is among living peoples. (Even placement that simply reflects handedness would bear on the subject.) Sets of symbols and whole sites could be compared and contrasted on this axis. The potential behavioral information provided by such data is absolutely fundamental to any reconstruction of the evolution of mental processes. Art is almost alone as a domain where durable products of prehistoric human behavior have immediate and unequivocal relevance to studies of psychological development.

Attitude, anamorphism, discovery, complementarity, and progression are among the techniques whose study can lead to the recognition of sequences of figures that can only be properly viewed in a particular order. In rare favorable cases, such as the narrow gallery at Altamira, it may even be possible to reconstruct the itinerary Paleolithic visitors must have followed to traverse a gallery while viewing its decorations. When a long viewing sequence is established, the order of the figures may provide other clues to the meaning of the series as a whole. Certainly the series in its interrelated entirety offers more information to the analyst than the individual figure considered alone.

Other aspects of the use of enhancement techniques deserve further study. The details of the application of particular techniques to particular kinds of figures need to be thoroughly analyzed. For example, while bird/human hybrids are apparently common, I know of no bird/mammal hybrids in Paleolithic art, a fact also remarked by S. Giedion (1981: 61). Relationships between species, their complementarity, opposition, or equivalence, all factors fundamental to the theories of Leroi-Gourhan and others, might be manifest in the merging of particular animals in hybrid forms. Further study might also show that particular techniques of figure enhancement were only applied to specific kinds of figures—one or a few animal species or some set of geometrics—rather than to all indiscriminately. Relationships between sites or galleries within a site might be indicated by similarity in enhancement techniques and their application. Sites, periods, panels, or galleries might be classified and compared

on the basis of the categories of figure-enhancement devices represented, their applications, and frequencies.

One final illustrative example of the utility of this approach may be given. At Altamira, engravings in the final gallery can be seen to be related to the polychromes on the Great Ceiling, with respect to species represented and the ways in which size contrast, omission, positioning, counterpoise, repetition, and complementarity are employed. But, enhancement techniques are not used in the same ways in the two galleries. In fact, size, counterpoise, frequencies of repeated figures, complementarity, positioning, and omission are applied to the final gallery in ways just the opposite or inverse of their application on the Great Ceiling. Here, relatedness seems to be indicated by complementary opposition rather than identity.

Enough has been said to indicate the potential of the study of this material. Perhaps it is not the most fundamental substance we might examine. It may prove to be the case that more basic aspects of meaning, at least at the iconic level, are borne by the choice of animals or forms represented. But, the enhancement techniques described undeniably serve to stress and structure that meaning. The study of structure and stress can clarify systems of meaning in ways that the study of single meaning-bearing elements cannot.

Meaning in Paleolithic depictions probably always exists at several levels, of which some (the levels of more arbitrary meaning) may be completely closed to, and others very difficult of access to, the modern analyst. I suspect that for the most symbolic levels of meaning, we are constrained to recover only evanescent glimpses of the information encoded in Paleolithic art. However, it seems clear that if any reception or decipherment is ever to be possible, all relevant aspects of the information-bearing attributes of the data must be taken into account. Enhancement techniques are surely an important and neglected part of the message, and one that merits much closer and more critical study.

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The Cave as Paleolithic Sanctuary

Before embarking on a somewhat speculative discussion of the nature of Paleolithic art and its environment, I should like to qualify what I am about to write. First of all, we ought never to attempt to explain what we do know—the Paleolithic decorations themselves—in terms of something we cannot know or do not know, such as their supposed religious/magical significance. It seems evident to me that Paleolithic art must be approached empirically. We must try to understand it in its own terms. What seem to us to be logical meanings or connections of figures are probably quite unlike the meanings and connections the Paleolithic artists saw. Nor does getting free from our own ethnocentrism to draw on ethnographic analogy—in this case the study of the ways in which living non-Western peoples such as Bushmen, Australian Aborigines, or Eskimos, make, use, and interpret their art—clarify the purposes of artistic production and interpretation that characterized the very different cultures of the Paleolithic past. All of today's societies, hunter/gatherers as well as urban capitalists, have equally long histories of adaptation to the changing world around them. None has remained static and frozen in time. All cultures are parts of ecosystems that are constantly in flux, so the idea that an adaptation made long ago could be preserved unchanged for millennia is an absurdity. There are no living Paleolithic peoples. They all vanished some 10,000 years ago, and no modern people are their

living representatives. The Paleolithic cultures are extinct, and consequently, it is highly likely that many of the ways they thought and behaved have vanished without trace, being replaced by newer ways that conveyed some advantage as living conditions changed. Despite these cautions, I believe that there is utility in the postulate that at least some Paleolithic caves, including some decorated Paleolithic caves, served as sanctuaries. But I repeat that my comments are not intended to be an explanation of Paleolithic art; they are rather my own interpretation of some of its documents and their occurrences.

The pioneers in the study of Paleolithic art—the Abbé Breuil, Max Raphael, Annette Laming, André Leroi-Gourhan, Herbert Kühn among them (Breuil 1952: esp. 52–53; Raphael 1945; Laming-Emperaire 1962; Leroi-Gourhan 1964, 1965; Kühn 1956a, 1956b)—were uniformly in agreement that many or most caves containing Paleolithic depictions, or at least some portions thereof, are prehistoric “sanctuaries.”¹ As Siegfried Geidion succinctly put it: “the depths of the caverns were holy places where, with the aid of magically potent symbolic pictures, sacred rituals could be performed” (Geidion 1962: 525). However, none of those authors defined in any further detail what they meant by the term “sanctuary,” nor have they told us what kinds of archeological remains a sanctuary might leave for the prehistorian to recover. It is true that many modern visitors experience feelings of unease and reverential awe while viewing incredibly ancient decorations in the depths of a cavern, but those may simply be a reaction to the unfamiliarity of the experience, or the prior expectations of the visitor, and are not in themselves sufficient reason to believe that all caves where such feelings are experienced were truly prehistoric sanctuaries.

■ SANCTUARY: A DEFINITION

Let me define a sanctuary as a “holy place”: a symbolically structured space set apart from the routine activities of daily living, and dedicated to the performance of activities that establish culturally patterned interactions with a culturally postulated supernatural,² including activities intended to promote the inculcation of, and reflection by the communicant on, culturally patterned beliefs about the relationship between the natural world and the culturally postulated supernatural. In the case of the Paleolithic caves, the symbolic alteration of the natural space that makes a sanctuary may be the deliberate construction of buildings and features, or decoration, or both. However, these alterations must be “out of the ordinary”—that is, both unusual and inexplicable in terms of workaday routine.³ Usually the fact that sanctuaries were set apart from the (other) routines of daily life is suggested by the absence in them of the wastes and discards of more strictly economic activities, but contemporary or later reuse of sanctuaries may invalidate this criterion, so that need not always be the case, and more evidence than that, in the form of intentionally patterned remains of other types, is required. The phrase “culturally postulated supernatural” is meant to include conceptions of those powerful “natural forces” that are thought to control the survival and prosperity of humans as individual members of social groups,

and the resources on which they depend. So, particular sanctuaries need have no relationship to concepts of divinity or spirituality sustained by mainstream modern religions; they can be the loci of operations designed to establish or maintain a positive balance of relationships between humans and those natural forces.

Be aware that we do not suggest that all caves with Paleolithic levels, or all caves with Paleolithic wall art, are sanctuaries in this sense. The simple-minded equation of painted cave and sanctuary must be rejected. Decorated cave sites may have served a great many purposes. And those purposes must have changed over time. As a modern example of such change, the church of Hagia Sophia, once a Christian sanctuary, was transformed first into an Islamic sanctuary, and then into a museum. The cave of Altamira, too, is now a museum, which was certainly not its function when its remarkable polychromes were produced. The identification of places that may have served as sanctuaries must rest on solid evidence of their extraordinary character, and its relationship to behavior of the time, rather than on the ill-defined feelings of awe that they produce in today's viewer.

Just as the painted caves undoubtedly had multiple and sometimes changing functions, so too did the depictions themselves. It is certainly not the case, as many think Breuil suggested, that *all* of Paleolithic art was produced by specialized groups of dedicated and trained "professional" artists and ritual practitioners. The inexpert quality of some depictions in some galleries suggests that they might have been produced playfully, by persons who were not trained artists.⁴ Some decorations may simply be means of "domesticating" space: of turning the apparently chaotic and threatening natural cave environment into culturally organized space, whose formerly confusing galleries are now made comfortingly familiar by the presence of familiar figures on the walls and ceilings. Cave decorations illustrating the nature and behavior of the major prey animals, or suggesting how to deal with predators, had educational value, and made it possible for those who had successfully dealt with problematic or dangerous encounters to share their experiences with others in vivid fashion. Partaking with others in the production or viewing of cave art might have served several purposes. It was a socially integrative mechanism, reinforcing group solidarity. By creating a symbolic environment where the spatial separation between hunters and their prey was denied, it gave the illusion of control over nature, thus allaying anxieties that the hunt might be unsuccessful.⁵ When the hunt did fail, producing and viewing the art provided a supplementary source of gratification, relieving individual and group anxieties and restoring the hunters' confidence in their prowess and eventual success. When the hunt was successful, communal rituals centered in the painted caves might have served to reintegrate successful hunters with their communities, restoring the balance of nature, perhaps atoning for the violence of the hunt, and giving thanks to the prey for their voluntary sacrifice, and by providing all communicants a feeling of collective participation and accomplishment, reducing the envy of those less successful or less competent members of the group. We should also bear in mind that overpainting or superimposition of figures, or the imposition of meandering lines over earlier depictions, may not have been done by the group that originally produced the decorations. It may instead indicate an

attempt to alter and reinterpret, or deface and obliterate, figures originally produced by a group that used symbols that differed from and were considered foreign to, or even opposed to, the values of a later group of decorators.

Let me proceed to demonstrate that some Paleolithic sites do contain true sanctuaries as I have defined them, using concrete examples drawn from my own research experience in Cantabria. In the last thirty or so years, careful excavations, combining scrupulous vertical control and the exposure of relatively large expanses of single occupation levels in some Paleolithic sites, have shown that contrary to usual preconceptions, the different sectors of many archeological levels are neither chaotic nor random in contents, nor were the activities performed in prehistory conducted uniformly (or equally) in all parts of a site. This should not be news to anyone, but its implications invalidate the traditional idea that *any* sample of a hundred or more tools from a particular level will be characteristic of all the artifacts contained in the level, and adequate (as good as any other) for the assignment of the assemblage from that level to a particular Paleolithic industrial complex. It also contradicts the traditional belief that any difference between assemblages is mostly due to the evolution of industrial complexes over time, or (in the case of the Mousterian facies, for instance) to the differences between the assemblages that characterize distinct identity-conscious socio-cultural groups—tribes or what have you. The different areas in a single occupation site can be shown to have had different functions, and some of those areas are appropriately called sanctuaries.

■ THE EARLY AURIGNACIAN MORTUARY COMPLEX AT CUEVA MORÍN

As a first example of a level containing a Paleolithic sanctuary, let me describe an Early Aurignacian occupation at Cueva Morín. There, in one and the same level, there was a hut foundation with fireplace and cooking debris, separated, by a screen wall supported by wooden posts, from a distinct area dedicated to the burial of deceased members of the group.

It is the nature of the structures in this area and their contents that indicate it to have been a sanctuary; it lacks artistic decorations. Southwest of the posthole alignment, between it and the cave wall, we found in 1969 two excavated tombs, each covered by a mound of dark earth. Part of these mounds had been removed by excavations earlier in the twentieth century. The best-preserved tomb complex consisted of a trench containing the three-dimensional soil shadow or pseudomorph in fine sediment of a tall Aurignacian human, probably male. The pseudomorph had its arms bound to its chest, and its head and feet were severed from the body—the head was found in the grave beneath the rest of the body. Atop the upper body we found the pseudomorph of a small ungulate, and what seems to have been a slab of animal ribs was deposited on the legs before burial. Next to the grave proper, at the level of the thighs, there was a small pit containing burnt bone fragments and red ochre. An earthen mound covered the tomb, and during its construction a small fire, intense enough to redden the underlying mound fill, had been lit atop it—it

too contained burnt bone. This hearth was covered by another layer of mound fill. In the process of digging this grave, designated Morín I, the prehistoric cave residents destroyed the remains of an earlier burial (Morín III), whose legs, intentionally truncated and charred, were found in the base of the trench containing the later burial. To the south of the tomb of Morín I was another smaller grave, Morín II, but the organic material found in this one was a formless deposit of a rank-smelling buttery substance rather than a pseudomorph. Beside this grave, near its narrower end, there was another “offering pit” filled with burnt bone and ochre, and this one communicated with the interior of the trench by means of a narrow tube (see also González Echegaray et al. 1973; Freeman and González Echegaray 1973; González Echegaray and Freeman 1978). We interpreted the mutilation of the two cadavers of Morín I and Morín III, and the presence of food offerings in and near the graves, as indications of concern for the possible survival of the physical form after death, while we think that the location of the burial complex, in close proximity to the hut foundation, shows that the deceased ancestors were thought to continue as part of the social group, but with a change of status. Clearly, the physical remains of the deceased became unimportant after a short time, as witnessed by the callous disturbance of Morín III to bury Morín I. Morín, of course, is not unique. Elaborately treated burial precincts are known from other Paleolithic sites. But Morín provided a wealth of detail that is often missing from the reports of earlier excavations, which makes it an especially convincing example. In several respects, the Morín burial complex has all the earmarks of a sanctuary: it was used for purposes apart from daily routines, and was the scene of performance of elaborate rituals that must have some connection with a supernatural world. Yet the Morín mortuary area belongs to a very special subset of “holy area.” It is more than just a cemetery as an area for disposal of bodies of the dead: it is their temporary repository while their material substance dissipated, the site of ritual treatment to facilitate their transition from this life to the afterlife, from the natural world of the living to the supernatural world of the ancestors.

