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The reconstruction of Upper Paleolithic adaptations: The Biscayan regions as seen by an archeologist from North America.

La reconstrucción de las adaptaciones del Paleolítico Superior. Las regiones del Golfo de Vizcaya vistas por un arqueólogo norteamericano.

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PALABRAS CLAVE: Paleolítico superior, Paleoeología, Nueva Arqueología, Gascoña, Euskadi, Cantabria, Asturias.

RESUMEN

Esta comunicación resume algunos acontecimientos recientes en la construcción de las teorías acerca de las adaptaciones y las estrategias de movilidad de los cazadores-recolectores por L.R. Binford y otros representantes de la «Nueva Arqueología» norteamericana. Después describe el estado actual de nuestros conocimientos sobre las relaciones humano-medio ambiente en el Paleolítico superior de las regiones limítrofes del Mar Cantábrico de Francia y España, basándome en las investigaciones arqueológicas y de las ciencias naturales de las últimas dos décadas. Propongo un modelo descriptivo y sugiero unas hipótesis para explicar los cambios adaptativos mayores en la secuencia prehistórica regional para estar probadas por investigaciones futuras.

SUMMARY

This paper summarizes recent developments in the construction of theory concerning hunter-gatherer adaptations and mobility strategies by L.R. Binford and other exponents of North American «New Archeology». Then it describes the state of our current understanding of Upper Paleolithic human-environment relationships in the regions of France and Spain bordering the Bay of Biscay, based on archeological and natural science research conducted during the last two decades. A descriptive model is proposed and explanatory hypotheses for major adaptive changes in the regional prehistoric record are suggested for further systematic testing.

1. INTRODUCTION

Prehistoric archeology can be said to have passed historically through *phases* of description and dating, culture history, and culture process. In reality, these are all *aspects* of prehistoric research as conducted in the present period. The ultimate goals of all prehistoric research are explanatory; the debates of at least the last two decades have concerned the *nature* of archeological explanations of past human behavior. Those debates have fundamentally pitted materialists against idealists, which is ironic as the subjects of all archeological studies are material remains not ideas, not even people. We use material remains as proxy data to try to learn about prehistoric human activities and, conceivably, beliefs. The vigorous trans-Atlantic discussions which have characterized the archeology of the post-World War II period have forced all researchers, not just the participants in the theoretical polemics, to *define* their ideological focus and pragmatic approach to archeology. Thus, at least in the recent Anglo-American and French literature, one encounters increasing references to logical positivism, functionalism, cultural ecology, cultural materialism, Marxism, structuralism, symbolism, etc.

It is my point of view, one I believe widely shared by working archeologists in both the New and Old Worlds, that the goals of prehistoric research should be to reconstruct and explain the operation of extinct human adaptive systems in their environmental contexts. By achieving these goals, it is also my feeling that we can contribute to the general understanding of processes of culture change. Given this framework, I regard archeological excavations as peculiar, semi-replicable experiments in the study of human behavior. This view stems from the basic, albeit modified, acceptance of a Hempelian logico-deductive approach to science. The vision of archeology as a unique form of experimental science is tempered by adherence to the archeological ethic, so heavily emphasized in post-1970 North America, whereby excavation must attempt to collect and preserve as much information as possible while sites are being destroyed by the researchers. A paradox of the «new archeology» when combined with the «archeological ethic» is that one cannot know what «facts» to collect in the absence of specific theories. «Facts» are not collected without reasons; reasons are supplied in a theoretical context. One cannot, therefore, collect *all* the facts about an archeological site, only those for which one can perceive some theoretical relevance based either on one's own specific research interests or on a general archeological and paleoenvironmental education.

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Nonetheless, we do have an ethical obligation to not willingly overlook or destroy information which we perceive and which we realize to be of potential interest to other workers with problem orientations somewhat different from our own.

If one accepts the proposition that archeologists cannot really «get inside the heads of prehistoric people» (a proposition obviously not accepted by the followers of structuralism and symbolism in prehistory), then one faces the question of «why» to reconstruct behavior. Many have been seduced (by spectacularly well preserved, single-component sites and by special analytical techniques such as lithic replication, microwear and refitting) to see the reconstruction of «moments in time» as the ultimate goal of prehistoric research. This is a *descriptive* goal, ultimately no less so than the classification per se of artifacts or of assemblage types (archeological «cultures» and «phases»). In addition, such exercises are particularistic in nature, although they can make for popularly pleasing scenarios, such as «a day in the life of a Magdalenian».

