

MUNIBE (Antropologia-Arkeologia) 57	Homenaje a Jesús Altuna	9-18	SAN SEBASTIAN	2005	ISSN 1132-2217
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The American-European dialogue in the study of the Upper Paleolithic: some reflections on international collaboration in honor of Jesus Altuna

El diálogo Americano-Europeo en el estudio del Paleolítico superior: reflexiones sobre la colaboración internacional en honor de Jesús Altuna

KEY WORDS: Upper Paleolithic, paradigms, culture-history, processualism, America, Europe.

PALABRAS CLAVE: Paleolítico superior, paradigmas, historia cultural, procesualismo, America, Europa.

MOTS CLÉS: Paléolithique supérieur, paradigmes, histoire-culturelle, processualisme, Amérique, Europe.

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ABSTRACT

American archeologists have been seriously involved in research on the European Upper Paleolithic since the work of G.G. McCURDY between the two World Wars, but especially since HALLAM MOVIOUS' excavations at La Colombière and L'Abri Pataud in the 1950s, and accelerating thereafter in recent decades. Particularly after the development of processual archeology in the U.S. (and of economic prehistory in the U.K.), intellectual challenges to the dominant culture-historical paradigm have arisen in European Upper Paleolithic studies. While there may continue to be elements of truth in the argument that there are fundamental transatlantic differences concerning the operational definition of "culture" and in prehistorians' ability to identify actual ethnic groups, there are growing (and some pre-existing) convergences over issues of middle-range theory, methodology and even problems worthy of investigation. A symposium organized by the author at the Liège UISPP Congress attempted to explore areas of convergence and divergence between American and European Upper Paleolithic researchers and to assess the contributions—positive and negative—of the former, to continue a fruitful transatlantic debate.

RESUMEN

Arqueólogos norteamericanos se han implicado seriamente en la investigación del Paleolítico superior europeo desde los trabajos de G.G. McCURDY entre las dos Guerras Mundiales, pero especialmente desde las excavaciones de HALLAM MOVIOUS en La Colombière y L'Abri Pataud en los años 1950, con una clara aceleración en las últimas décadas. Han surgido desafíos intelectuales al paradigma cultural-histórico dominante en los estudios del Paleolítico superior europeo, especialmente con la aparición de la arqueología procesual en los EE.UU. (y de la prehistoria económica en Inglaterra). Aunque todavía pueden existir elementos de la verdad en el argumento de que hay diferencias transatlánticas en cuanto a la definición operacional del concepto de "cultura" y en la pretendida capacidad de los prehistoriadores paleolíticos de identificar grupos étnicos reales, hay convergencias crecientes (y algunas pre-existentes) sobre cuestiones de la "teoría a nivel intermedio", de la metodología, e incluso de la definición de los problemas aptos para la investigación. Un simposio organizado por el autor en el Congreso de la UISPP en Lieja intentó esclarecer las áreas de convergencia y de divergencia entre los investigadores norteamericanos y europeos y evaluar las contribuciones—positivas y negativas—de los primeros, a fin de continuar un debate trasatlántico fructífero.

LABURPENA

Ipar Amerikako arkeologo batzuk G.G. McCurdy bi mundu gerren artean egin zituen lanez geroztik inplikatu dira serioski Europako Goi Paleolitikoari buruzko ikerkuntzan, baina bereziki Hallam Movius-ek La Colombière-n eta L'Abri Pataud-en 1950eko urteetan egin zituen hondeaketetatik aurrera eta azken hamarkadetan nabarmen azeleratu egin da inplikazio hori. Erronka intelektualak egin zaizkio Europako Goi Paleolitikoaren inguruko azterketetan nagusi zen paradigma kultural historikoari, batez ere AEBetan arkeologia procesuala (eta Ingalaterran historiaurre ekonomikoa) agertzean. Nahiz eta oraindik "kultura" kontzeptuaren definizio operazionalari dagokionez eta historiaurregile paleolitikoaren gaitasun aldarrikatu horretan —benetako talde etnikoak identifikatzekoan— Atlantikoan zeharreko ezberdintasunak daudela dioen argudioan egiaren elementuak egon daitezkeen, badaude handitzen diren konbergentziak (eta batzuk aurretik existitzen ziren) "maila ertaineko teoria" delakoaren, metodologiaren, baita ikergai diren arazoaren definizioaren ere, hainbat konturi buruz. Egileak UISPPren Kongresuan (Liejakoan) antolatatu zuen sinposio bat Ipar Amerikako ikertzaileen eta Europako ikertzaileen arteko konbergentzia eta dibergentzia eremuak argitzen eta lehenengoan ekarpenak —positiboak eta negatiboak— ebaluatzen saiatu zen Atlantikoan zeharreko eztabaida emankorri jarraipena emateko.