■ THE SANCTUARY AND FACE AT EL JUYO

The second case concerns finds at our more recent and extensive excavations at the cave of el Juyo near Igollo. In the latest Cantabrian Earlier Magdalenian occupation at this cave site (Level 4), Dr. González Echegaray and I found a large structural complex, containing curiously patterned debris of a highly unusual nature (see, e.g., Freeman and González Echegaray 1995).

This complex consists of a nearly 2-meter-deep semicircular dugout depression, some 6.5 square meters in area, whose straight side opens to the northwest. Its curved side is walled with a lining of small fragments of limestone, often held in place by a mortar of clay. Along its northern portion, the west side of the structure is partly closed by a large flat limestone slab. Inside the dugout we found three smaller trenches, each filled and covered by elongated mounds. The fill of the trenches and the mounds consisted of from four to seven pairs of alternating levels of (1) usually

reddened earth, ash, and elongated goods such as spear-points and the atrophied lateral phalanges of deer (“offering levels”), deposited paralleling the long axis of the mounds; (2) other layers of mottled earth partly brought into the cave from outside, partly dug from underlying occupation levels in the cave, which were deposited as cylindrical lots, almost all of them ca. 10 centimeters in diameter; these lots were usually arranged in groups of seven (“fill levels”). The cylinders of fill—there were apparently more than 1,200 of them—appear to have been carefully deposited from thin-walled cylindrical containers. In some areas they were easy to detect because adjacent cylinders contrasted in color; in others their presence was betrayed by differences in texture and the vertical orientation of included artifacts along the sides of the containers. Associated with these mounds were four small pits containing mollusk shells and other objects, especially eyed needles, and one of the pits contained the masterful cutout contour of a hind’s head; the complex also included two accumulations of fossil shells, each containing a single whistle made of a hollowed nodule of iron oxide. And, set into a wall made of clay and large stones, facing the cave entry so as to dominate the whole complex, was the large ($35 \times 32 \times 22$ centimeters) stone face or “mask” of a hybrid being, divided along its midline into two sides, its proper right a human face with beard and moustache, its proper left a large cat, probably a cave lion. The last rays of the setting sun would have entered the cave to strike the face on what would have been the summer solstice 14,000 years ago. As a final act before its Magdalenian occupants abandoned the cave, never to return, the complex, mounds, face, and all, was covered by a pavement of stones, including one huge flat slab measuring 2 meters \times 1.2 meters \times 15 centimeters, and weighing about half a ton, that seems to have been carried some distance from its source. Two shallow hearths encrusted in this pavement were aligned with the top of the stone face, perhaps as a cryptic indication of its location. This would seem to indicate the great importance of the precinct in the system of beliefs and behavior of the cave occupants, and their desire to conceal and protect it during their absence. We consider there to be sufficient reason, from the lack of evidence of routines of daily life within it, the strikingly anomalous nature and relationships of the materials it contained, and the elaboration and painstaking detail with which its structures were built and finally hidden, to call this complex a sanctuary, and the face the representation of a supernatural being with central significance to the beliefs and activities focused on the sanctuary. (Curiously, though the nature of the sanctuary, its orientation, and the character of the face suggest beliefs and rituals that are associated with death in more recent periods, there is no direct evidence that human remains played any part in whatever rites were performed there.)

These two cases should be sufficient to demonstrate that precincts that are legitimately called sanctuaries do exist in Upper Paleolithic contexts. The third and last case I shall discuss, and the one most relevant to this volume, is that of the decorated cave of Altamira (Freeman and González Echegaray 2001). I shall try to show from the nature and organization of its depictions and archeological deposits that the term “sanctuary” is appropriately applied to this cave and its decorated areas, as well.

■ ALTAMIRA: THE POLYCHROMES ON THE “GREAT CEILING”

The first piece of relevant evidence is that there is no significant occupation debris in the painted precincts at Altamira. Certainly, a small number of tools and other residues have been found below the Great Ceiling, as elsewhere in the cave. But the true occupation levels, with more than a small scattering of objects, are restricted to the vestibule nearer the cave entry, an area a few meters from the paintings, and any material found beneath the painted ceiling could have been accidentally scuffed there from the intensively occupied vestibule. There is no doubt that routine economic activities such as cooking and waste disposal took place in the vestibule, so the absence of any accumulation of such living debris under the paintings is significant. The use of the decorations as a cultural means of organization of unfamiliar and confusing space, a plausible motive for the decoration of some caves, is very unlikely at Altamira, since there is no conceivable way for a visitor who has enough light to see the figures on the Great Ceiling—or elsewhere, for that matter—to become disoriented.

The decorations in true sanctuaries are known to reflect a society’s most fundamental beliefs and values. Consequently, they are not to be displayed in haphazard fashion, but are systematically organized. There is overwhelming evidence that the nature and placement of the famous polychrome figures on Altamira’s Great Ceiling are not random, but obey strict principles of planning and organization. The evidence is the following. Obviously, all the figures are polychromes, a fact that by itself makes them stand apart as a stylistically and technically homogeneous group. All the bison are depicted as of approximately the same size, between one and a half and two meters in maximum dimension, and very much larger than all but one of the other figures on the ceiling and walls of the chamber. There is unity in subject matter, as well, since all but three of the figures are now correctly identified as bison, and those three (deer and horses) are symbiotic species, creatures whose ranges overlap with each other and with that of the bison, so that they can and do share the same habitat. In position and arrangement the figures form a patterned composition. Modern viewers of Western art are used to the idea that a composition should have a “ground line,” atop which the feet of all standing figures should rest. While there is no ground line at Altamira, nevertheless the legs of all the standing animals are all oriented in the same general direction, more or less southward. None is either “upside down” or even “perpendicular” with reference to the other standing bison. That is equally true for the hind and the standing horse. The reclining or wallowing animals, all bison, are all “perpendicular” to the standing animals, and all have their heads to the north and their backs to the west. While the artists have sometimes incorporated natural bulges or hollows on the ceiling into the depictions of particular animals, to suggest the three-dimensional volume of their bodies, the outlines of one polychrome figure never interfere or overlap with those of any other, with one exception. In that single case we see two animals that were deliberately drawn sharing a single pair of hind legs, rather than an accidental or careless superimposition of

unrelated figures. It is abundantly evident to any open-minded observer that the artists followed a plan for the placement of the animals that was carefully thought out and systematically executed. The polychromes seen together form a harmonious and integrated whole. In fact, most of the figures in other galleries of the cave are also arranged in orderly compositions, whose organization is as well expressed in its Final Gallery as on its Great Ceiling. That means, of course, that any attempt to understand the individual figures must consider them as interrelated parts of the larger composition, whose meaning is more than the isolated animals that compose it.

As a preliminary to their understanding, we must start with what we know, the characteristics of the animals and their arrangement, and in the process we must at least consider the possibility that an observed composition may in fact be a faithful reflection of a normal, expectable association of free-ranging animals. In many, perhaps most, cases the interpretation of a composition including several animals as a literal representation of a familiar natural scene is far and away its simplest and most logical explanation, no matter what its deeper symbolic meaning may be. Without first understanding what (if any) natural association is portrayed by considering all its details, we cannot hope to reach its deeper levels of meaning.

The principal composition on the Great Ceiling is the group of large polychromes. Since all the figures once considered to be wild boars are now correctly identified as bison, this composition consists of animals of three species: some twenty bison; possibly two cervids: a large and stretched-out figure at one end of the composition, another debatable figure that Breuil saw atop another bison; and possibly two horses (both the full profile and the large head). The co-existence of these three species in nature is well-known. Bison are the most numerous animals depicted, and thus the richest source of analytical information. The artists have depicted both adult male bison and adult females of that species. Bulls are easy to recognize, since the prepuce of an adult bull is often clearly visible on the animal standing in profile, the favorite attitude of the Paleolithic artist. Recognizable representations of cows are more difficult to produce than are those of males, since the distinctive sexual attribute of the cow is her udder, so much smaller than in domestic milk cows that it is virtually invisible when the animal is shown in the broadside standing position. The artists have been at some pains to show their audience that cows are present, illustrating several animals rolling in the dust, with hind legs spread. In one case, the udder of the wallowing animal is clearly displayed, and the other rolling animals may be cows as well. There is marked sexual dimorphism among bison. The bulls are much more robustly built, more heavily bearded, and have more massive, rounded heads and deeper chests than the sligher, more angular cows. There are animals with both builds on the Great Ceiling, and one might have guessed that cows as well as bulls were probably shown, but there could have been no proof of this without the displayed udder on the wallowing cow.

Recognizing that adult bison of both sexes are unquestionably represented helps us decipher other aspects of meaning of the composition. The social organization of European bison is known in some detail. Some early observations of native bison were made, and they are in agreement with the more detailed studies of

reestablished herds in the Bielowicza forest/heathlands in Poland. During most of the year, adult bulls are solitary, while adult cows form small herds with their young. However, adult bulls and cows come together in larger herds annually, during the breeding season (Hainard 1949). Altamira's artists deliberately depicted a herd of bison in breeding condition, during the period of rut. The polychromes include several curiously posed animals, in stereotypic postures that are characteristic of the rut. These unusual postures have not been recognized for what they are by most modern investigators. Once the possibility is entertained that the composition depicts a herd of rutting bison, however, one recognizes in those curious poses some of the stereotyped species-specific attitudes that are peculiar to or at least most common during the season of the rut.

Such are, for example, two of the male bison. The first seems to be a young male, back exaggeratedly humped and tail up. The second is a large male, whose head and forequarters were badly copied by Breuil. In both cases, the animals' attitudes, their hunched backs and upraised tails, are characteristic of sexually excited animals.

A female is shown with tail raised, back arched, neck stretched forward, and head straining upward open-mouthed. She has always been correctly identified as a mooing cow, but more important, this tense bellowing posture is typical of cows at the peak of sexual excitement. Her figure overlaps that of a large male, whose body may be seen quite clearly as a darker shape within her outline. The two apparently share a single pair of hind legs, a convention we shall see again in a pair of engraved bison in the Final Gallery. What is more, the same subject matter seems to be depicted in the two cases: a cow mounting a bull in a pre-copulatory ritual characteristic of bison and other bovids.⁶

Even the arrangement of the group is typical of a herd of rutting bison. The females are in the center of the group, as they would be in the middle of the herd, surrounded by the adult males. Four of the central animals (at least two of whom are apparently female) are shown rolling or wallowing, and dust-wallowing, a characteristic of the behavior of bison of both sexes in wild herds, has been noted to be especially frequent in the breeding season, and may serve as a "displacement activity" for sexually excited animals. On the edges of the composition, male bison face the center of the group, as though they were confronting the other males closer to the center. Battles between peripheral males and senior, dominant males are also characteristic of the breeding behavior of these large bovids. In short, in every detail the attributes, attitudes, and positions of the polychrome bison on the Great Ceiling are characteristic of a herd of free-ranging bison during the breeding season.

The recognition that the composition shows a rutting bison herd (and associated other animals) leads to further interpretations, for like most large mammals, bison are seasonal breeders. The period of the rut is late summer, from July through September. Consequently, there is a temporal component to the meanings of the Altamira polychromes, as well. The artists apparently intended their depictions to suggest the late summer, although there is no reason why the paintings would have had to be produced at that season. In fact, the ceiling could have been decorated at any time of the year.

The bison are often outlined with fine-line engraving, or have engraved details. Although neither Breuil nor Leroi-Gourhan seems to have paid special attention to the fact, on several of the polychromes—at least five—there are linear scraped areas or long groups of fine lines forming narrow shapes that look like the shafts of spears penetrating the bodies of the bison. Breuil mistook one of these linear forms for the back of a “wolf” he recorded on one of the bison. The lack of attention Breuil gave to these lines is somewhat surprising, since the symbolic spearing of animals fits so well with his interpretation that much of Paleolithic art was motivated by the concerns of “hunting magic.” In fact, there may be some truth to his interpretation in this case. Rutting animals are known to be less wary of hunters, making them easier to approach and perhaps to kill than they are when they are not in breeding condition.