Alternately, the goal of archeological reconstruction can be nomothetic in nature and, in that sense, it can contribute to the building of the general science of human behavior. Prehistory uniquely (and in contrast to ethnography) provides the social sciences with time depth and the ability to study long-term change. Prehistory sacrifices detail (as it must) in favor of coarse-grained treatment of major trends in adaptive patterns. There is scant place for the individual person in the broad sweep of prehistory, except as the occasional skeleton to be analyzed anatomically, paleopathologically and dietarily. The study of general laws of human behavior assumes that the subject matter of archeology is not idiosyncratic. Indeed, the truly unique and idiosyncratic phenomena of the archeological record are ultimately irrelevant.

The high hopes of the theoretical revolution of 1965-1975 were tempered within this last decade by the growing realization of the complex nature of the archeological record. Even at the most pristine of sites cultural remains cannot be directly interpreted as «fossilized human behavior». The extant archeological record is in each case the result of a combination of site role/activities, disposal behavior, reuse/recycling, and natural formation/disturbance processes. Recognition of the complicated relationship between archeological debris and human behavior led some to despair of ever achieving the goal of contributing to the explanation of broad scale, general culture change. Instead, practitioners of the so-called «behavioral archeology» have taken the study of disposal behavior to be *the goal* of research (not simply a tool leading to further, higherlevel analyses). This is similar to the particularistic aims of those

who see archeology as the study of material objects for their own sake. These archeologists would, in effect, return the discipline to the normative paradigm under which *things* not *adaptive* systems were the subject matter, to at most be taken as indicators of culturally mediated mental templates and thus either deliberate or *de facto* symbols of ethnic identity.

At this stage, the long-standing debate over ethnographic analogy and its relevance to prehistory takes a vital turn in the evolution of processual archeology. This comes about as L.R. Binford begins to argue for the need to establish base lines for study derived from the application of uniformitarian principles to such phenomena as ungulate anatomy, butchering options, disposal patterns determined by human anatomy, structural arrangements, numbers of participants, quantity and quality of debris, etc. There are certain physical facts which apply equally to the present and to the prehistoric past, at least in so far as it concerns *Homo sapiens*, which can allow us to use our knowledge of the present to unravel patterns in the archeological residues. This realization has led to productive ethnoarcheological work (of the etic variety) among such groups as Nunamiut Eskimo, San (Bushmen), Navajos, and Australian Aborigines. Eskimos have been studied by BINFORD (1978, 1981) for example, not because he feels that «Mousterians» or «Magdalenians» were like them in all respects, but because certain elements of the Eskimo situation can arguably hold to be constants (e.g., *Rangifer* anatomy, camp features such as hearths, human body size). Comparisons are not motivated by the sort of 19th century unilineal cultural evolutionism which saw no problems with drawing direct, total analogies between Paleolithic Europeans and contemporary Eskimos. Rather they are justified by reasoned applications of the uniformitarian principle to *specific aspects* of behavior which leave behind material correlates referable to law-like statements relevant under both modern and ancient conditions of life.

Ethnoarcheological research has led to some basic cross-cultural generalizations particularly regarding disposal patterns at different kinds of hunter-gatherer camps. While the relationships between residues and site functions are not direct and often involve the less obvious kinds of remains (e.g., lithic debitage and faunal remains rather than finished tools), the ethnoarcheological record is not anarchic. Regularities exist, facilitating the interpretation of archeological sites.