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PROLOGUE: A PERSONAL INTRODUCTION

I am an American archeologist whose maternal grandfather and great-grandfather were serious amateur prehistorians in Southwest France in the late 19th and early 20th centuries. I have systematically conducted Upper Paleolithic and Mesolithic research (mainly excavations and analyses of collections) in four countries of western Europe (Spain, France, Belgium and Portugal) for the past 33 years. However, I am based in the United States, having been trained at the University of Chicago (with a brief visiting fellowship at the University of Michigan) and having taught for 30 years now at the University of New Mexico—all hotbeds of “processual archeology”, under the influence of LEWIS BINFORD, his early students and colleagues. I have discussed my own paradigmatic biases and proclivities in how I view the Paleolithic record and the prehistoric research enterprise in several recent publications (STRAUS 1987, 1991, 1997, 2003).

In my career, I have attempted to combine a theoretical preference for attempting the scientific explanation of regularities in prehistoric human adaptive behavior with a commitment to making a significant contribution to the archeological record through excavation, interdisciplinary analyses and publication. I take particular pride in the fact that my studies have all been published (usually both in the country concerned and in the U.S.), especially important being the fact that all the major excavations I have (co-) directed have resulted in complete monographs: *Cueva de la Riera* (STRAUS & CLARK, 1986), *Abri Dufaure* (STRAUS, 1995), *Trou Magrite* (OTTE & STRAUS, 1995), *Grotte du Bois Laiterie* (OTTE & STRAUS, 1997), *Abri du Pape* (LEOTARD, STRAUS & OTTE, 1999), *Huccorgne* (STRAUS, OTTE & HAESAERTS, 2000). The current excavation of Cueva del Mirón (since 1996) is a fully collaborative, bi-national, bi-lingual, co-funded project, in which MANUEL GONZÁLEZ MORALES and I share the co-direction and divide the responsibilities of the increasingly large and complex enterprise according to our respective skills, preferences and situations (see GONZÁLEZ MORALES & STRAUS, 2000; STRAUS, GONZÁLEZ MORALES, FARRAND & HUBBARD 2001; STRAUS & GONZÁLEZ MORALES, 2003 a,b).

Having long believed that a kind of productive synergy can develop (and has in fact resulted at times) from the tension between an Americanist “anthropological” approach and a continental European “culture-historical” approach to Upper

Paleolithic prehistory, I felt that the time was possibly ripe for a frank airing of the differences of opinion *and* areas of convergence between the two “sides”, but hopefully without unnecessarily polarizing the transatlantic debate that has been going on in reality since the days of the late HALLAM MOVIUS (MOVIUS, 1953) and especially the late SALLY BINFORD (BINFORD, 1972). The idea for a symposium on the role of Americans in the study of the European Upper Paleolithic at the XIV UISPP Congress in Liège arose from my wish to participate more actively in the work of the VIII Commission on the Upper Paleolithic and because of my interest in asking both European and American scholars active in research to explore whether the supposed paradigmatic differences that presumably divided us in the recent past actually continue to have validity and, if so, to what extent and with what possibilities for compromise. Having organized and presided over a particularly dramatic episode of interparadigmatic discord in 1976 (a symposium at the Society for American Archaeology in Saint Louis on the significance of Upper Paleolithic inter-assemblage variability), I was interested to see how a quarter-century of debates and joint research projects between Americans and an ever-wider circle of Europeans (now including researchers from Central and Eastern Europe, plus Western European countries such as Portugal, Italy and Belgium, as well as from the traditional players of the past: France and Spain) might have changed the dynamics of the relationship.