There are many other figures in the Great Hall, but they neither add nor detract much from this interpretation. The monochrome figures, including the archaic-appearing red drawings on its southern part, which may make up a still earlier composition, are a repetition of the bison theme, with additional horses, aurochs, deer (the supposed moose are probably stylized red deer), and ibex. Since they would seem to correspond so well to his theory of bovine-horse opposition, it is surprising that Leroi-Gourhan ignored these figures in his analysis (1965). In addition there is a small number of hands, including both lefts and rights, one positive, the rest negative. The painted signs, interesting though they are, add little to the previous interpretation.

■ ALTAMIRA: ENGRAVINGS ON THE GREAT CEILING

The engraved mammals, on the other hand, seem to make up a different but complementary composition that reflects, as did the polychromes, a concern for the reproduction of important food animals. But in this case, unlike the polychrome series, the engraved series is principally concerned with the herds of red deer. The engraved deer include both complete or near-complete figures, and some 20 isolated heads, all of which seem to be of hinds. As was the case for the polychrome bison, both adult males and females are represented among the engraved cervids. And, as with their bovine counterparts, stags spend most of the year apart from the hinds and young, traveling alone or in pairs, while the females and fawns form separate herds. Stags and hinds will usually not herd together except during the autumn rutting season, from September through October, though they may be found together in winter in areas where snowfall is heavy. But in this case, too, the Altamira artists have left us in no doubt as to the season intended. The engraved figures include a calling or “bell-ing” male, in a stereotyped posture characteristic of rutting deer. Breeding herds characteristically consist of a single male and a harem of as many females as he can defend, and belling is an important means of signaling territory and maintaining control over the harem (Laurent 1974). There are as well a few engraved horses or horses’ heads. It is also interesting to note that an engraved bison’s head nearby, that looks quasi-human, recalls the hybrid bison/human masks in the Final Gallery, to be described in another paper. In marked contrast to the polychromes, engraved figures of deer greatly outnumber the bison or other engraved animals. What is more,

there is but a single isolated head among the polychromes, outnumbered by the many depictions of whole animals. But among the engravings, the majority are not whole animals, but isolated heads. It is likely that these contrasts are deliberate, and that the two compositions (the polychromes and the engravings) stand in a special relationship to each other. That idea, as we shall see later, is strengthened by figures in the rest of the cave.

If the polychromes and engravings of the Great Ceiling are any indication, there can be no doubt that sexual differentiation of individuals within a single species was an important element of symbolic classification to the artists at Altamira. This is clear enough in the case of the cervids, where secondary sexual characteristics obviously set off the predominantly female animals from antlered males. The illustration of primary and secondary sexual characteristics of bison of the two sexes is a significant aspect of the organization of the polychromes. A concern for true, rather than analogical, sexual complementarity, and an obvious interest in procreation, are even more clearly manifest in the painting of pre-copulatory ritual of rutting bison, also represented by one of the principal engravings in the Final Gallery. Only the uncritical imposition of preconceived classificatory schemes of a higher degree of abstraction kept Breuil, Raphael, Leroi-Gourhan, and the others who have gone before us from recognizing the subject matter of the Great Ceiling correctly. Had they proceeded otherwise, having the slightest familiarity with the behavior of the depicted animals, even their captive or domestic relatives, they would certainly have recognized the artists' concern for sexual differentiation of individuals and the reproductive behavior of two of their principal food animals.

The engravings on the Great Ceiling include at least seven unusual anthropomorphic figures, most obviously male, with distorted heads. Three of these are within or right next to the large hind, three near one of the male bison, and one just below another. Although these figures are by no means as realistic representations as the engraved or painted animals, they look enough like people to be identified as human figures. The figures stand erect with their arms upraised as though they were praying (for which reason they are often called "orants"). There is little doubt that they are oriented with respect to the painted animals, whom they seem to be supplicating.

Since the engravings on the ceiling are sometimes covered by painted figures (particularly the polychromes) it is usually assumed that they are part of a previous phase of decoration. However, the close association of polychromes with some of the engraved figures, particularly several large hinds, and the attitudes of the "anthropomorphs," suggests that they are not entirely unrelated. Whether there was actually a significant lapse of time between the engravings and the superimposed figures is less important than the thematic analogies between them.

■ ALTAMIRA'S GREAT CEILING: THE NATURE OF THE SPACE

The figures on the Great Ceiling are not in "ordinary space." The floor under them has been so lowered that today's visitor gets little impression of the precinct's original

condition. The visitor now examines the polychromes with relative ease, but it was by no means easy to execute or to view the decorations in the prehistoric past. Imagine the condition of the great painted gallery as it was when the Paleolithic artists knew it. Its floor was rough, and its ceiling sloped irregularly downward. Where it was highest on the north side under the polychromes, the ceiling was only 2 meters above the floor, and where it was lowest to the south, it was just under 1 meter high. No erect adult could have walked about under such a low roof. The artists must have worked in extreme discomfort, spending hours squatting or kneeling, with heads tipped back at a painful angle, eyes smarting from sweat and dripping paint, arms outstretched, muscles cramped or trembling from fatigue, as they worked on the figures overhead. The projections on the ceiling were so obtrusive, the multicolored animals so large, the painters who produced them were so close to their work, that there could have been no possible way for a painter to see an entire polychrome, and maintain correct relationships between all its elements, as it progressed. Contrary to Apellániz's conclusions (1982), the artists must often have worked in pairs or teams, some mixing color, others applying it, some drawing the animal's outline or modeling its body by applying paint or scraping it away, while others guided the proper placement of lines and masses of color from a viewpoint that gave the correct perspective. (Incidentally, the difficulty of representing another person's hallucinations makes this an obstacle to the "shamanistic" hypothesis of Clottes and Lewis-Williams [2001].) Under such conditions, painting is not a means of self-gratification but a laborious, exhausting, painful sacrifice. The decision to locate the largest and finest figures in the cave in such an incommodious position reflects a deliberate choice of an area that was not only painful to decorate, but inconvenient to view. In several places, visitors must have had to crouch or recline to see the paintings. To see the engravings, visitors had to move their illumination and their bodies from place to place while maintaining uncomfortable postures. The Great Ceiling required a sacrifice on the part of the viewer as well as the painter. Despite its uses today, the Great Ceiling at Altamira was not simply a sort of Paleolithic "art gallery" that would have been visited purely for pleasure. Neither was it the sort of space one used for the routines of daily life. Even if it had been accessible to anyone who wanted access, decorating or visiting it was an extraordinary experience.

The idea that Altamira was a Paleolithic sanctuary, then, seems eminently reasonable. That there is something out of the ordinary about its decorated spaces is obvious. The paucity of archeological residues under the Great Ceiling shows that that part of the chamber was treated as "special," and was not regularly utilized for the routine activities of daily life. While all living space is culturally ordered, the symbolic organization of the decorative program at Altamira is so systematic, regular, and all-inclusive that it goes well beyond the most elaborate symbolic structure characteristic of those "secular" spaces used for mundane social and economic activities. The values and beliefs symbolized by the figures at Altamira have to do with the reproduction of the principal food resources used by the human group: especially bison and deer. The orants (and likely, as we shall show later, the engraved human-like bison's head) show that there is more to the picture than the animals themselves:

a concern for the maintenance of a balanced relationship between humans and the natural world on which their survival depends, and for the continued well-being and growth of the human group itself. The rigor with which the organizational program is applied to the Altamira figures also indicates that it is justifiable to apply the designation “Sanctuary” to the Great Ceiling at Altamira.

NOTES

1. Breuil considered Paleolithic art to have been an integral part of ceremonies conducted by “cult ministers,” that were held in specially dedicated sanctuaries, the painted galleries. There, “se sont déroulées des cérémonies sacrées, dirigées sans doute par les grands initiés de l’époque et introduisant les novices à recevoir, à leur tour, les instructions fondamentales nécessaires à la conduite de leur existence. Les fresques, les gravures exécutées par les ancêtres étaient l’objet de gestes rituels et l’occasion des enseignements jugés indispensables, et de nouvelles fresques exécutées sur ces mêmes parois, venaient compléter la décoration de ces lieux réservés” (Breuil 1952: 23). It was the influence of Breuil that generalized this idea of the decorated cave as “Sanctuary,” in whose depths took place “la recherche de véritables arcanes presque inaccessibles au vulgaire . . .” (Breuil 1952: 23).

2. For purposes of this chapter, it is unnecessary to make a distinction between “religion” and “magic,” terms now distinguished by a load of meaning and emotion that would probably have been unfamiliar to Paleolithic people.

3. Of course, we must always be aware that what we consider inexplicable in such terms may not have been so considered by the prehistoric people who used the caves in question.

4. Not everyone, nor even everyone endowed with the needed talent, became a producing artist, according to Breuil. Art was, he believed, not an individual phenomenon, but “un fait social, collectif, témoignant d’une véritable unité spirituelle, . . . supposant l’existence d’une sorte d’institution en régissant le développement par une sélection et un enseignement des mieux doués” (Breuil 1952: 22). Art, then, was institutionalized and its production entrusted to a select and well-trained few. Following Breuil’s argument to its logical conclusion, many who are “consumers” of archeological fact, and have written about Paleolithic art without knowing it at first hand, have promulgated the idea that every artistic product of the Upper Paleolithic is a masterpiece, an error that Breuil himself would scarcely have sustained.

5. The function of cave paintings as “transitional phenomena” is discussed more fully in Freeman et al. (1987).

6. This stereotypic behavior of bovine animals has been known since the time of Aristotle: see his *Historia Animalium* (Loeb Classical Library, 1970 [345?/342? B.C., no. 348]), vol. 2.

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Caves and Art

RITEs OF INITIATION AND TRANSCENDENCE

In a previous chapter in this volume, I discussed some of the evidence that leads to the recognition that certain precincts in Paleolithic sites with or without decorations are truly sanctuaries, citing cases from Cueva Morín, the Cueva del Juyo, and the Great Ceiling at Altamira. But the evidence I presented for Altamira was incomplete. The cave and its decorations provide a more extensive demonstration of its uniqueness and the propriety of calling it a sanctuary in its integrity. In its decorations, the Great Ceiling bears a symbolic relationship to the depictions in the Final Gallery of the cave (also called the Cola de Caballo) that is so striking that it can only have been intentional. The central galleries at Altamira seem to serve as a sort of symbolic bridge between the decoratively richer galleries near the vestibule and the final recesses of the cavern. What is more, some of these details suggest that the Altamira sanctuary was the locus of periodic rites of transition or initiation. Before we can evaluate this suggestion, it will be necessary to complete the description of the galleries of Altamira and their depictions (Freeman and González Echegaray 2001).

■ ALTAMIRA'S CENTRAL GALLERIES

As one goes beyond the Great Ceiling into the central galleries of the cave, finger-engraved meanders appear on the ceiling. Another set of meanders was part of a

fallen frieze further on, where it may perhaps mark a break in the continuity of depicted subject matter. Animals in this area are represented by finger engravings, by engraving with a sharp implement, or by painting, and there seems to be no difference in the selection of species represented in each technique. The series of animal depictions begins with digital engravings of wild oxen, followed by the true engraving of a hind. Engraved horses and deer and one large bison occupy the next gallery in the sequence. Black horses are followed by the red scalariforms of the “Rincón de los Tectiformes”; its end is marked by a large patch of red paint. Along the sides of the main gallery there were friezes (one of which is now partly collapsed), with engravings of horses, deer (stags as well as hinds), an anthropomorph, and more meanders.

Further on, when the corridor turns sharply, engravings of deer vanish as if by magic, not to reappear until the Cola de Caballo. We find engraved figures of wild oxen and goats, and black drawings of horses and bison, but no deer. Black ibex are added to those animals as we pass along the next gallery, when at last the hind also reappears, but only as a single head in black outline. Black horses are found with the first enigmatic black marks (like those in the walls of the Final Gallery) and along the irregular wall we find the first “masks,” in this case less well defined than they are in the Cola de Caballo. In these intermediate galleries, the figures and their relationships correspond more and more closely to the symbols and organization of the Final Gallery as we progress in the direction of that gallery from the Great Ceiling.

■ ALTAMIRA'S FINAL GALLERY: THE COLA DE CABALLO

When we began our part in the 1980s reevaluation of Altamira and its depictions, we chose to invest a great deal of effort in a reexamination of the Final Gallery of the cave. (The methods we employed are described in great detail, with our conclusions, in an earlier report [Freeman et al. 1987].) This gallery, also called the “Cola de Caballo” from its fancied resemblance to that appendage, has a number of characteristics that make it an ideal laboratory for the testing of recording methods and the development of analytical techniques concerning the importance of positioning and relationship in the organization of Paleolithic art. It could be studied as an “isolate” (though we now know that it is not unrelated to other parts of the cave), and it is small enough (just 70 meters long, usually less than 2 meters wide, and sometimes even narrower, and from less than a meter to about 2 meters in height) so that it could be examined completely in a reasonable time. An adult can usually touch the walls on either side without having to move from the middle of the track. In addition to these spatial constraints, the gallery is richly decorated, with fingertip meanders, deep and fine-line engraving, and black drawings, some representing animals, others depicting complex geometric figures, and others that are just “marks.” Its size and the shape of its corridors naturally constrained the ways the Paleolithic artist could place the decorations, as well as the ways they would later be viewed or studied. The gallery makes many sharp bends that divide its topography into clearly distinct

sections. The walls and ceiling of the gallery are highly irregular, covered with projections and crevices that provide a large number of surfaces suitable for decoration, and these irregularities keep many of the figures from being seen from anywhere but one strategic viewpoint. These characteristics make it possible to deduce where the Stone Age artist or viewer stood (or crouched, or lay) to produce or see such figures, and in what direction he or she must have been looking at the time. Since there is only one way into the gallery, and one way out following the same track, we can even establish the most probable order in which most of the figures were intended to be seen, to determine which were seen entering and which were only visible on the return trip. Of course, this is much harder, usually impossible, for larger, more open spaces. Our first step was to produce an accurate map of the Final Gallery, locating on it each and every figure we detected.