An additional body of knowledge is required in order to deal with archeological contexts: the processes which have affected sites since the time of abandonment. A complex business, the study of site formation/disturbance processes begins with «actualistic» experiments such as the creation of artifi-

cial sites to monitor the effects of sun, water, mud, freeze-thaw, gravity, animals, etc., the observation of carcass dismemberment by carnivores/scavengers, and the conduct of such exercise as trampling, burning, butchering, lithic tool replication, use and resharpening etc, under controlled conditions. In some respects, this kind of research is an extension of the «experimental archeology» pioneered principally in Britain several decades ago, but it obviously goes well beyond the study of the decay and erosion of structures. As focused in the Binfordian sense, ethnoarcheological and experimental studies attempt to build bridges between material facts and the behavioral patterns originally responsible for their creation. This constitutes what is called «middle range research» (BINFORD 1977, 1983) and the patterned results constitute a growing body of «middle range theory» —the link between «static» objects and «dynamic» behavior, to use Binford's favorite description. Once regularities in the relationships between «statics» and «dynamics» can be perceived in the present world and their causal relationships and conditioning factors determined, then archeologists can begin to apply this uniformitarian knowledge to the prehistoric past, for which we have only «static» remains which are the results of human activity and post-occupational natural processes.

One further recent advance by the «new archeology» should be mentioned by way of introduction to this discussion of the Basque Upper Paleolithic. It is the understanding derived from worldwide ethnographic studies that hunter-gatherer adaptive strategies can be ideally categorized into two basic types: «forager» and «collector» (BINFORD 1980, 1982). It is not necessarily the case that any one human group will fit into one or the other of these types of systems all the time, although different environments, demographic densities and possibly even evolutionary grades of early hominids will tend to have associated with them one type of organizational pattern as opposed to the other. Foraging refers to the relatively unspecialized gathering of food and other materials within a short radius of a residential base camp without elaborate planning or much storage of foodstuffs. Foraging is generally done on a daily basis; when the resources surrounding a residential camp are expended, the entire group («band») moves its camp to another location in what BINFORD terms «residential mobility» —moving consumers to resources—. This is similar to the «circulating» settlement pattern envisaged by Marks and FRIEDEL (1977) in their study of Paleolithic adaptations in the Negev region of Israel. This is a relatively simple system, particularly feasible under conditions of equable, relatively temperate, humid climate, with more or less evenly distributed resources and fairly low population densities. It is, in the

ethnographic present, more characteristic of hunter-gatherer groups living at low latitudes.

«Collecting» in BINFORD'S terminology involves moving resources to consumers by sending specialized, often well-equipped logistical parties to acquire specific food (and other) resources at particular locations (e.g., hunting sites, quarry sites, etc.). This form of behavior requires careful «gearing-up», planning and organization, as well as accurate information about resource location and state. Naturally other activities (information gathering, lithic collection, etc.) can be «embedded» into logistical trips such as hunting expeditions. Such sophisticated behavior is typical of high latitude hunter-gatherers often engaging in storage as a strategy for year-round survival under harsh conditions, particularly when key resources (e.g., herd ungulates) may be aggregated and patchy. MARKS and FRIEDEL independently coined the term «radiating» settlement to characterize the archeological result of such a system. Major residential sites may be surrounded by series of minor, specialized, logistical locations often at considerable distances from the base camps. These ideal constructs are useful in attempting to understand the operation of Paleolithic adaptive systems under varying conditions which prehistorians must first reconstruct with the collaboration of natural scientists.

2. THE DEVELOPMENT OF BISCAYAN PALEOLITHIC PREHISTORY

Prehistoric investigations of a serious nature began roughly simultaneously in Pyrenean France and Vasco-Cantabrian Spain in the last third of the 19th century with the work of E. and L. LARTET, E. PIETTE, the Conde de LERSUNDI, M. SANZ DE SAUTUOLA, J. DEL CASTILLO and others. These researchers established the existence of Stone Age sites (and art) in the Pyreneo-Cantabrian region and began the work of creating a cultural sequence based on supposedly temporally characteristic faunas or artifacts. On both sides of the international border, but especially in northern Spain, it was the first decade and a half of this century which saw a proliferation of major, systematic excavations of sites which, to this day, remain keystones in the Middle and Upper Paleolithic culture-stratigraphic sequence of the southern sector of the Franco-Cantabrian region. These sites included Isturitz, Santimamiñe, El Castillo, Cueto de la Mina and many others, and the cast of prehistorians included such illustrious figures as E. PASSE-MARD, COUNT L. BÉGOUËN, T. de ARANZADI, E. EGUREN J.M. de BARANDIARÁN, A. de GÁLVEZ CAÑERO, L. SIERRA, H. ALCALDE DEL RIO, J. BOUYSSONIE, H. OBERMAIER, H. BREUIL, Conde de la VEGADEL SELLA and E. HERNÁNDEZ-PACHECO. Along with massive, very fruitful excavations, these workers were responsible for the vast bulk of cave art discoveries (and publications) in the