THE SYMPOSIUM: AN ATTEMPT AT A ROUNDTABLE WORKSHOP

In an effort to include a wide range of views, I invited a large number of researchers from many European countries and from North America, with a goal of trying to achieve rough parity between the two groups. A complication involved how to consider Canada on the one hand, and Great Britain on the other. Certain researchers in or from French-speaking Canada can perhaps be seen as participating in the “French” school, while most active British prehistorians were trained in or heavily influenced by the “Cambridge school of economic prehistory”, which can be interpreted as similar or parallel in many respects to the ecological functionalism of American “processual archeology”. In the end, the United Kingdom was “represented” by a Briton who works in Germany (Martin Street) and by an American who teaches and does research in Britain and also does

research in Italy (Randall Donahue). Canada was represented by a Canadian who teaches in the U.S. and who does research mainly in France, but also in Russia (Randall White).

Perhaps not surprisingly, my invitations to Americans were generally accepted, although not without some amount of protest that there is no such thing as an American perspective or paradigm any more than there is a single European one. American archeologists do like to talk about paradigms, biases and the relationship between theory and method (see CLARK, 1991; CLARK & WILLERMET, 1997). This may come from aspects of the national culture of the United States, which tends towards openness, frankness, self-analysis—and abrupt lack of subtlety; it may also result from the anthropological (i.e., social science) training of most practitioners. And also somewhat not surprisingly, I had a much harder time getting acceptances from Europeans—especially French, Spanish and Italian colleagues, despite professions of interest in the topic. It seems to me possible that (in addition to their being very busy—and yet the Congress was held in a very central location in Western Europe!) the reluctance of many Europeans to “bare their souls” and publicly discuss their experiences with and opinion of the role of American colleagues face-to-face, might stem from deeply-engrained, culturally inculcated reticence and politeness. Certainly Europeans sometimes do have strongly held views of how “their” Paleolithic prehistory has been appropriated and interpreted by transatlantic guests (e.g., APELLANIZ & RUIZ, 1992/93; BARANDIARAN, 1993; GONZALEZ MORALES, 1991), but it is not easy to move the debate into the civil, but oral format of a workshop. I speculate that on the ancient, densely populated continent of Europe (as opposed to the young, brash and wide-open America), it is seen as better to reserve criticism to the less direct, less personally confrontational medium of the printed page or to comments made behind closed doors, than to make a public display. It was even a bit difficult to get people to sit around an open square arrangement of tables in the traditional university classroom in which the workshop was held. Nonetheless, some candor and optimism was expressed at the Liège meeting, for which I am gratified. Ten papers resulting from the Liège symposium have been published in a volume of British Archaeological Reports (STRAUS, 2002).

Besides those already mentioned and myself, others who participated with more or less formal presentations included NUNO BICHO, HARVEY

BRICKER, GEOFFREY CLARK, FRANK HARROLD, JANUSZ KOZLOWSKI, MARCEL OTTE, JEAN-PHILIPPE RIGAUD, JAN SIMEK, OLGA SOFFER & JIRI SVOBODA. In addition, there were several statements made by members of the audience in the fairly lengthy and animated discussion portion of the workshop. Notable interventions included those of JORDI ESTÉVEZ, MICHAEL WALKER, PAOLA VILLA, among others.

PARADIGMS: FLOGGING A DEAD HORSE?

I admit that I am a product of the early 1970s in terms of my view of archeology. The baggage that I carry is a historic confrontation between “traditional” (“normative”—today, “essentialist”) archeology versus “new” archeology, between culture-history and cultural process, between particularism and nomothetism. This was a world-view imposed by the optimistic, messianic “gospel” according to LEWIS and SALLY BINFORD, KENT FLANNERY, STUART STRUEVER & others, with whom I had direct and indirect contact, and it was believed to represent a “scientific revolution” of the sort argued by THOMAS KUHN (with whom I also had both bibliographic and personal contact through the late PAUL MARTIN). I still believe that archeology can be part of anthropology, which is both a social and a biological science, and thus concerned with general patterns of human adaptive behavior, present and past. Yet I freely acknowledge that archeology can also be part of history, but believe that for such remote periods as the Upper Paleolithic it is unrealistic from a practical standpoint to speak of cultural or ethnic entities in the same sense that we claim to be able to do in the ethnographic present or in the historic era of the last few thousand years.