The twists, bends, and irregularities of this gallery subdivide it into six distinct segments or corridors, that we have given names. With one exception (the "Empty Corridor") each of them contains decorations, including a total of 74 masses of undecipherable charcoal lines and patches. The other depictions are one positive handprint in black, two patches of finger meanders (one that is an extension of the meanders at the entry into the first five meters of the first corridor), several black tectiforms, several engravings including both geometrics and the figures of five bison, eighteen deer, two horses, and three supposed "goats," as well as three black outline drawings, all of which seem to portray horses. The positive handprint, near the end of the first patch of "macaroni," is that of a youngster's left hand, which from its outline may have worn a glove. There are also some indeterminate figures that may be clumsy or unfinished attempts to represent unidentifiable animals. One of the fine-line engravings, a bison, had been partly completed by the addition of black lines to form its haunch and foreleg, suggesting that the engravings and black line drawings in this gallery are most probably contemporaneous. A series of large projections from either wall of the Final Gallery, uncannily suggestive of the heads of humans or bison, has been minimally altered by engraving, pecking, or the addition of black lines, to enhance the resemblance. These are the so-called masks at Altamira. So far, nine certain masks are known from the Gallery.

The Empty Corridor splits these representations into two series. The two galleries nearer the entry are the Bison Gallery, where all three engraved animals are bison, and the Low Gallery, where there are four engraved animals on the ceiling, all stags, and two black outlines of horses, one on the right wall, the other on a block projecting from the floor. No engraved horses appear between the entry and the Empty Corridor. The Low Gallery ends with an engraved "geometric" figure on the ceiling, and near it, in a lateral fissure on the left wall of the corridor, there are more finger-engraved meanders.

Beyond the Empty Corridor comes a complex corridor consisting of our Gallery of Tectiforms and a wider room at its end called the Chamber of Masks. The former contains two groups of black tectiforms, each accompanied by spider-like figures (circular or oval figures with lines radiating outward) next to recesses in the right wall of the corridor, and a third engraved geometric a few meters farther on. Aside

from two horses, one of which is among the finest engravings in the cave, the other engraved figures in this unit are three bison, including a pair of animals shown engaging in stereotyped breeding ritual, the female mounting the male. The last corridor is the Cervid Gallery. It becomes so low and narrow that one must lie flat to wriggle through it until it finally becomes impassible. There are fourteen figures of deer, including two stags and twelve hinds, and three other animals interpreted as goats, though they may be yearling stags instead. It is important to note that in each series, the one before and that after the Empty Corridor, representations of bison come first, with deer present only in the innermost part of each.

■ FIGURE DISTRIBUTION IN THE FINAL GALLERY

There is much more evidence, if that were needed, that the distribution of figures in the Final Gallery is organized rather than haphazard, and that their placement corresponds to a carefully executed plan followed by all the artists. The divisions we detected evidently provided the framework for this symbolic pattern. Even the apparently random black marks obey its dictates. In the first part of the Final Gallery, there are about twice as many of these patches of linear marks on the left wall as on the right (20 as opposed to 12). Beyond the Empty Corridor, this lateral distribution is reversed, with about twice as many on the right (26) as on the left (16). The difference is statistically significant: the likelihood that the reversal of proportions is accidental is less than 0.05 (less likely than one chance in twenty). There are so many of them, and they are often so far from the few black drawings, that the explanation that the black marks may result from the artists' sharpening their charcoal crayons as they worked is also unreasonable, and another alternative, that they were used to blaze a trail to be followed, is ridiculous for a corridor where there is only one possible route in and out. Other practical reasons for the distributions have been considered, and all rejected, leaving the conclusion that their organization is simply a reflection of the intentional symbolic organization of space. Other evidence for lateral differentiation comes from the placement of the engraved bison and the painted geometrics, all on the right wall of the Gallery. All but one of the hinds' heads are also on the right wall of the Gallery, an apparent reflection of the fact that these figures occupied a symbolic position that was somehow complementary to that of the bison.

However, the most revealing evidence of deliberate organization of the decorations is the differential distribution of engravings of bison on the one hand and deer on the other. In corridors where bison are found, there are never any deer, and (of course) where there are deer there are no bison. It is remarkable to us that this mutual exclusion, which seems so obvious, was not detected before. It is all that is needed to show that, in the Final Gallery, cervids and bison stand symbolically in equivalent positions in a system of complementary opposition. Contrary to Leroi-Gourhan's interpretation (1964), the ubiquitous horse does not seem to occupy any particular place in this system. Figures of horses are represented in every technical style known in the site: polychromes, red outlines, black outlines, and engravings. Since horses are found next to both the animals that are at the poles of the comple-

mentary opposition, it is unlikely that they are themselves part of either group more than the other.

Other details of the size, positioning, and distribution of the representations help complete this interpretation. First of all, while the density of depictions of deer increases as we go deeper into the gallery, bison are if anything more numerous toward the cave entry. This difference of focus is underlined by the fact that all but one of the bison actually face the entry, while all but three of the deer face into the Final Gallery. All the engraved bison without exception are whole animals, but only six of the deer (five of them males) are whole: the other twelve are represented by heads or heads and necks alone. The bison are represented as proportionally larger in scale than the deer: only seven of the deer might be called "large" if we are generous in our usage, but all six of the bison are "large" by the same standard. The degree of aggregation represented also differentiates the two species. Except for one case (a pair of bison engaged in pre-copulatory behavior, the female mounting the male), individual engravings of bison are always some meters distant from each other (a minimum of 2.5 meters, an average of 11). Engraved deer, on the other hand, always appear in groups. True, the two major concentrations are separated by more than 30 meters. But within either concentration, that in the Low Gallery or that in the Cervid Gallery, the average distance between individual engravings on the same wall is just over 1 meter, and the closest non-superimposed figures actually touch. In the Cervid Gallery, the distance between any engraved deer and another on the opposite wall may be as little as 1 meter and is never greater than a meter and a half. These observations all reinforce the interpretation of bison and deer as symbolically related by the principle of complementary opposition.

Possible correlates of the symbolic opposition of deer and bison that would have been meaningful to prehistoric hunters are not hard to find. The fact that representations of deer far outnumber those of bison is in accord with the archeological evidence from Altamira's Paleolithic levels, where the most abundant mammal bones are those of red deer. Deer were probably a more frequent prey, and a more frequent dietary item, than were bison (and deer were certainly more common than bison in the landscape). Deer and bison contrast markedly in behavior, as well. Deer remain hidden as much as possible, do not move about much during the day, and (except during the rut) are timid, skittish, and difficult to approach. Bison, on the other hand, are ordinarily highly visible animals, and are active during the day. Deer fall prey to wolves and other large predators quite frequently, while adult bison are such large, powerful creatures that herds are relatively untroubled by non-human predators. Descriptions of techniques used in the bison hunt by Plains Indians before the introduction of the horse and firearms indicate that the animals allowed stealthy hunters (sometimes disguised in wolf- or deerskins) to approach nearly within arm's reach of them before moving away. (Hunters armed with spears or bows sometimes approached the herd concealed behind horses, when they had them.) There was in fact a quite peculiar relationship between these majestic beasts and their human hunters, involving aspects of prey behavior and techniques and organization of the hunt, that clearly differentiate deer from bison as subjects for physical, mental, and

cultural manipulation. The analogical relationship of people and bison in the art of the Final Gallery suggests that people thought of themselves, as well as the bison, as essentially unthreatened, dominant creatures of their kind in a usually predictable and benevolent environment.

■ THE MASKS IN THE FINAL GALLERY

There is one other kind of decoration in the Final Gallery, the eerie, minimally retouched natural projections that are conventionally called “masks.” They are in many ways the most remarkable of the decorations in the Final Gallery. These are natural head-like projections from the cave wall that resemble face-on or profile heads of men or animals. Each of them has been deliberately modified to make their naturally suggestive appearance still more evocative, just as was the case for the face in the sanctuary at el Juyo. In the course of our investigations, we discovered six of these figures, which when added to the three already known raises the total to nine. The presence of masks is not restricted to Altamira among Cantabrian sites. There is a particularly fine example of a large mask representing the profile of a bison in the cave of Castillo. A smaller, frontally viewed face of a small horned animal was also found in the same site (Alcalde del Río, Breuil, and Sierra 1912: esp. fig. 144, lams. 62, 85, 86), next to what may be yet a third such figure.

Most of the masks at Altamira are clearly intended to represent bison. One is the frontal view of a human face. There are also three that while apparently representing bison also suggest human features, or, in one case, represent a hybrid figure that from one viewpoint is a bison, but becomes very man-like when viewed from a different perspective. The conclusion is inescapable that the artists intended to represent a transformational series, including figures that are bison in every respect, figures that are wholly human, and hybrid figures that establish a symbolic equivalence between the two species.

Masks are related in both subject matter and frequency to other depictions in the Final Gallery. The relationship between the engraved whole bison and the bison masks is in many ways analogous to the relationship between whole engraved deer and engravings of deer heads. If the masks are included in the count of bison figures, however, the density of depictions of bison increases from the Bison Gallery to the Chamber of the Masks, just as the deer increase from the Low Gallery to the Cervid Gallery. Beyond the Empty Corridor, the ratio of heads to whole deer is 9 to 5, while the ratio of masks to whole bison is 5 to 3. The difference between the ratios is negligible. Near identity in proportions in this case confirms the postulated correspondence, leading us to conclude that consciously or not, the Paleolithic artists intended these figures to be compared, weighing one against the other.

But there are also major differences between the series “whole deer + deer heads” and “whole bison + bison masks.” All the masks are very much larger than the heads of the engraved bison, but that is only true for a minority (three of twelve) of the deer heads. While six of the nine masks are on the left wall, all six engraved bison are on the right, as are all but one of the heads in the Cervid Gallery. These

differences are statistically significant, and there is a very small probability of their being due to chance. So, while the bison + mask group is intended to be seen as somehow related to the engraved deer head + whole deer group, the relationship indicated is not one of equivalence. Neither the sequences nor the species are intended to be seen as interchangeable. The difference becomes clearer when the mask distribution is examined more closely.

■ THE FINAL GALLERY: EQUIVALENCE AND TRANSCENDENCE

The largest concentration of the masks (four) is found in the Mask Chamber. This is the room where the figurative depiction of bison reproduction in the Final Gallery is located. It is also the room in which the masks make the clearest statement of the equivalence of humans and bison. In that sense, the Mask Chamber is a focal part of the Final Gallery—the locus of a most important condensation of fundamental symbolic values. These symbolic statements are distinctly separated from the chamber filled with cervids. They embody aspects of belief that differentiate bison from deer.

The positions of the remaining masks indicate that they also serve other important symbolic functions. Those five masks are strategically sited at liminal points along the Final Gallery where there is a fundamental change in the nature of the decorations, as if they were the guardians of “gates” or portals through which one passed as one symbolic assertion was completed and another began. Most often, the masks at these portals are all but hidden from view until the visitor is right atop them, when they suddenly spring into the peripheries of the visual field in a way that can be startling even to the viewer who is familiar with the experience. All the “Mask Gates” but one are marked by a single mask. The other, the first gate one sees on entering and the last on leaving, is flanked by a pair of masks, one on either side of the corridor, but even in this case, only one was intended to be seen at a time. The one seen on entering is wholly a bison. The one seen on leaving is a bison-human hybrid. The masks on the right wall invariably face the entering visitor, and those on the left the exiting viewer. The visitor who passes through the Final Gallery viewing all its decorations in the most efficient manner, without stopping to retrace steps or turning to look about, will in every case but one see the masks on his or her right—the exception can be seen from both directions.

In the case of the engraved heads of deer, a part animal, less than a complete deer, is used to evoke the animal as a whole in a sort of graphic synecdoche. In contrast, some of the masks suggest hybrid beings, part-human, part-bison, that are something surpassing a whole animal: strange and complex “supernatural”¹ entities whose nature transcends that of either humans or bison. All three of the masks on the right side entering are simply bison, and none really suggests a human visage. But the very next mask, the first one the visitor sees on turning back through the Mask Chamber, is a purely human visage. It takes no overdeveloped imagination to see in the long, saturnine mask that next appears a suggestion of blended human and

bovine features. The two profiles that follow are simply bison, but the next, though fundamentally bovine, once more looks oddly human. The last mask one sees on exiting is the most extreme example of a hybrid visage in Altamira. It behaves almost as an optical illusion. Without any voluntary effort on the viewer's part, it shifts back and forth between its human and animal aspects. Seen in sequence, the masks present a gradual transition from depictions that are simply bison or purely human to representations of hybrids blending bison and human natures, suggestive of the symbolic metamorphosis of the former into the latter, and a metaphoric equation of these two very different beings.