region. This «heroic age» of research was abruptly ended by World War I (though not completely in Spain where, however, the research of Fathers BARANDIARÁN and CARBALLO did end twenty years later with the outbreak of the Civil War). The concerns of this period were to discover, document and date finds in respect to the developing relative chronological synthesis of the Abbé Breuil. In the French sector, such research continued between the World Wars, notably at Isturitz and Lespugue by Count and Countess de Saint-Périer and at le Mas d'Azil by St. Juste and M. de Péquart.

In the post-World War II period «normative» research has certainly continued with major excavations in Le Portel, La Vache, Aitzbitarte, El Pendo and La Lloseta, but new techniques of excavation and analysis and, sometimes, new research goals have been involved. These included the rationalization and standardization of lithic and bone artifact typologies for the Upper Paleolithic by D. de SONNEVILLE-BORDES, G. LAPLACE and I. BARANDIARÁN. Gradually, beginning in the 1960s, radiocarbon dating has been applied to Upper Paleolithic deposits, although its full significance as an *independent* chronological yardstick has not yet been fully realized. Modern excavation methods became the norm. Interest in Upper Paleolithic lifeways (although sometimes as an adjunct to chronological concerns to assign artifacts to Last Glacial climatic phases) arose with the creation by F. BORDES of the Institut du Quaternaire at the University of Bordeaux (working in the Pyrenees in collaboration with C. CHAUCHAT, J. CLOTTES, COUNT R. BÉGOUËN and R. ARAMBOUROU), together with the formation of a prehistory and paleontology section in the Sociedad de Ciencias Aranzadi around the figure of J.M. de BARANDIARÁN in Donostia (notably through the work of J. ALTUNA) and the organization of a multi-disciplinary, Hispano-American excavation of Cueva Morín by J. GONZÁLEZ ECHEGARAY and L.G. FREEMAN. «Science in archeology» arrived with the application of palynological and geomorphological analyses, first by Arl. LEROI-GOURHAN, M. M. PAQUE-REAU, C. THIBAUT and K. BUTZER. The «Aranzadi» group, ever active in exploration, excavation, analysis, training and publication, remains the only true interdisciplinary paleoanthropological *institution* in the region.

Little by little in the late 1960s and 1970s, a few individuals (some influenced by the famous Bordes-Binford debate) began to question the significance of interassemblage variability in the Upper as well as Middle Paleolithic and breaking out of the strict culture-phylogenetic paradigm. Others (notably F. DELPECH and J. ALTUNA) began to see mammalian remains at Last Glacial archeological sites as more than just chronological indicators, but rather as evidence of Upper Paleolithic subsistence gathering

techniques. Ultimately, perhaps somewhat influenced by Anglo-American developments in faunal analysis («British economic prehistory» and Binfordian-Flannerian «cultural ecology»), they began to conduct very thorough analyses beyond simple species identifications (e.g., body parts, seasonality indicators, fragmentation studies, age and sex determination, body size, etc.). One could suggest that, as in the United States with the close wedding of ethnography and archeology, under the BARANDIARÁN School, Basque prehistory was always concerned with how people lived - including their subsistence. B. MADARIAGA began asking questions about the role of Upper Paleolithic and Mesolithic mollusc exploitation in the Cantabrian Region. G.A. CLARK and G.N. BAILEY also took up where the Conde de la Vega del Sella had left off in the behavioral interpretation of Asturian shell middens.