“Paleoanthropology” was defined as the holistic, interdisciplinary study of early human evolution—both biological and cultural—against the backdrop of Late Cenozoic environmental changes. Its formulation was intimately associated with researchers at the Universities of Chicago (e.g., F. CLARK HOWELL—a human paleontologist— KARL W. BUTZER—a cultural geographer—and their students) and California Berkeley (J. DESMOND CLARK and, later, GLYNN LL. ISAAC—with strong connections to the “Cambridge School of economic prehistory”). The stress was on culture-ecological adaptations (i.e., heavy natural science focus), but these include significant demographic and social aspects, which become “easier” to monitor archeologically at the recent end of the story in the Upper Paleolithic, with the appearance of

anatomically “modern” humans and their “artistic” and symbolic activities. At the same time that paleoanthropology was being developed in the U.S. in the mid-late 1960s, the Binford-Flannery “processualist” movement was also exploding on the scene and the two phenomena were closely linked in terms of their main players, in terms of their optimistic view of our ability to reconstruct regularities in prehistoric human behavior & adaptations. Some of the players (e.g., FREEMAN, the BINFORDS) were involved in archeological research in both the Old World and United States; in short, there was considerable cross-fertilization between the “New Archeology” movement and American paleoanthropology, both in sharp theoretical opposition to traditional culture-history (see the papers in CLARK & HOWELL, 1966 and in BINFORD & BINFORD, 1968; as well as BUTZER 1964, 1971).

The tension between the anthropological and the historical perspective on the Stone Age past (centered respectively in the U.S. [and in the U.K. of GRAHAME CLARK] and in France) goes back to the disquiet of HALLAM MOVIUS (MOVIUS, 1953, p.188) about the equation made in traditional archeology between stone tools and cultures. American archeology in the late 1940s and 1950s was reaffirming its bonds to anthropology (e.g., TAYLOR, 1948; WILLEY & PHILLIPS, 1958) and, in the functionalist milieu of American archeology, many archeologists accepted as relevant to the study of the past, LESLIE WHITE’S (WHITE 1959, p.8) definition of culture as “the extrasomatic means of adaptation” (e.g., BINFORD, 1962). A major consequence was the famous BORDES-BINFORD debate over the significance of variability among Mousterian lithic assemblages: ethnicity versus function. It was not that American archeologists denied the possible existence of culturally identified entities in the remote past, but they argued that 1.) they were not likely to be observable by such characteristics as the relative frequencies of common artifact types or the presence of fairly banal tools that could easily be found at different times and places as a result of independent invention (see spectacular recent proof of this in the reliable 10,000 BP dating of the “Mousterian” horizon in Abric Agut, Barcelona [VAQUERO & *al.* 2002]) and 2.) ethnicity is not universally prominent, but rather is something that may *become* more manifest, especially under density-dependent conditions of resource competition. Understanding when and why ethnicity and ethnic boundaries become more important and developing plausible archeological means of monitoring “style” that

serves (deliberately or unconsciously) to mark ethnicity are seen by many American archeologists as important problems to be worked on—not as automatic, inherent givens, especially among low-density forager societies. A clear, useful discussion of the differences between Americanist and European perspectives on using artifacts as cultural markers can be found in a perceptive article by BINFORD & SABLOFF (1982).

In my opinion (and I know in the opinion of several other American practitioners of European Upper Paleolithic studies), the core difference boils down to the operational definitions of “culture” that are generally used (or assumed) by prehistoric archeologists on the respective sides of the Atlantic (or English Channel): “culture” as adaptive mechanism versus “culture” as “civilisation” in the French usage, meaning bounded ethnic entity. The Americanist approach is to be hesitant to project historical ethnic concepts back into the remote past, whereas the Europeanist (and Near Eastern) one sees little problem with assuming 1.) the existence of such bounded units and 2.) archeologists’ ability to “see” them in the archeological record by means of archeologically defined assemblages, artifact types or morphological traits. It is perhaps ironic that modern cultural anthropologists (since the seminal works of EDMUND LEACH and FREDRIK BARTH) have de-emphasized the absolute “reality” and fixity of ethnic groups—seeing ethnic boundaries and definitions are far more flexible and transitory (and situationally adaptive) than did traditional ethnographers—while many Upper Paleolithic specialists in Europe continue to regard the identification and tracing of such archeological constructs as major goals of prehistoric research. Classification of the archeological record into such higher order taxa with supposedly real ethnic meaning is apparently viewed by some as the ultimate raison d’être for the profession (e.g., BOSSELIN & DJINDJIAN [1999] with their creation of an ever more geographically expansive Badegoulian “culture” by means of folk migrations during Dryas I sensu lato, to which STRAUS & CLARK [2000] replied in characteristic “American” fashion and were misunderstood [BOSSELIN & DJINDJIAN 2000]). Of course, less traditional Europeanist approaches would nonetheless continue to emphasize the historical (or natural historical) aims of Paleolithic research, even if a strict belief in the reality of prehistoric “litho-cultures” may be waning in many quarters.