Significantly, the equivalence of humans and bison is also suggested by figures in other decorated caves. A vertical red bison at Castillo is one example, and the "calligraphic" black bison at La Pasiega another that is even more remarkably human. Figures of hybrid men-bison are also known from France. There are two examples in the Sanctuary at Trois Frères, one of them the well-known semi-human, bison-headed figure, said to be playing a flute or musical bow. The most remarkable figure of the kind in Spain is the vertical bison/man modeled by the natural relief of a stalagmitic column at Castillo. This figure has a bison's head and body, supported by human legs and feet (Ripoll Perelló 1971–1972). The column is crowned by the roughly sculpted head of another bison, made by enhancing a naturally evocative formation.

■ THE COHESIVENESS OF SYMBOLS AT ALTAMIRA

It is evident when all the evidence is reviewed that the compositions at Altamira, engravings as well as paintings, polychromes included, form a single interrelated whole that represents similar concerns in different ways. Once the figures are correctly identified and the structure that underlies their placement and their relationships is understood, the unifying integrity of the whole can be seen. We found exactly the same subjects—deer, bison, horses, ibex, anthropomorphs, and geometric figures—represented both on the Great Ceiling closest to the cave's entry and the Final Gallery in its deepest recesses. The same animals are found in the central galleries, and those galleries make a structured symbolic transition between the galleries at the two ends of the decorated space.

The same curious scene of an excited cow mounting a bison bull is repeated both on the Great Ceiling and in an engraving in the Final Gallery. The use of a virtually identical design, with both animals sharing a single pair of hind legs, to repeat this unusual subject matter in different media is enough by itself to show that the procreation of the bison herds was as much a concern of the engravers of the Final Gallery as of the painters who made the polychromes on the Great Ceiling. Cervid reproduction is another theme uniting the two galleries, as is evident from the association of antlered stags and antlerless hinds in the Cervid Gallery and in the Great Ceiling's engraved series. The human-bison relationship so clearly seen in the masks of the Final Gallery is also present in muted form in the man-like face of an engraved bison on the Great Ceiling.

A single set of structural principles was applied to the symbolic organization of the two galleries in precisely complementary and opposite ways.² While the species and themes represented are continuous between the Great Hall and the Cola de Caballo, and the organization of symbols in both areas obeys the same underlying structural principles, the application of those principles in one gallery consistently yields inverted transformations of the placement and relationships of figures in the other. The Great Ceiling gets its name from the fact that its famous polychrome decorations are all on its ceiling. Most of the important figures in the Final Gallery, in contrast, are on its walls, with few on the ceiling. The most numerous and striking figures in the Final Gallery are its engravings; it is painted figures that dominate the Great Ceiling. The decorated area on the Great Ceiling is undivided space, whose two major compositions, the paintings (principally bison) and the engravings (principally deer), are superimposed on each other without separation. The Final Gallery, on the other hand, is split into two major segments, each with subdivisions, and the bison and deer themes are segregated and occupy alternate galleries. The polychrome composition contains just one hind and several bison, while the Final Gallery, like the engravings on the Great Ceiling, has many hinds and few bison. The polychrome hind is disproportionately large compared to the bison, while the bison in the Final Gallery are much larger than the deer. In the Final Gallery, there are several large heads (the masks), while on the Great Ceiling there is but one each in the paintings and the engraved series. Complete polychromes on the Great Ceiling are often three-dimensional (from the natural irregularities over which they were painted), while the large painted head is flat; in the Final Gallery, the heads are three-dimensional projections, and the whole animals are flat. Further contrasts are numerous, but the enumeration of data that all point to the same conclusion would serve no purpose other than to burden the reader with redundancies.

It is also true that there are systematic similarities and contrasts between the engraved symbols on the Great Ceiling, on the one hand, and its paintings on the other. They do not coincide exactly with the comparisons and contrasts we have made of the figures in different galleries. In fact, one can find enough points of contrast between the engravings on the Great Ceiling and those in the Final Gallery considered by themselves to show that the two sets of figures were also intended to embody the same pervasive set of concerns in contrastive and complementary ways.

All the evidence we have reviewed indicates that the decorations in all Altamira's galleries were produced and arranged according to a single uniform program of symbolic organization. This program involves such a complicated and multi-faceted interplay of parallels in subject matter and relational oppositions, and its application was so pervasive and time-consuming for those who produced it, that it can scarcely be accidental. (Incidentally, in my opinion, that implies that the different compositions I have discussed, in all the galleries, must be broadly of the same age.) The remarkable extent and consistency of interrelationships between the major compositions in Altamira's decorated galleries clearly show the importance of the symbols employed to the cultural system of the artists, support the identification of Altamira as a sanctuary or set of interrelated sanctuaries, and reveal the operation of sophisticated,

insightful, and playful human minds capable of tours-de-force of symbolic construction and cultural complexity rivaling those of any living human group.

■ SPECULATIONS: ALTAMIRA AND INITIATION

The observations presented in the preceding interpretation, including, I submit, the identification of Altamira as a sanctuary, have a sound basis in the data, and can be empirically demonstrated. While it is possible to carry interpretation further, I realize that to do so involves a great deal of speculation. In this case, by speculation I mean logically constrained conjecture, *not* the free play of imagination. The facts in the case of Altamira permit plausible inference that leads to interesting suggestions. I caution the reader that conjecture is not fact, and assertion is not proof. While my conjectural interpretation may in fact be correct, it may also be wrong, and alternate interpretations I have not considered may fit the data equally well.

I have said that the idea that Altamira was a prehistoric sanctuary is justified. There are many kinds of sanctuaries that serve different purposes. The themes represented by Altamira's decorations indicate some dimensions of its purpose, while the correspondence of the characteristics of the Final Gallery to those of some sacred sites used for initiation ceremonies—rites of transition and transformation—in historic times suggests that it too may have served similarly.

The masks of the Final Gallery, hidden away deep in the bowels of the cave, depict a transformation or intergradation between humans and bison, suggesting that, for the artists, the two were somehow equivalent. In the same gallery they represented deer (which, to judge from their frequency in the Magdalenian level, were the principal prey of the hunters) as more abundant but at the same time markedly smaller than either the masks or the engraved bison, emphasizing the symbolic pre-eminence of the latter over the deer.

On the contrary, in the most accessible composition, and the nearest to the light of day and to the space used for the ordinary activities of daily living, the polychrome figures of bison are much more abundant than are those of deer. At the same time, the bison are drawn at a relatively smaller scale than the painted deer. Significantly, the closer they approach the large hind, the smaller the polychrome bison become. And there is a group of much smaller black outlines of bison near her figure, one just below her neck.

It seems possible that the artists, decorating the most visible part of the cave, tried to emphasize the special importance of the hind relative to humans and bison by painting her at an exaggerated scale and associating her with engraved "orants." Perhaps it would not have been advisable to show disdain for deer, a principal mainstay of human subsistence, despite the fact that they were comparatively easier than the bison to capture and kill. Perhaps, in order to counterbalance any suggestion of disdain that might be inferred from the treatment of deer in the Final Gallery, to avoid insulting so important a subsistence resource, and to ensure that deer would continue to sacrifice themselves to the needs of humans, the artists symbolically expressed reverence for and supplication of the large hind as a representative or em-

bodiment of all deer in general. No such symbolic compensation was needed in the case of the bison. The artists had already convincingly incorporated their belief in the equality of humans and bison by means of the symbolism of the masks in the Final Gallery.

The Magdalenian artists at Altamira seem to have declared in the polychromes on the Great Ceiling and the masks of the Final Gallery some of their society's fundamental beliefs concerning the relations between humans and the natural environment. If the Final Gallery expresses the wisdom of a community by means of figures whose attitudes and arrangement correspond to definite principles of symbolic organization, the Great Hall recombines the same symbols in accordance with a new and complementary structure, to reveal another side of the same message.

The animal world as revealed at Altamira is divided into two principal groups. One is that of the large, powerful bison, animals that aside from human beings had almost no effective mortal enemies in nature. The bison are contrasted to the timid and vulnerable deer. In a stable, rich, nurturing environment, a perceived equivalence between the sturdy, brave, and carefree bison on the one hand and human beings on the other would be quite understandable. As the bison did in their proper domain, humans reigned in their own.

The polychromes, executed on the Great Ceiling so close to the light of day, express their message with simplicity, clarity, and power, and their content is not hard to decipher. But their message is incomplete. In the shifting shadows of a dark and twisted gallery lay hidden their secret conclusion. That conclusion is only revealed to those who follow a narrow and arduous path, finally creeping along on their bellies, until finally they arrive in the very innermost entrails of the grotto, from which the only possible way out is to return along the selfsame constricted path. Their secret is a simple but profound equivalence: bison and humans are each the shadow of the other. The multiplication of the bison herds signifies the florescence and increase of the society of humans.

The characteristics of this obligatory itinerary and its hidden message suggest that Altamira was the locus of prehistoric rites of initiation. Following a narrow and menacing path, the novice was eventually swallowed up in the deepest bowels of the earth and lay there nearly helpless and immobile. Only after contracting to turn in the smallest possible space to force a way back out the womb of the earth was it possible to emerge again, first to a wider gallery where ritual practitioners could explain the hidden message of the depictions to the initiates, then to daylight, symbolically reborn, but transformed by the revelation bestowed in the process of symbolic death and rebirth.

Symbolic indications of transformation and transcendence characterize the three sanctuaries that I have discussed in these chapters. I have indicated that the treatment of the Early Aurignacian burials and the mortuary precinct at Cueva Morin suggests a concern for the neutralization and placation of the possibly threatening physical remains of the deceased by means of mortuary rituals, and the transition of the dead by such means to a new social status, still as members of the ongoing social group. At the Cueva del Juyo, whatever the exact nature of the rituals there

performed, the sanctuary shows a preoccupation with both the change of seasons—the regular periodic diminution of day length, the annual regression of the sun from the time of its longest and most beneficent appearance—and the fusion of the two sides of humanity and the natural world: their more “natural,” uncontrollable, instinctive, and bestial side symbolized by the large cat that is the head’s proper left side, and the more “cultural,” controlled, and benign side, symbolized by the bearded human that is its proper left. At Altamira, it is a symbolic equivalence of bison and people that is indicated by the mask series in the Final Gallery. In all these cases, a fusion of “opposites” that transcends what we can observe in nature is indicated. The late Mircea Eliade called such reconciliations of opposed principles a characteristic of the oldest and most widespread symbols of the “paradoxical state of the totality, the perfection, and, consequently, the sacredness of God” (Eliade, 1971: 146; see also 1979). While we need not believe that all that is implicit in this affirmation can be applied to the Paleolithic evidence, the fact that it comes from such a respected authority on the history of religious systems reinforces our interpretations.

The conclusions concerning relationships between the depictions presented in the previous chapter as certain are susceptible to validation and proof. We do not pretend that the more speculative aspects of our interpretation are necessarily correct, or that they are less imaginative than those of Henri Breuil, Max Raphael, or André Leroi-Gourhan: in fact, our interpretation shares some particulars with each of theirs. But because it is based on a minute examination of the cave and its compositions in their manifold details, it is more consistent with all the data, and explains more of the characteristics of Altamira and its decorations than did they, and at the same time it is in better agreement with what we know of ecology, ethology, psychology, and socio-cultural anthropology, and all that we know of the history of symbols.

NOTES

1. By the term “supernatural” I mean here that the figures go beyond any possible experience of the natural world. I do not mean the term to be understood as it is in ordinary everyday usage, with its accompanying baggage of meaning and emotion.

2. I did not approach the study of the Altamira figures using the theoretical framework of French “Structural Analysis,” as exemplified by the work of Claude Lévi-Strauss (esp. *Anthropologie structurale*, 1958). Though my stance is not by any means “anti-theoretical,” I believe that a slavish and overly rigid adherence to any theoretical viewpoint can or must lead to distortions of or falsifications of the data studied, or (at very least) to the imposition of an inappropriate and subjective interpretive scheme on them. The relationships of complementary opposition described here in fact suggested themselves as our investigations of the cave and its depictions progressed.

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PART

VI

THE BENEFITS OF COOPERATION

As one who has personally benefited greatly from contact with situations and investigators in Spain, as have my students, I feel that it is only seemly to conclude the collection with the chapter “The Participation of North Americans and Spaniards in Joint Prehistoric Research in Cantabria.” Although some chauvinists assume that in our collaboration, the Europeans alone have been the recipients of vast knowledge gained in the course of cooperating with their wiser U.S. counterparts, in fact the story is actually one of both give and take. Equally important information has passed in both directions. New World archeology historically resisted the idea of stratigraphic excavation, until one U.S. prehistorian learned better at the cave of Castillo.