3. NEW PERSPECTIVES ON THE UPPER PALEOLITHIC OF VASCO-CANTABRIA AND GASCONY.

The brief recounting of the post-war developments in prehistory along the 43rd parallel leads to the question as to whether there is any relationship between the theoretical rumblings in Cambridge, Southampton, Ann Arbor, Chicago and Albuquerque, and the actual conduct of Stone Age research in the Franco-Cantabrian region and if so, what might its nature be. I am frankly somewhat ambivalent in this regard. It is true that I was trained at the Universities of Chicago and Michigan and in the field at the famous Carter Ranch in Arizona (among other places) by «new archeologists» and «cultural ecologists» and it is true that I have been a close colleague of Binford for over a dozen years at the University of New Mexico. On the other hand, it is equally true that my maternal grandfather and great-grandfather were early prehistorians in the Charentes (southwest France) and that my early education was humanistic and historical in nature. Thus, I am a living example of some of the schizophrenia which I believe affects many of us «Anglo-Americans» (though to my knowledge I haven't one drop of English blood in my veins) who actually conduct field research in Europe while professing belief in the value of the processual paradigm in archeology. Can one really espouse the goals of explaining culture process when the basic problems of research involve grappling with complex stratigraphies, sorting out chronologies, classifying artifacts, determining the details of subsistence patterns, etc.? How can one get to nomothetic level of explanation when so many particulars must still be resolved? Is it indeed likely or even possible to conceive of doing «new archeology» with such fragmentary, still poorly dated sam-

ples of human activities as are represented for even such a relatively recent period as the Upper Paleolithic?

I will answer with a brief discussion of another paradox of the «new archeology». «True believers» have followed the approach of T. Kuhn (1962) to the history of science by declaring that a «revolution» occurred in archeology in the 1960s overturning of the previous «normative» paradigm. Having shown itself to be explanatorily bankrupt, the phylogenetic approach to normal science was believed to have collapsed, to be replaced by an entirely «new» archeology. I do subscribe to the view that the «explanations» of traditional archeology were non-explanatory (e.g., the mere assignment of phenomena to static «culture» categories resultant from supposed vitalistic trends toward progress or «evolution» in a non-Darwinian sense, diffusion, migrations, invasions or unexplained «independent inventions»), however, I do not think that science develops solely via revolutions totally overturning all the work of past researchers. Most of the discoveries of science are cumulative; most (but not all) may be valid under more than one paradigm. So it is with prehistoric archeology, even if many discoveries which would be made today were not made under the former paradigm because there was no theoretical basis or context of relevance for researchers to realize the existence of «facts» under their very own noses!

The *modus operandi* of many of us who truly want to understand the functioning of extinct adaptive systems and to explain some of the fundamental changes which occurred in the Stone Age (e.g., The Middle to Upper Paleolithic transition, the development of complex Terminal Pleistocene settlement-subsistence patterns, the origins and adoption of food production economies), is to do field research with short- and interim-term goals (the sort really achievable during our active lifetimes and which do constitute building blocks for future higher-level interpretations) and to propose preliminary models or scenarios based on re-studies of inadequate data sets from abundant, large-scale old excavations and on some detailed analyses of better-quality data from recent (but usually more limited) excavations carried out with modern methods. Such models are set forth as *working hypotheses* to be tested by further excavations and analyses. Thus they are true to the epistemological tenets of the «new archeology». Few workers in the Cantabro-Pyrenean region, however, are explicit in describing their long-range goals so it is hard to draw definite conclusions as to where they would place themselves *vis á vis* the putative revolution in archeology. My suspicion is that some researchers are approaching prehistory from an integrative, natural science, reconstructionist perspective, while others may be emulating some of the *techniques* of some Anglo-American ar-

cheology (e.g., multi-disciplinary analyses, radiocarbon dating, statistics and computer use¹ because they are «modern,» «progressive,» fashionable. Some pay lip service to the classic phylogenetic scheme, while others still fundamentally believe in it as an «explanation» of the prehistoric past. Nonetheless, the quality of the data being recovered is generally quite good - so long as the results are published fully and in timely fashion.

Is there room for anything beyond increasingly sophisticated *description* in Upper Paleolithic prehistory? How do we deal with the problem of a woefully inadequate data base in order to move into a genuinely explanatory phase or mode of research? The simple answer to the second question is to follow the suggestion of A. LEROI-GOURHAN to «multiply the number of excavations,» while continuing to search for new sites (especially open air ones), to perfect and standardize our recovery, recording and analytical methods, while paying increasing attention to microstratigraphy and site formation processes. The answer to the first question is somewhat more complicated. To attempt one, let us look at some recent research in the area.