But is the whole picture one of absolute incomprehension and incompatibility? I would argue that since the 1970s, and as consequences

of a broadening of the nature of the prehistoric research enterprise beyond a narrow artifact-centered focus, as well as of methodological convergence and increased transatlantic collaboration, there is now much more of a meeting of the minds, even if the differing operational definitions of culture may remain a sticking point for some. An excellent example of inter-paradigmatic congruence focused on the exigencies of modern archeological recovery and recording methods and of the interdisciplinary approach to the analysis of the holistic paleo-anthropological record is provided by an honest joint assessment recently published by two long-time collaborative research partners, HARROLD DIBBLE & ANDRÉ DEBÉNATH (DIBBLE & DEBENATH, 1991). A *modus vivendi* can be achieved and indeed can be very productive.

Quite frankly, the ability to bridge the transatlantic gap may be facilitated by the current absence from the arena of discussion of towering, but highly opinionated—some would say, demagogic—figures à la BORDES or BINFORD. Indeed, perhaps the time has come to simply get on with the normal science of meticulously building an interdisciplinary record from sites, sediments, paleobotanical samples, faunal remains, radiometric assays, stone and organic artifacts, while politely keeping one's ideas as to WHY one is doing it to oneself? And yet there is the lingering feeling among some of us that "data" do not simply exist in a theoretical void. There have to be more-or-less explicit reasons for which we would want to know about such things as seasonality, flint sources, microdebitage concentrations, or dental hypoplasia. Or why many French (and other European) colleagues are so concerned (obsessed?) with *chaines operatoires*?

I would argue that in fact many American archeologists interested in the European Upper (and Middle) Paleolithic have had a Franco-centric (indeed, Périgord-centric) myopia that made the paradigmatic gulf seem even greater than it may have been in the wider context. Impressed by the paleo-ethnic pretensions of the "Bordeaux" school in the 1950s and 1960s, some Americans may have tended to ignore the "paleoethnology" of ANDRÉ LEROI-GOURHAN'S "Paris" school and its clear (although not complete) convergences with Americanist anthropological archeology—not to mention the significantly different (and distinctly socio-economic) foci of prehistoric archeology practiced in the former Soviet Union and its neighbors in Eastern and Central Europe. A broader view of Upper Paleolithic prehistory as conducted

throughout Europe—not only today, but also in the past—would reveal a far broader variety of underlying theoretical perspectives and, hence, methods than many American archeologists (with sometimes only limited exposure to more than just one area of Europe) have realized.

OBSERVATIONS FROM THE SYMPOSIUM

Is there *an* American "school" or "perspective"? Maybe or maybe not. Is there a European one? Not likely! This point was forcefully made by OLGA SOFFER, and is well taken. SIMEK, given his very long-term collaboration with RIGAUD now spanning several major excavations, also questioned the wisdom of drawing a line between "American" and "European" schools. But CLARK was predictably of the opposite opinion, believing that there is a fundamental difference having to do with the operative definition of "culture" as used on the two sides of "the pond" (or English Channel). (This is a point that—based on her submitted abstract—would also very strongly have been made by ANTA MONTET-WHITE, who is an extraordinary observer of these matters as a Frenchwoman who was partly trained in France, but with a Ph.D. from the University of Michigan, a long career of teaching at the University of Kansas, and direction of Upper Paleolithic excavations in France, ex-Yugoslavia and Austria.)