Still, today some U.S. excavators carefully separate the finds from different soil horizons, apparently without realizing that those discolorations formed after the deposits were laid down and that they often crosscut more archeologically meaningful “natural” layers of sedimentation. That is not to say that the conclusions of those excavators are always wrong, but it does suggest that European excavators may sometimes use preferable procedures. On the other hand, European-trained archeologists, educated as geologists and paleontologists, may be overly concerned with “refining stratigraphy” and establishing the supposed relative age of their finds

and too little concerned (or quite unconcerned) with their anthropological significance. They may engage in attempts to overrefine chronological relationships without realizing the extent of deliberate human (or “natural”) interference with the “normal” orderly succession of deposits. Just as dangerous is the assumption that all differences between archeological assemblages must be the result of the evolution of functionally similar industrial complexes over time, before demonstrating that the assemblages in question are actually functionally equivalent. International and interdisciplinary cooperation can go far to palliate these shortcomings.

*The Participation of North Americans and Spaniards
in Joint Prehistoric Research in Cantabria*

It is a great pleasure for me to be invited to contribute an essay on this topic, since I have enjoyed the most cordial and fruitful relationships with Spanish colleagues, especially in Cantabria, in my own research during the past 37 years. The careers of researchers from other countries run like colored threads through the historical fabric of prehistoric investigations in Spain, against the broad background of their Spanish counterparts. Despite changes in her political climate, and differences in philosophy and orientation between her own professionals, Spanish prehistory has been from the first fully international, and refreshingly open to outsiders. My own studies were from the outset facilitated by Spanish colleagues, even though they themselves sometimes could not seem to agree with each other on anything other than the importance of fomenting the discipline of prehistory. In my first years of work in Spain, students who wanted access to the wonderful artifact collections in Spain's many museums needed written recommendations from Spanish professionals who knew them, or at least knew their professors. In my experience, recommendations written by one specialist to another were always accepted and attended with grace, even when the two specialists would not be seen in the same room together. Now, I have studied in several other countries in Europe, North Africa, and Asia, and I must say that my experience shows Spain to be unique in this respect.

As I said, the history of Spanish prehistory is intertwined with the careers of foreigners. They include such stellar personalities as the Sirets, Émile Cartailhac, the Abbé Henri Breuil, Paul Wernert, André and Arlette Leroi-Gourhan, F. Clark Howell, and, especially, Hugo Obermaier. (See Straus's overview [1992].) But to deal with all international collaboration in Spanish prehistory is not my goal: it is the easier one of discussing North American involvement in research on Cantabrian prehistory. And, it is fortunate that this topic has been summarily treated before, in a useful chapter published by Lawrence Straus (1979). Nor will I attempt to provide a full bibliography of publications that resulted from this international collaboration: my own alone, or that of Straus, would fill many pages.

The beginnings of North American interest in Cantabrian prehistory date to the early years of the twentieth century (if we exclude from consideration earlier but sporadic visits by North Americans to Altamira, to view its famous paintings). As Straus (1979) has noted, serious involvement of North American professionals with Cantabrian prehistory seems to have begun in 1912, with visits to the caves of Castillo and Altamira by Henry Fairfield Osborn and George Grant MacCurdy, mentioned in Osborn's work *Men of the Old Stone Age* (1918: 162), and in MacCurdy's *Human Origins* (1924: 22). The impression the visit made on them is readily apparent from the space their works on world prehistory devote to the Spanish Paleolithic. Osborn in particular was impressed by the exceptionally complete, 15-meter-deep stratigraphic sequence of Paleolithic deposits exposed by Obermaier and others in the site of Castillo.

But in the following year, a young American came to work in Cantabria and the result was to have a truly revolutionary impact on the prehistory of North America. Though the story is known to many Americanists, it is not known by most Old World prehistorians, so I will repeat it here, since it beautifully illustrates the mutual benefits that international collaboration in research can produce.

The scholar in question was Nels C. Nelson (Obermaier 1916: 173; 1924: 162). At the time, Nelson worked for the American Museum, where he had helped Osborn prepare *Men of the Old Stone Age*, with its description of Osborn's visit to Castillo, for publication. Impressed, Nelson came to help excavate at Castillo in the summer of 1913. Previous to his work there, the principles of careful stratigraphic excavation were almost unknown to U.S. archeologists, and except for isolated instances, were simply not applied to the study of North American sites (Gamio in Mexico and Uhle in California had both conducted stratigraphic excavations earlier, but Uhle's conclusions were largely discredited in the United States). One of Nelson's teachers at the University of California, the influential A. L. Kroeber, was not convinced that there was any possible utility to stratigraphic excavation in North America. The time depth of the accumulation of sites in the U.S. was believed by most authorities to be very short, so that there seemed to be little likelihood that peoples whose cultures were significantly different would have settled on exactly the same spot, an opinion that is both incorrect and, strangely, still used by some as an excuse for ignoring obvious "cultural" stratification in open-air sites, even in the Old World, where most prehistorians know better. What is more, such marked differences as those between

Mousterian and Upper Paleolithic assemblages had not yet been found in U.S. sites, and Kroeber, who thought that only such major disjunctions in artifact assemblages could serve as indications of cultural change, was simply not prepared to appreciate the fine-scale changes that Max Uhle could demonstrate stratigraphically in his excavations at the Emeryville shell-mound (Willey and Sabloff 1993: 63–64). Kroeber's negative opinion discouraged others, including Nelson himself, from attempts at stratigraphic excavation.

But virtually single-handedly, Nelson was shortly to change this attitude, showing North Americans the advantages of applying the techniques of careful stratigraphic excavation that they would soon adopt as standard practice. At the time of his visit to Castillo, Nelson had already begun his own research in the Galisteo Basin, in the U.S. Southwest. Though he must have known of Uhle's work, he apparently thought that stratigraphic excavation would be of little use in his research. But, by his own account, he was so impressed by the stratigraphy he saw exposed on the walls of the deep excavation at Castillo, and by the culture-historical results that he saw could be obtained through careful attention to stratigraphy, that on his return to the southwestern U.S., he began to search for a site with an undisturbed long stratified sequence that would establish the foundation for a cultural chronology of the Galisteo Basin, and finally found it at Pueblo San Cristobal. In a personal letter written in 1960 to the U.S. archeologist Richard Woodbury, Nelson said: "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" (Woodbury 1960: 98).

True, Nelson did not follow the example he had seen at Castillo exactly, for what he introduced was excavation by arbitrary spits, rather than natural levels of deposition. That may be partly excused on account of the fact that much of the stratigraphy in southwestern U.S. sites is anthropogenic—results from human activities such as the excavation of building foundations, or the dumping of garbage in abandoned houses—and his previous American experience had been with shell-middens, where it is notoriously difficult to distinguish natural strata. But in spite of this, his techniques were infinitely superior to the complete disregard for stratification that had characterized U.S. archeology before. Nelson's evident success and the obvious validity and wide diffusion of his results led to the adoption of his technique of stratigraphic excavation by the majority of those working in the field. It is quite correct to say that the collaboration of Nelson in the excavations at Castillo was a principal factor in developments that produced a revolutionary change for the better in the methods and theories of North American archeology (Willey and Sabloff 1993: 99–103).

Nelson went on to do research in Central Asia and the caves of the Yangtze. He was not involved again in Cantabrian prehistory, except perhaps incidentally. The same cannot be said for Osborn or for MacCurdy, both of whom returned to review Cantabrian research on other occasions. Under the direction of MacCurdy, the American School of Prehistoric Research sent a team of American students to

Cantabria again in 1929, where they visited the Prehistoric Museum in Santander, and the excavations of R. P. Jesús Carballo, the Museum's director, at the site of el Pendo (MacCurdy 1930: 5). That year, most of the "students" were themselves university professors and officials, including the then deans of Mt. Holyoke College and Clark University. Carballo invited the school to return and participate in his excavations the following year, and MacCurdy did so with another group, this time consisting of university and college students who were probably more willing to get their hands dirty. The roster of participants in the 1930 excavation at el Pendo reads like a *Who's Who* of Anthropology. Those involved were Lloyd Cabot Briggs, Jeanne Ernst, John P. Gillin, Robert Greenlee, Theodore McCown, Robert Merrill, John Z. Miller, Pachanan Mitra, Cornelius Osgood, Froehlich Rainey, Lucille Serrem, Sol Tax, J. Townsend Russell, V. J. Fewkes, and Robert W. Ehrich (MacCurdy 1931). As far as I am able to determine, those of the students who were anthropologists (the majority) without exception continued to work abroad, obtained higher degrees, and established major international reputations in later life.

There certainly must have been further visits to Cantabria by North American archeologists between 1930 and 1961, but apparently none remained to do extensive fieldwork until I (and later Henry Irwin) arrived in Santander in winter 1962. That summer (and the next), as a graduate student in anthropology at the University of Chicago, I had assisted F. Clark Howell in his excavations at the Acheulean butchering sites of Torralba and Ambrona on the Spanish Meseta. At that time Francisco Jordá Cerdá, then of the University of Oviedo, was the delegated Spanish co-director of Howell's excavations in Spain. Jordá, Spain's foremost authority on the Mousterian, invited me to stay in Spain to do my doctoral research on the Mousterian and the nature of the transition to the Upper Paleolithic. After a preliminary period of typological training in Talence, France, under the tutelage of my late friend and mentor, Prof. François Bordes, I returned in early winter to begin work. Cantabria is still a major international center for the study of the Middle and Upper Paleolithic, while the abundance of spectacular decorated caves there and the preeminence of Altamira in the study of Paleolithic wall art have given the name "Franco-Cantabrian" to this manifestation of creativity in Western Europe. At the time, the largest well-provenienced Mousterian collections in Spain were housed in the Museo Arqueológico Provincial de Santander, internationally famous as the home of one of the world's finest collections of Paleolithic materials. Jordá of course presented me to its new director, Dr. Miguel Ángel García Guinea, and its vice-director, R. P. Joaquín González Echegaray. Although I was generally well treated by all my museum hosts in Spain, and particularly so by Prof. Jordá himself, I was never received more warmly elsewhere than I was by the directors of the Santander Museum. They gave me a place to work, opened the Museum's warehouse to me, and spent hours giving me valuable advice, and discussing with me the peculiar characteristics of Cantabrian Mousterian collections and their feelings about their significance. The director and vice-director of the Santander Museum were internationally known, highly respected scholars, and there was a ferment of interest in archeology and history that I believe was unparalleled anywhere else in Europe at the time. At the heart

of its operations was the Seminario Sautuola, involving interested people from all sorts of backgrounds—doctoral students, high school students, businessmen, school teachers, laborers—who were eager to volunteer for the tasks that needed doing. The Museo was host to scores of world-renowned scholars from other countries whom it was an honor to meet, and their induction into the Seminario is still one of the most prized of their memories. Collaborating scholars from other disciplines gave freely of their time to help solve special problems beyond usual archeological expertise—I think particularly of Dr. Benito Madariaga de la Campa, historian, veterinarian, member of the Oceanographic Laboratory, and expert identifier of mammal bone and mollusks, and Sr. José María García Cáraves, banker, outstanding photographer and editor, and then director of the Centro de Investigaciones Submarinas—while the directors were more than generous in sharing credit for work accomplished and authorship of reports with all their collaborators regardless of status. There was also an unparalleled openness to new and innovative ideas and a tolerance for different methods that I have not met elsewhere. At any time, one might find speleologists there checking their equipment prior to an expedition, or tables full of tools being studied by archeologists, or a small group painting little slabs of limestone to try various media for the suspension of pigments, in an attempt to replicate Paleolithic techniques, or perhaps a team transferring the original tracing of a recently discovered engraving to a background in publishable form. There were lectures both planned and spontaneous. All the while, questions were being asked, opinions solicited, critiques offered, information exchanged. Despite the relatively free rein given to all sorts of activities, they produced organized results, rather than confusion, as is attested by the many scholarly publications that were their result. In the early 1960s, the Museo Arqueológico Provincial was a veritable paradise for the student of prehistory, no matter what his specialty or country of origin. While such scientific excitement has existed elsewhere, particularly at focused international symposia, which last a few days and then dissolve as participants return to their homes, the Museo managed to maintain that high pitch of productive work and excitement for several years before it finally changed.

Later that winter, the Harvard student Henry Irwin, whom I had met in Talence, came to Santander, looking for an opportunity to do field research on the Upper Paleolithic. Knowing my interest in Mousterian developments, the Santander Museum permitted me to clean the deep stratigraphic section at Castillo and take carbon-14 samples for analysis, and I was joined by Irwin in that operation. Later, with González Echegaray, I was permitted to dig a small test pit down into the pre-Mousterian levels at the same site. During that same period, I put a small test pit in a promising corner of Cueva Morín, in collaboration with Museum volunteers, and discovered that there were still intact sediments in the cave that would repay excavation. The provincial civil engineer, Dr. Alfredo García Lorenzo, also made it possible for me to study collections of Mousterian implements from the Cueva de la Flecha. When I returned to Chicago after the summer 1963 excavations at Torralba and Ambrona, I had more than ample information to complete my study of the northern Spanish Mousterian, whose central aspects were the Cantabrian collections. During

the summer of 1966, after my doctoral thesis had been completed, I was able to examine all the remaining Mousterian artifact collections from Spain. While I have undertaken several seasons of research elsewhere, both in Spain and abroad, always in collaboration with specialists from my host country, and usually under pleasant conditions, I have never found that research to be as congenial to me as has been my work in Cantabria.