4. THE STATE OF THE ART IN REGIONAL UPPER PALEOLITHIC RESEARCH.

The 1970s and early 1980s witnessed a large number of temporal and regional syntheses, with studies of the Mousterian (FREEMAN 1970), the Early Upper Paleolithic (BERNALDO DE QUIRÓS 1982), the Solutrean (STRAUS 1983a), the Lower/Middle Magdalenian (UTRILLA 1981), the Upper Magdalenian (MOURE 1974), the Azilian (FERNÁNDEZ-TRESGUERRES 1980), the Asturian (CLARK 1983; GONZÁLEZ MORALES 1982), the Pyrenean Upper Paleolithic in general (ARAMBOUROU 1976; BAHN 1984; CLOTTES 1976), and Paleolithic faunas (ALTUNA 1972; DELPECH 1983; FREEMAN 1973, 1981; STRAUS 1977, 1983b). A number of major site monographs were also published in this period: Cueva Morín (GONZÁLEZ ECHEGARAY and FREEMAN 1971, 1973), Tito Bustillo (MOURE 1975; MOURE and CANO 1976), Duruthy (ARAMBOUROU 1978), El Pendo (GONZÁLEZ ECHEGARAY 1980), El Rascaño (GONZÁLEZ ECHEGARAY and BARANDIARÁN 1981), Enlène (BÉGOUËN and CLOTTES 1982), Ekain (ALTUNA and MERINO 1984), Erralla (ALTUNA, BALDEÓN and MARIEZKURRENA 1985), Les Eglises (CLOTTES 1983), El Castillo (CABRERA 1984), La Riera (STRAUS and CLARK 1986), and El Juyo (FREEMAN et al. 1987). These documents provide baselines for the construction of a modern prehistory of the region; many go beyond synthesis of the known facts to engage in constructive interpretation and even reasonable speculation.

This is not the place to attempt grand synthesis (and indeed a few essays (BUTZER 1986; FREEMAN

1981; STRAUS 1985, 1986a; STRAUS et al. 1988) and several chapters in books (BAHN 1985; STRAUS and CLARK 1986) have presented first approximations thereof (*pace* GONZÁLEZ ECHEGARAY 1984). Let me mention, however, what I take to be some of the main things we seem to have learned in the past two decades about the adaptations of Terminal Pleistocene hunter-gatherers in the Pyreneo-Cantabrian region.

First, studies of large numbers of artifact assemblages (BERNALDO de QUIRÓS 1982) have shown that the Early Upper Paleolithic sequence in northern Spain was, despite some general similarities, quite different from that of the classic Périgord region. There are similarities which crosscut the traditional Perigordian-Aurignacian «cultural» categories and little evidence of directional change in the assemblages, except the very late appearance of assemblages with many backed pieces (the «Gravettian» (STRAUS and HELLER n.d.; STRAUS n.d.a.). Variability among Early Upper Paleolithic assemblages can be explained by a combination of factors, including site functions, lithic raw materials and the development of composite technologies including weapons tipped or barbed with «armatures» (backed bladelets, gravette points, etc.). Stronger evidence shows the existence of regular, non-directional interassemblage variability in the «Solutrean» period (20,500-17,000 B.P.), correlated with differences in faunal assemblages and topography (STRAUS 1983a).

The excavation of La Riera Cave confirmed that the traditional chronology of Solutrean «phases», based on the unique stratigraphic sequence of Lauge-rie-Haute in Périgord, is wrong, at least for northern Spain; time alone does not explain assemblage differences. La Riera was clearly used for a variety of purposes during this and other periods, leading to the deposition of differing suites of tools, debitage, bones and structural remains, regardless of normative «cultural» attribution. Indeed many Solutrean assemblages, if stripped of their (sometimes rare) foliate or shouldered points, look very much like either «Lower Magdalenian» assemblages or «Gravettian» ones in terms of «substrate» tool composition (STRAUS and CLARK 1986). A similarly wide range of assemblage variability has been demonstrated for the Franco-Cantabrian Magdalenian (STRAUS and CLARK 1986). Indeed the «fossil director» artifact of the Upper Magdalenian, the antler harpoon, was clearly an invention linked to intensified fishing. Harpoons appear early at some sites (e.g., TITO BUSTILLO) but never at others. Considerable chronological overlap exists between levels typologically assigned to the «Lower» and «Upper» Magdalenian at different sites. Finally, there is considerable typological and chronological overlap between the late Magdalenian and Azilian in these regions, calling into question this part of the traditional culture-stratigraphic scheme

as well (STRAUS 1987a). The «Asturian» may simply represent bulk garbage deposits from shellfishing and other activities of late «Azilian» hunters and early «Neolithic» pastoralists (STRAUS 1979). Such reinterpretations are basic to a new approach to the archeological record consonant with processual archeology.