Have Americans made an impact—beginning with the seminal Abri Pataud Project of the late HALLAM MOVIUS, and continuing with the long-term involvements of the "Michigan" school especially in Central Europe and the Balkans, and of the "Chicago" school in Spain? Yes, and the different points of view, goals and methods of hosts and visitors have created not only significant elements of "the record", but also productive synergy in many cases. This is true even if some of the Americans' work was a bit "premature", as in the case of pre-computer attribute analysis at Pataud as pointed out by BRICKER (or, for the Lower Paleolithic, pre-taphonomy spatial analysis at Torralba y Ambrona). These points were made by several speakers (e.g., BRICKER, CLARK, HARROLD, STRAUS, & ESTÉVEZ, the latter of whom documents the importance of the arrival of American archeologists on the very "traditional", pervasively culture-historic prehistoric scene in Spain in the 1960s-1970s, in his recent book with ASSUMPCIO VILA [ESTÉVEZ & VILA, 1999]). In some particular cases (e.g., Portugal, as argued by BICHO), the major intervention of just one American

researcher (ANTHONY MARKS of Southern Methodist University), together with shorter-term projects by other North Americans (DAVID LUBELL of the University of Alberta and the present author), can help lead to a total revolution in Upper Paleolithic research in a country. In others, a dose of new fieldwork can reinvigorate a long, fertile tradition—despite the partially differing perspectives of the transatlantic partners, complementary in their interests and talents (as in the case of the South Belgium Prehistoric Project of OTTE & STRAUS, a long-term collaboration that has led to both many publications and perhaps some—less public—mutual reflection). Some countries with long, traditions of active Upper Paleolithic research have seen the development of mutually productive collaborations, with Americans bringing different kinds of expertise or emphasis, as well as new stimulation. This seems to have been the case in the Czech Republic and in Italy, as reported by SVOBODA & DONAHUE, respectively.

Has there been (perceived and hopefully unintentional) intellectual arrogance on the part of some Americans? “Yes”, as was unabashedly stated by RIGAUD. In part because of a shameful national tendency to not learn or use non-English languages (even in academic milieux!), Americans sometimes cite other Americans (or English) as the sources of ideas or discoveries that had actually been made by researchers who published their findings in languages other than English. This reliance by Americans on secondary literature in English only (or mainly) can be rightly very hurtful or even insulting to the European researchers in question. The unfortunate consequence of America’s putative global ascendancy is the growing feeling among speakers of Europe’s other great languages that they too must publish in English in order that their work not go ignored, not only in the Anglophone world, but also among the other countries of even the European Union (Thus, for example, the recent creation in Porto of the *Journal of Iberian Archaeology*, which publishes articles on Spain and Portugal only in English, even if the authors are Spaniards and Portuguese who can understand one another’s own languages! Another new English-only publication, *Eurasian Prehistory*, is a co-production of Harvard and Jagellonian Universities in the U.S. and Poland respectively.) This “imperialism” of the English language is understandably all the more painful to French prehistorians, since, until not long ago, French—

the language of BOUCHER DE PERTHES, de MORTILLET, BREUIL, LEROI-GOURHAN and the BORDES - was the universal lingua franca of our discipline. In the still relatively short span of my career, I have witnessed Spanish prehistorians shift from French to English as their preferred language of international communication. I regard this as a sad fact.

More pernicious still would be American smugness about the superiority of a “scientific”, anthropological paradigm in Paleolithic prehistory and complete denial of the possibilities for recovering historical elements from the record of the remote past. This is something that has concerned me in recent years, probably in unconscious reaction to the rise of post-processualism (with its concern of human agency and contingency in the face of the pervasive ecological determinism and materialistic functionalism of processualist archeology) on the one hand, and my long association with MARCEL OTTE on the other.

THERE IS MUCH HOPE: CLOSING OBSERVATIONS

A point of significant convergence that I see developing is interest in prehistoric exchanges and contacts by means of the study of exotic objects and widely-distributed “art” styles. There is a growing realization that, at least in the more recent phases of the Upper Paleolithic (i.e., “Magdalenian”, “Epigravettian”), the density of well-excavated sites and the independent chronological controls provided by radiocarbon (despite biases in the samples and problems among the dates, such as calibration “plateaux”) are sufficient to begin to talk about something close to actual human groups with longitudinal histories through time and across space. We can begin to monitor movements, possible boundaries, visits, “commerce” and “influences” among such bands across short, medium and even long distances and as they develop over the course of generations, especially with the altitudinal and northward (re-) expansions of human settlement in Europe. Such work, by analyzing the nature and distribution of fossils, marine shells, amber, non-local minerals and lithic raw materials, distinctive portable “art” objects or decorative elements, and even peculiar cave art styles (now datable by AMS when the images were made with charcoal), can potentially satisfy both the more traditional culture-historical goals of some prehistorians and the oft-stated, but usually frustrated aim of processual archeologists to get at aspects of social organization and their

supporting belief systems in the remote past. Someday—hopefully—physical, intersite refits (such as those discovered by Anne Scheer among three Gravettian sites in the Blaubeuren Valley of southwest Germany [SCHEER 1992]) can be found to provide even clearer links among places and people.