I have been almost totally dedicated to Cantabrian research, and to a continuing collaboration with Dr. Joaquín González Echegaray, since our joint excavations at Cueva Morín in Villanueva de Villaescusa (1968–1969). When our test indicated that there was still much to be learned at that site, I made application to the National Science Foundation for a full-scale excavation to be conducted in collaboration with Echegaray. In the meantime, in 1966, he and Dr. García Guinea had undertaken a first and limited campaign of excavations there, in which Henry Irwin, by then teaching at Washington State University, and Antonio Gilman, now professor at California State University, Northridge, participated. With NSF funding, I returned to co-direct excavations at Morín with González Echegaray in the summers of 1968–1969. Participating scientists included the American geo-archeologist, Dr. Karl Butzer, the radiologist/dating specialist Dr. R. Stuckenrath from the United States, the palynologist Mme A. Leroi-Gourhan from France, the archeologists Dr. Paul Janssens from Belgium, Dr. B. Bender from England, Dr. Jesús Altuna (who studied our mammalian fauna), Dr. José María Apellániz, and Dr. B. Madariaga the malacologist, while several members of our field crew, then students, have since gone on to become professional archeologists. They include G. A. Clark, M. Conkey, K. Flataker Müller-Wille, John Fritz, and Major McCullough from the United States; S. Frankenstein from England; A. Moure-Romanillo, M. S. Corchón, and M. de los Angeles Querol from Spain. The fieldwork was most productive: it helped clarify the nature of Cantabria's peculiar cleaver flake-rich Mousterian, to eliminate supposedly transitional "Aurignaco-Mousterian" industries, clarified the nature of the causes of difference between the different kinds of Mousterian known as "facies" (Freeman 1994), discovered the first intact Chatelperronian level known from Spain, yielded evidence of structural complexes in both Mousterian and Archaic Aurignacian levels, and to our astonishment provided a series of Archaic Aurignacian burials, one of which contained a human body in an unusual state of preservation, its flesh represented in a fragile three-dimensional pseudomorph called a "soil-shadow." The discovery of this unique find, its transportation to the United States for conservation, and its eventual return in 1970 to the Altamira Museum, where it is currently located, involved collaboration between a surprising number of institutions: the Museo Provincial and the University of Chicago, who were responsible for the excavation; the Spanish Ministerio de Cultura, which issued the excavation permit and had to give permission for its temporary exportation; Sres Angel Bedía and Javier Echevarri's Santander boat-building establishment, that built the fiberglass and plastic container around the earth containing the burial; the Diputación Provincial de Santander, which provided the equipment and labor to remove the burial *en bloc* from the cave; the Museo Etnográfico de Muriedas (Casa de Velarde), where the encased burial was stored

awaiting transport; the Spanish and U.S. Health Departments, which had to issue and accept death certificates for this 30,000 (+) year-old individual; the U.S. Air Force, that flew the burial between Santander and Washington; the U.S. National Museum (Smithsonian Institution), where the underside of the pseudomorph was excavated by Freeman and González Echegaray, where it was studied by L. Angel and T. Dale Stewart, physical anthropologists of the Department of Anthropology, and where the whole was then embedded in plastic by John Widener of its Model Shop; and the Altamira Museum, where the embedded pseudomorph is now displayed. Excluding the various universities from which our student assistants have come, this is the largest number of institutions that have ever been involved in any of the research projects in which I have been engaged, to date. If there were any doubt about the efficacy of collaboration between institutions, regions, and nations across all borders, this case by itself should provide enough evidence to be convincing.

Research on Cueva Morín continued each year through 1970; our work is published in two large monographs and a compact book (González Echegaray and Freeman 1971, 1973, 1978), as well as several briefer articles, but some aspects of the analysis have continued periodically until today.

The important cave of el Pendo, in Escobedo (Camargo), was the site of renewed investigations during the years 1953–1957. These investigations were undertaken by the Seminario de Historia Primitiva of the University of Madrid, under the direction of its chief, Dr. Julio Martínez Santa-Olalla. The research was truly a large-scale, international collaboration, involving participants, many of them already accomplished professionals, from France, Holland, Belgium, and England, as well as Spain (in the latter group were some from Cantabria, including the Ingeniero de Caminos of the Diputación Provincial, Dr. Alfredo García Lorenzo, the young J. González Echegaray, and others). However, it was not until after Dr. Santa-Olalla's death that it became possible to study and publish their results. A team under the direction of González Echegaray was charged with that work in 1972. Dr. K. W. Butzer, then at the University of Chicago, was delegated for the geological study, and I was entrusted with the task of studying the artifactual materials from its important Mousterian levels. Other participants included the now-familiar names of González Echegaray, I. Barandiarán, M. Apellániz, C. Fuentes Vidarte, B. Madariaga, J. A. González Morales, and Arl. Leroi-Gourhan. Another of the original participants, the late A. Cheynier, made his field notes and observations freely available to us. This research was finally published in 1980 (González Echegaray et al. 1980). There is no doubt that had these fully modern investigations, whose methods were far in advance of their time, been published in timely fashion, their worldwide impact on the study of European prehistory would have been revolutionary as well as precocious.

I have continued to engage in field research in Spain, almost without a break, every year since the Morín excavations began. That project established the pattern for the continued, extensive, and productive collaboration between the two excavation directors that has continued unbroken since that time. One of the most significant research projects that we have undertaken is the 1980 re-excavation of Magdalenian deposits at the cave of Altamira (González Echegaray and Freeman 1996), and the

detailed restudy of its magnificent decorations: our results are soon to be published by the Maison des Roches in France. (In many countries, no non-citizen would have been permitted to collaborate in the excavation of such a significant national monument.) The reanalysis of the cave's superb figures led to the rectification of some previously erroneous species identifications, and to the discovery of unsuspected principles of symbolic organization and aspects of meaning of the decorations (Freeman et al. 1987). Certainly from the standpoint of its duration and the abundance and quality of the information it provides, our most important project is our research at the Cantabrian Earlier Magdalenian cave site of el Juyo (1978–1997), of which major aspects are already published and others in preparation. Scientists who collaborated in the research included I. Barandiarán Maestu, of the Universidad de País Vasco in Vitoria; J. Altuna, of the Museo de San Telmo, in San Sebastián; M. Hoyos, of the Instituto Lucas Mallada, Museo Nacional de Ciencias Naturales, Madrid; J. Fernández Tresguerres, of Oviedo; J. L. Casado Soto, of the Museo Marítimo, Santander; Mme. Arl. Leroi-Gourhan and A. Boyer-Klein of the Musée de l'Homme, Paris; S. Porter of the University of Washington; Richard Klein, then of the University of Chicago; Wm. Crowe, then of Chicago's Field Museum of Natural History; F. Santamatielde from Santander, who served as our staff photographer; and J. Ogden, of Walnut Creek, California, our staff artist. Visiting professionals, including B. Bronson from the Field Museum, L. Keeley from the University of Illinois at Chicago, and F. Harrold from the University of Texas at Arlington, also took part in the excavation. Our research at el Juyo contributed in at least some small way to the formation of too many who became professional prehistorians or anthropologists for me to list them all, but among them are the Spanish scholars F. Bernaldo de Quirós, C. González Sáinz, V. Cabrera, M. González Morales, M. del Carmen Márquez Uría, E. Baquedano, M. Dolores Herrera, M. de la Rasilla, Sergio Ripoll, Silvia Ripoll, M. del Carmen Gutierrez, R. Doce, and Monica Ibáñez, and the North Americans F. Gleach, M. Rosenthal, H. Stettler, J. Pokines, and K. Cruz-Urbe. From 1987 on, the el Juyo excavation also served as a field school, and over the course of its eleven-year duration, it provided a basic training in modern methods, theory, and results of Paleolithic research to some 150 students, many of whom have gone on to obtain advanced degrees in the professions. In the year before the field school began work at Juyo, its North American students participated, with Wm. Crowe and M. Ibáñez, in Dr. Victoria Cabrera's excavations at the cave of Castillo.

I would be remiss were I not also to mention the fruitful collaboration between Cantabrian and North American investigators in the absolute dating of the earliest Upper Paleolithic complexes in the region: complexes that unexpectedly have proven to be as early as any early Upper Paleolithic levels elsewhere in Europe (Cabrera and Bischoff 1989).

In 1983, stimulated in part by the intellectual ferment I had witnessed as a member of the Grupo de Trabajo de la Prehistoria Cántabra (a highly productive informal association of investigators that originated in the 1970s, but disappeared soon thereafter), together with W. Crowe and the lawyer Ralph E. Brown I helped found the Institute for Prehistoric Investigations in Chicago, and at the same time González

Echegaray formed the Instituto para Investigaciones Prehistóricas in Santander. It was always our intention that these two organisms, one Spanish, the other North American, should work in close collaboration, and although both have grown larger than they were originally, that has been the case ever since. The two institutions have shared personnel, facilities, and resources in ways that have been highly productive, and their research has resulted in many scholarly publications. In addition, the two institutes have financed the publication of the work of other scholars, sometimes in fields other than prehistory, and the Instituto in Santander maintains an extensive research library for the use of its collaborators.

In addition to its own investigations of Cantabrian prehistory on land, the Instituto's Laboratorio para Investigaciones Arqueológicas Subacuáticas, directed by Dr. J. L. Casado Soto of the Cantabrian Maritime Museum, is engaged in ongoing research into the maritime history of Cantabria and of Spain in general: it has discovered and excavated parts of the Roman port installation in Santander, and explored important wrecks in the bay and off Castro Urdiales. In 1986–87, Dr. Casado Soto, Dr. Manuel Martín Bueno of the University of Zaragoza, and I undertook a research program in the Ría de San Vicente, funded by the Comité Conjunto Hispano-Norteamericano, excavating a stratified offshore anchorage whose deposits bracket the later Middle Ages and the early modern period, including the time of Columbus's discoveries. These materials are especially interesting due to the role of *chalupas de San Vicente* in Columbus's voyages (Casado 1992; Casado and González Echegaray 1995).

IPI has more recently been engaged in the study of Cantabria's history, particularly concentrating on textual and artistic documents of the Middle Ages. My colleagues J. González Echegaray, A. del Campo Hernández (of the University of Cantabria), and I have translated into Spanish and English and commented on the text of the entire corpus of works of the celebrated Cantabrian eighth-century churchman, St. Beatus of Liébana; the first Spanish volume resulting from that collaboration was published by the Biblioteca de Autores Cristianos in 1995 (González Echegaray, del Campo, and Freeman 1995); IPI participated as well in the earlier publication of del Campo's translation of Apringius of Beja's commentary on the Apocalypse (del Campo 1991). We have also studied religious symbolism as manifested in the art of the miniatures illustrating Beatus's *In Apocalypsin*, and in the sculptures of Cantabria's Romanesque and Gothic churches.

Among my North American students I am proud to count several archeologists who have gone on to do further research in Cantabria as professionals. In 1971, G. A. Clark completed his study of the Asturian complex in Cantabria (Clark 1976). Together with another graduate student at Chicago, Lawrence Straus, he conducted an intensive survey of the Upper Ebro and Arlanzón valleys in northern Burgos on the edge of Cantabria in 1972. Straus, whose 1975 doctoral thesis is a study of the Solutrean in Cantabria and the Basque country (Straus 1983), has been the most constant in his involvement with Cantabrian prehistory, having taken part during 1973–1974 in the excavations of Cueva Chufin (Riclonés), directed by F. Bernaldo de Quirós and V. Cabrera, and in those at Rascaño undertaken by González Echegaray

and L Barandiarán (1981). Margaret Conkey studied design elements on Magdalenian engraved bones for her doctoral thesis at Chicago, presented in 1978. Her research permitted the tentative identification of Altamira as a site for periodic aggregation by people who lived apart in other caves during other seasons, and her ideas have had considerable impact on the thinking of other investigators (see Conkey 1980). Another student, F. Harrold, though he did not take part in the Morín excavations, incorporated some of their results in his 1978 doctoral thesis on the Chatelperronian.

The doctoral research of James Pokines was fundamentally based on Cantabrian materials: the small fauna from the Magdalenian levels at el Juyo, compared to modern collections he made in Cantabria. His thesis was accepted in 1997, and is the basis for his 1998 monograph, published by British Archaeological Reports. His findings have advanced the reconstruction of paleoclimates and environmental change between 14,000 and 15,000 years ago. In 1998, Heather Stettler was also awarded a Doctorate in Anthropology. Her thesis (1998) involved a study of decorative motifs on bone artifacts, their distributions, and their changes through time, in Cantabrian Paleolithic sites. Her master's thesis was based on the study of the distributions of seeds and other macrobotanical remains in the el Juyo levels, and Pokines's master's research on bone weapons and implements from the same site. Both spent extended research periods in Cantabria, aside from the time involved in assisting in our excavations and those of Cantabrian investigators, and both benefited from working in close collaboration with other scholars from Cantabria and elsewhere. Since both these professionals also have extensive experience elsewhere, it is still too early to tell whether they will continue to devote themselves to research in Cantabria, as I would hope.