A second major contribution of recent Upper Paleolithic research is the study of subsistence patterns. The Cantabrian evidence is largely the result of long-term, detailed work by J. ALTUNA and associates (e.g., ALTUNA 1972, 1976, 1981, 1986; ALTUNA and MARIEZKURRENA 1986, 1986; see also KLEIN et al. 1981). This evidence points to intensification in the food quest beginning in the Solutrean period, with both diversification of exploited resources and the development of specialized methods for procuring large numbers of two key ungulate prey species: red deer and ibex. Diversification entailed the exploitation of previously little- or un-used resources such as fish, shellfish, birds, ibex, chamois, boar and roe deer (although the appearance of the latter two species in the record is mostly linked to reforestation at the end of the Pleistocene). These resources were increasingly exploited (to the point of overexploitation in the case of limpets) throughout the course of the Würm Tardiglacial and early Holocene (ORTEA 1985; STRAUS 1977; STRAUS and CLARK 1986; STRAUS et al. 1980, 1981). The specialized mass hunting of *Cervus* and *Capra* is now well documented by numerous studies in Guipúzcoa, Santander and Asturias with MNI, body part, age and sex analyses of Solutrean, Magdalenian and Azilian archeofaunas, which are in sharp contrast with the much smaller assemblages of the Middle and Early Upper Paleolithic. A comparison between opportunistic foraging in the earlier period and logistical collecting strategies in the latter period can be clearly made, particularly with reference to specialized ibex hunting sites along the Pyrenees and Cantabrian Cordillera (STRAUS 1987b).

Subsistence in the late Last Glacial of Gascony was characterized for the most part by specialized, highly efficient slaughter of reindeer (as well as of ibex, horse and bovines), as shown by recent excavations at Duruthy (ARAMBOUROU 1978), Dufauré (STRAUS et al, n.d.), Enléne (BÉGOUËN and CLOTTES 1982), Les Eglises (CLOTTES 1983), and by studies of older collections (BAHN 1984 STRAUS 1983b). When major environmental changes came at the end of the Pleistocene, the effects on hunter-gatherer subsistence were far more abrupt and severe in France than in Spain, since the key resource of Aquitaine, *Rangifer*, went extinct within a few centuries and reforestation brought about an entirely different resource structure to which human groups had to relatively quickly adapt. In Cantabrian Spain, adjustments were made, but people basically continued ex-

exploiting most of the same species (plus more plant foods) in a broad-spectrum subsistence system already in place for some 10,000 years. Thus Cantabria and Gascony provide interesting points of comparison in terms of Late Upper Paleolithic adaptations (e.g., STRAUS 1983b, c, 1987d) and in terms of the nature of the Pleistocene-Holocene transition (e.g., STRAUS 1986b). On-going research along the 43rd parallel thus contributes to the comparative, analytical study of *regional* settlement-subsistence systems in Last Glacial Europe (cf. GAMBLE 1986; SOFFER 1985).

In line with that broad-scale comparative approach, I have recently proposed a scenario for mobility patterns in the Magdalenian period of northern Spain (STRAUS 1986a). In this very preliminary model I envisage relatively low residential mobility with base camps established along the coastal zone (then somewhat wider than today) and logistical locations in the foothills and Cordillera/Picos de Europa slopes. I interpret regional population densities to be relatively high by this time and I suspect the existence of fairly well defined, rather small band territories (compared with much larger annual territories on the plains and plateaux of France, Germany, Central and Eastern Europe). The abundance of cave art and the relationships between art «sanctuaries» and habitation sites in this period may in part be indicators of such territorial relationships (and of relatively closed mating systems?) (see STRAUS 1987c). Presently available data developed by ALTUNA for a large number of Magdalenian (and Solutrean) levels throughout Vasco-Cantabria fail to detect strict seasonality to the utilization of upland and lowland areas (probably since logistical parties or even residential groups had easy access to all the region's habitats within a few hours walk of any base camp) (STRAUS 1986a, 1987b). In contrast, growing evidence suggests transhumant patterns of reindeer hunting in the Magdalenian period along the Pyrenees: high, montane residence in summer, low in the cold seasons, with specialized ibex hunting parties sent into the mountains even in winter (ARAMBOUROU 1978; BAHN 1984; CLOTTES 1983; DELPECH 1983; GORDON 1986; STRAUS et al. 1988; STRAUS 1987b).