Such studies can even provide a practical rapprochement between processualists and post-processualists in the difficult task of reconstructing late Pleistocene social relations and human agency. While not devoid of major problems (i.e., how to distinguish direct procurement of exotic goods, images and ideas from diffusion by down-the-line exchange and contacts), such research is most promising in trying to understand how the Tardiglacial world of Western Europe organized itself demographically, socially and economically, and how it managed to recolonize vast areas of the highlands and especially the north, especially under conditions that remained precarious with frequent, abrupt climatic shifts. The subject of the recolonization of northwest Europe is providing an extraordinary example of problem-oriented convergence among researchers from different countries and theoretical backgrounds (e.g., CHARLES 1996 ; RENSINK 1993; HOUSLEY & al. 1997, OTTE & STRAUS 1997; FAGNART 1997; JOCHIM & al. 1999). All this research provides a promising basis for understanding the web of human group and individual connections that held the Magdalenian/ Epigravettian world together and allowed it to expand so successfully. I am struck by the coincidence of two current doctoral dissertations on Magdalenian inter-regional connections, one by ESTEBAN ÁLVAREZ (n.d.) and the other by REBECCA SCHWENDLER (2004). Their studies follow in the multinational footsteps of PAUL BAHN, MEG CONKEY, YVETTE TABORIN, JEHANNE FÉBLOT-AUGUSTINS, and others. These two students—both multilingual—come out of very different intellectual milieux: the very “culture-historical” Universidad de Salamanca and the very “anthropological” University of New Mexico. Yet—despite differences in their theoretical bases and principal methodologies—the convergence of research aims is astonishing and most promising for the future, a future in which the notions of Europeanist and Americanist perspectives and approaches to the Upper Paleolithic may be substantially erased and replaced by a new diversity of views that have much less to do with research traditions or countries of origin than with openness to synthetic innovation.

EPILOGUE: JESÚS ALTUNA & THE “CHICAGO SCHOOL OF PALEOANTHROPOLOGY”

I have known JESÚS ALTUNA since 1973, first as a student and then as a junior colleague. There is no question that since the publication of his seminal dissertation in *Munibe* over 30 years ago (ALTUNA, 1972), Dr. ALTUNA has been the “Dean” of late Quaternary mammalian archeofaunal analysis in Iberia. His *de facto* “Habilitation” was his also still-essential and widely cited volume on the history of animal domestication in the Basque Country (ALTUNA, 1980). Mine is not the place to discuss his career and staggering contributions not only to paleontology and zooarcheology, but also to Stone Age prehistory, to the preservation and advancement of Basque language and culture, and to the professionalization and development of the Sociedad Aranzadi. As one of the original disciples of the late Father JOSE MIGUEL DE BARANDIARAN, JESÚS ALTUNA is one of the pillars of Basque “anthropology” *in sensu lato* and a highly respected figure in the profession of zooarcheology in Europe. His excavations, especially in Ekain, Erralla, Amalda and Aitzbitarte, are key elements in the modern-quality record of Euskadi and northern Spain more generally. His studies (with J.M. APELLÁNIZ) of the Magdalenian art of Ekain and Altxerri— and his custodianship of these extraordinarily well-preserved sanctuaries and direction of the Ekain replica project—place him among the leading specialists in Paleolithic art in the world, but with a twist, since he comes at his study of cave art from a natural history perspective, with an abiding interest in how people represented the animals which shared the Ice Age environments of the Basque Country with them. The “art” is always closely contextualized in the archeology of artists, including their subsistence and their catchment areas or “territories” in a mountainous land that ALTUNA knows so intimately and loves so passionately.