In close collaboration with a colleague from the University of Santander, Dr. M. González Morales, Dr. Lawrence Straus, now professor of anthropology at the University of New Mexico, is currently engaged in excavations at the exceptionally important Upper Paleolithic site of the Cueva del Mirón, near Ramales: their work, involving flotation and the most advanced electronic techniques for data recording and analysis, is expected to set a new high standard for Paleolithic research worldwide. Those excavations are training a new generation of investigators, many of whom will become professors in Spanish and North American universities.

The work of my university, and later, that of the Institute for Prehistoric Investigations, has always been undertaken in strict collaboration with other research institutions: the Santander Provincial Museum, the Casa de Velarde, the University of Cantabria, the Museum and Research Center of Altamira, and the Instituto para Investigaciones Prehistóricas have been among the most important of these during my Cantabrian career. It has always involved other scholars with expertise in fields different from my own competences. In today's complicated world, no one can hope to master all fields. The funding for our research has always been international as well: the U.S. National Science Foundation, the Diputación Provincial de Santander, the Comité Conjunto, the Excelentísimo Ayuntamiento de Camargo, the University of Chicago, and private donations through the U.S. Institute for Prehistoric Investigations have been our largest sources of support, and in most

cases our research has required funding from multiple sources, not just a single one. Without such cooperation on an international scale by granting agencies, there is no way that sufficient funds for a large and extended prehistoric or historic research project could ever be accumulated.

The benefits of collaborative research across international boundaries accrue to all parties involved. Obviously, without access to sites and collections in Cantabria, my own research could never have been carried out. Without the facilities and infrastructure that have always been provided for that research by Cantabrian institutions, it would either have been impossible or impracticably costly. Without a continuing infusion of information from my Cantabrian colleagues about such subjects as stratigraphic interpretation, site formation processes, excavation methodology, raw material sourcing, faunas, artifact typology, the names of local suppliers and reconditioners of equipment, and the “politics” of science on the local, regional, and national scale, there is always a possibility of misperceiving or ignoring essential connections between data and theory, having an irremediable equipment loss or failure, or having difficulties with other people, that may range from misunderstandings with local communities to insurmountable obstacles stemming from the resentment of other professionals, government officials, or the press. I frankly might never have discovered the added dimensions made available to our understanding of past systems of belief and behavior by the imagery that ancient peoples employed, and by the organization of that imagery, had González Echegaray not insisted that Paleolithic wall art could not responsibly be ignored. Had I not heeded his urging, I would never have learned how rich, complex, and informative is the field of symbolic information available in Paleolithic art, or in the religious texts and art of Medieval Spain.

On the other hand, I believe that contact with North American prehistorians and others concerned with reconstructing vanished lifeways in all their aspects and relationships has benefitted our Cantabrian colleagues. This interchange of ideas has naturally been facilitated by the fact that we have all made a concerted effort to publish our results in Spanish as well as in English, an obligation we all take very seriously. Familiarity with the ideas of scholars interested in reconstructing the ways people behaved and felt in the past through the study of the distribution of finds within levels, familiar with the benefits and limitations of formal and quantitative analysis, and wary of the pitfalls of uncritical reliance on microstratigraphy, has saved the discipline of prehistory as practiced in Cantabria (and Spain in general) from the sterility of the purely “geological” approach to analysis. That approach has been typical of much of Old World prehistory as practiced elsewhere. It has also saved it from the statistical naïveté of those who assumed that visual examination of percentage lists, “*the statistical method*” to some, could be adequate to distinguish statistically significant differences between assemblages of artifacts or contextual materials. Despite the fact that it has repeatedly been shown to be false, the idea is still widespread elsewhere in Europe that the deposits in a prehistoric site were almost always laid down in regular and uniform fashion, from oldest on the bottom to youngest on the top, and that any stratigraphic exposure through a site’s deposits should be equivalent or identical to any other. Those who subscribe to this misguided

idea believe that the study of prehistory can only progress by making finer and finer subdivisions of stratigraphy that will result in an ever more precisely resolved and reliable view of the sequence of paleoenvironmental and climatic changes that took place in the region through time. To such excavators, sinking a narrow shaft—anywhere—throughout the depth of a deposit seems the logical way to proceed. I do not of course deny the undoubted importance of careful stratigraphic and temporal control. But at every site I have excavated, in Cantabria or elsewhere, there are gaps in deposition (often, as at Morín and Juyo, these are due to the complete removal of strata during periods of Paleolithic building and “housecleaning”), or there are other kinds of accommodation of the site by its ancient occupants, including even the complete inversion of temporal sequences of deposition (as often happens when earth dug from a pit or hut foundation is thrown out on the adjacent site surface: this can result in the redeposition of strata *in reverse order*). Natural processes also result in closely adjacent stratigraphic sequences that are incomparable. Temporal control must be maintained, but to understand how prehistoric people lived and how they were using a site, large horizontal exposures of materials in a single natural level are absolutely essential. Just as an example, there are hut foundations inside many Paleolithic caves: to understand how they were constructed and used it is not enough to excavate a small corner of one of them; one needs to see all or most of their floor plans. North American excavators long ago learned this lesson, and their Cantabrian colleagues have been among the first to appreciate it. Cantabrian prehistorians have understood that where the remains they excavate are intact, they record past cultural behavior, and cannot simply be understood as so many geological strata. They have seen that to reconstruct a more truthful picture of the past, its human inhabitants, their uses of space, and their effects on their habitat must be included. At the same time, they have also understood that conclusions about such matters must be based on evidence recovered from the ground, not on a priori schemes, and that no amount of model-building or philosophical speculation can provide reliable answers. That places Spain, and particularly Cantabria, at the forefront of research in the Old World.

When I began Paleolithic research in Spain in the 1960s, the field was dominated by a few strong personalities. Fortunately for the future of the field, those in charge of “official” prehistory in Spain, though some may have considered them rigidly unreasonable in other respects, always heard me out with tolerance (as they also did other North Americans, both experts and tyros). They showed themselves to be admirably open-minded and receptive to approaches to the past and new ideas about methodology and interpretation, provided only that they could be shown to be worthwhile. Their students (many of whom, I am proud to say, have also worked with me) are now themselves the leading figures in the field, and having been trained in that atmosphere, are proving themselves to be as innovative and productive as any investigators in the world.

Any respectable modern program of prehistoric investigations and analyses always requires extensive interdisciplinary collaboration, and that almost inevitably means that scholars from more than one country will be involved. There can be

exceptions, where investigations are conducted by a national research laboratory whose staff includes all the kinds of specialists the laboratory deems essential, but such institutional structures can be quite restrictive: to eliminate competition between staff members with similar interests, they may assign different researchers to specific delimited regions; they sometimes impose their own nationalistic or philosophical viewpoint on investigations; they can also be intolerant of what the relevant hierarchy sees as inessential innovations. Spain has not historically been so restrictive, though some of her autonomous regions today are jealous of “intrusion by outsiders” whether domestic or foreign. Cantabria, fortunately, is not one of them.

The story of the human career knows no national boundaries. Xenophobia and nationalistic chauvinism have no place in science: there can no more be a nationalistic prehistory than there can be a nationalistic mathematics, astronomy, or theoretical physics. The subject matter we study is the story of all mankind, and we are all equally the inheritors of the evidence on which its study is based. It is greatly to the credit of Cantabrian scholars that they have not only understood that principle, but put it into action. Our discipline has made substantial advances in Cantabria on that account, and my students and I look forward with great pleasure to long-continued collaboration with our Spanish colleagues in research there.

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Afterword

Because I strongly favor international and interdisciplinary research collaboration, more of my publications are co-authored than is the norm. For example, I have published a great deal about the Upper Paleolithic, based on research at the caves of el Conde, Morín, el Pendo, el Juyo, and Altamira. I have also written articles about measurement, education, statistical methods, and improvements in techniques for data recovery, most of which appeared in papers or books co-authored with J. González Echegaray or others. Fascinating though I think the results of such work may be, I have avoided republishing co-authored articles in this book so that its faults will be entirely my own. In recent years, I have been increasingly concerned with the study of Medieval religious symbolism, research that is outside the limits of this book. Consequently, the chapters presented in this volume are only a small selection from the much more numerous papers that I have published (alone or with others) during the course of my career. I believe that they are a representative cross-section of my thinking about the Paleolithic past. I found these pages quite interesting while I wrote them and am confident that my audience will also find something of value here.

The reader will have noted that none of the articles I have chosen deals with gender in prehistory. Despite the fact that I helped train one of the most active and

famous of the “feminist” archeologists, no concerned party will have found anything here concerning gender roles in the accumulation of prehistoric residues. I have no doubt that stone tools or cave paintings could have been produced by Paleolithic people of *any* sex (and many ages), but the evidence we would need to *prove* that any activity was performed by either men or women is simply lacking. I am not just trying to avoid the issue. In other publications, I have tried to suggest that women’s roles in prehistory were as important as men’s and to state why I think that must be the case. In fact, in research at el Juyo I indicated the reasons why I thought the complementarity of sex roles was indicated by evidence from the “sanctuary.” But on that occasion I was surprise-attacked by my feminist students for having dared to suggest that a particular range of activities—sewing, shellfish gathering, and so forth—had traditionally been ascribed by ethnographers to women. I did not insist on that interpretation of the el Juyo evidence, but my assertion about previous ethnographic observations was quite correct. If anything, my students should have criticized me for (inconsistently) using ethnographic analogy.

One might be inclined to ask why an archeologist trained in the United States decided to work mostly outside the Americas. That puzzled the late William A. Ritchie, who was New York State Archaeologist when as a student I was a crew member on his excavations. Years later, he asked me why I chose not to work in “my own home state.” I explained that I was interested in the behavior of our very early ancestors, which I could not study in New York. He seemed to find that answer more or less satisfactory, but he clearly had his reservations. The chapters in this book would have made my reasons clearer. I only regret that Bill cannot read them now.

The papers I have chosen for inclusion here reflect to some extent the development of my thought about the Paleolithic. I formulated some of the ideas expressed here, including those about the uses and abuses of substituting analogy from some ethnographically known group for reasoning from the real archeological evidence, and the need for more care in interpreting archeological “food remains” at a time when I was still a graduate student, while others only occurred to me later, in the course of what has been a long professional career. I have long been aware that before one can study a phenomenon, one needs to know what it is and how many kinds of it there are. Some of what has been seen as my “legalistic” inclination is really due to an intense concern for defining terms and for pushing recovery techniques as far as possible, given constraints on time and available resources.

The chapters in this collection have been arranged to lead the reader through the somewhat twisted pathways of my thinking about prehistory. Starting with more theoretical meanderings that are more or less divorced from the Paleolithic context, they proceed through considerations of the Paleolithic in general and then the particular Cantabrian situation, to a more specific treatment of each of the three major Paleolithic industrial stages: Lower, Middle, and Upper. The chapters on the Upper Paleolithic are based on my study of Paleolithic art; I have excluded many papers on my excavation experience for reasons stated above. The last chapter discusses the benefits of international collaboration in scientific research, a topic about which I feel very strongly.

These chapters make no claim to present all sides of discussions about my conclusions or to represent the latest tendencies in archeological research. I believe that those conclusions about the Paleolithic in general, the meaning of residues from Torralba, the nature of Mousterian facies, the irrelevance of artifact similarities or differences to the study of genetic relationships between hominids, the significance of decorations in the cave of Altamira, and others are correct and will eventually become part of mainstream archeology, even those where there is now some disagreement about particular interpretations. Whatever the case, I have been a producer rather than an armchair consumer of archeological data for more than forty years, and I offer the product of the researches synthesized here as a permanent part of archeology for others to consider and study.

When I began Paleolithic research in Spain in 1962, it seemed as though one could count the number of Spanish and foreign scholars working in the same field on one's fingers, and the majority of them, mostly those of the older generation (who held down the few available museum positions), had attitudes and used methods that had pretty generally been discarded elsewhere by the time of the Spanish Civil War. That has now changed for the better, and a vast increase in university positions has provided employment for dozens of younger scholars, many of whom have as modern an outlook (and use tests that are also as up-to-date) as do their counterparts anywhere. I take some pride in the fact that they generally think about the past in the same way that I do, even when they seem to be unaware of that fact. I am confident that their research will involve innovations that lead to a revolution in our understanding of the past.

As I said in the foreword, I hope that some of these chapters will stimulate further Paleolithic research. The future of our discipline is in the hands of a new generation. Perhaps those younger investigators will find that I have indicated some dead ends that lead no further and that they should not follow, or that I have shone a fitful light along some more productive paths. That, in any case, is my ardent hope. If I have succeeded I shall be amply rewarded.



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