I have long felt that the Cantabrian data presented a credible case for the role of population pressure in changing human subsistence patterns (STRAUS 1977, 1981; STRAUS and CLARK 1986; cf. BINFORD 1968; COHEN 1977). This view takes on added plausibility with the growing evidence for total or at least substantial human abandonment of northern Europe during the Last Glacial Maximum ca 20,000-15,000 B.P. (see GAMBLE 1986). As argued by Joachim (1983), this may have led to substantial, cumulative increases in human population densities in the relatively rich, relatively habitable environments

of southwest Europe at the time, causing a need for significant adjustments in subsistence strategies (these being of the sort earlier described - i.e., intensification through diversification and/or specialization).

This new, tentative understanding of human adaptation in northern Spain and southern France, including the development of hypotheses to deal with differences between the two regions and to explain changes through time in each, represents a contribution to the study of the processes of Pleistocene hunter-gatherer cultural variability and change. Archeological projects, like those of Ekain, Erralla, Amalda, Dufaure, La Riera, and El Juyo were designed to answer processual questions, not just to «fill in temporal gaps or to «better» typologically define supposed prehistoric «lithic cultures». Future research will undoubtedly seek to test specific aspects of the models now being advanced for Upper Paleolithic behavior.

One aspect of prehistoric reconstruction I have not dealt with is «paleoethnology» à la *Leroi-Gourhan*. Certainly, limited attempts have been made to describe particular features and structures of «living floors» in a few Upper Paleolithic sites in these regions (e.g., ARAMBOUROU 1978; BÉGOUËN and CLOTTES 1982; CLOTTES 1983; CORCHÓN 1982; FREEMAN and GONZÁLEZ ECHEGARAY 1970; FREEMAN et al. 1987; GONZÁLEZ ECHEGARAY and BARANDIARÁN 1981; MOURE and CANO 1976; STRAUS and CLARK 1983). No one has really attempted, however, to produce detailed scenarios of «moments in time» during any given occupation in these regions. This is perhaps for the best, first because such anecdotal work is ultimately idiosyncratic and non-explanatory and, second, because we do not yet accurately control for non-cultural, depositional processes, particularly in the very complex, stratified contexts of the limestone caves of the Pyrenean and Cantabrian regions. Even our thinnest archaeological levels and lenses are palimpsests and in most cases cannot be directly interpreted as pristine residues of limited sets of activities. Much research needs to be devoted in the area to the crucial factors of site formation and disturbance. Work along these lines is well underway at Abri Dufaure where, once such processes have been controlled for analytically, we have been able to recognize distinctive activity areas associated with extensive paved surfaces in the Magdalenian sequence of this rockshelter/talus site (STRAUS et al. 1988). They give us further insight into the general role of this locus in the annual settlement-subsistence rounds of that period (see STRAUS and SPIESS 1985).

5. CONCLUSIONS

Paleoanthropology is by definition an interdisciplinary subject. The research of archeologists, pa-

leontologists, paleobotanists and geologists is all equally required to reconstruct and explain the remote past. Some of the preliminary conclusions I have attempted to make are my interpretations of the research results of many others, notably the research team at the Sociedad de Ciencias Aranzadi. My interpretations may be wrong, but I am convinced that, despite the apparent empiricism of many of our studies, we are contributing to one of the fundamental goals of the «new archeology» as envisaged by the Anglo-American paradigms of cultural ecology, cultural materialism and functionalism, namely the long-term explanation of culture change. Such work requires painstaking excavation, analysis, re-analysis, and studies which often seem very classical in nature. Indeed, stratigraphy, chronology, and typology are research *tools* —not the ultimate aims of paleoanthropology. But when the *goals* of the work are to understand the operation of past adaptive systems, then this indeed is research far different from that considered the norm only a few years ago under the traditional, phylogenetic paradigm.

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