Early in his remarkable professional career, ALTUNA was in contact with the “Chicago School” of paleoanthropology in northern Spain, because he did the study of the mammalian faunal remains from the excavations in Cueva Morín (Cantabria), directed by LESLIE FREEMAN & JOAQUIN GONZÁLEZ ECHEGARAY (ALTUNA, 1971, 1973). This study constituted the first large-scale, modern-quality, archeozoological analysis of a major excavation with total recovery of large mammal remains in northern Spain—part of an interdisciplinary, multinational research project, in which faunal

analysis was an integral part of the research, not just an ancillary aspect. ALTUNA brought paleontological expertise and broad anthropological interests to this project. There was no doubt mutual intellectual stimulation between him and FREEMAN, whose deep interest in and knowledge of prehistoric faunas as a key source of information on prehistoric human adaptations would soon be manifested in his well-known *American Antiquity* article (FREEMAN, 1973), so heavily based on the empirical record created by ALTUNA. FREEMAN's first student, GEOFFREY CLARK, consulted with ALTUNA on his small Asturian mammalian faunal collections at the end of the 1960s. ALTUNA has also provided assistance to RICHARD KLEIN (a former student of F.C. HOWELL and later colleague of FREEMAN) in his analysis of the new collections from El Juyo, as well as to recent doctoral students from both Chicago and Stanford. Together, ALTUNA & FREEMAN, with his students and associates, have pushed faunal analysis to the front of the agenda for Paleolithic research in northern Spain, putting it on a par with the classic studies of artifacts and "art". The "dialogue" and collaboration between the Basque natural scientist with ethnographic interests and the American social scientists with ecological interests have been most fruitful and satisfactory.

Over the years he and I have cooperated in the study of the Obermaier faunal collections from Altamira (ALTUNA & STRAUS, 1976), the long sequence of Upper Paleolithic and Mesolithic mammalian assemblages from GEOFFREY CLARK's and my excavations in La Riera (ALTUNA, 1986), the rich Magdalenian and Azilian series from my excavations in the Abri Dufaure on the northern edge of the French Basque Country (ALTUNA & MARIEZKURRENA, 1995), the interesting sample from the Vidigal Mesolithic and Neolithic sites in coastal Alentejo, Portugal (ALTUNA & al., 1994), and (so far) the post-Paleolithic faunas from our on-going excavations in El Mirón Cave on the western edge of Euskadi in Cantabria (ALTUNA & al., 2004). The collaborations have always been very fluid, efficient and productive. Combining the rigorous and classical methods of mammalian paleontology, with increasing emphasis on the determination of age, sex, size, skeletal part representation, and

butchery-caused fragmentation, ALTUNA and his associates (notably KORO MARIEZKURRENA & PEDRO CASTAÑOS, as well as the various specialists in other aspects of Quaternary paleontology at Aranzadi: birds, micromammals, molluscs) have built an impressive and very solid edifice of knowledge on the human exploitation of animals in and around the Basque Country during the Middle and Upper Paleolithic, Mesolithic, Neolithic and Metal Ages. Interestingly, ALTUNA's distance—as a (mainly ungulate) paleontologist—from traditional (i.e., "French") training in prehistoric archeology, gave him not only a strong interest in how Stone Age foragers made a living, but also, I suspect, considerable skepticism about the reality of the classic Paleolithic "cultures" and a strong goal to put chronology on an independent, "objective" basis through the systematic application of radiocarbon dating. These are aspects of his approach to paleoanthropology that are significantly convergent with the Americanist processual perspective.

The archeozoological record constructed by ALTUNA is one of the richest sources of evidence on prehistoric adaptations anywhere in Europe. It has been abundantly "mined" and interpreted by FREEMAN, KLEIN, myself and our students. And it is the fruit of the vision, labor and constancy of one man who, sure of his own ideas and abilities, has had the openness of spirit and intellectual honesty to engage American anthropologists who, though technical novices, were full of ideas about what might be learned about prehistoric people from the bones of animals they hunted. The synergy between JESÚS ALTUNA—*el maestro*—and us wide-eyed Americans, between the eminent Basque biologist and the New World anthropologists, has been a model of mutual respect and productivity in the search to understand our common prehistoric heritage. Ironically, it has been, I think, ALTUNA *the scientist*, who has helped humanize my own "scientific" anthropology through his own blend of holistic Basque paleoethnology, as inherited from Father BARANDIARAN. Rigor *and* humanity; seriousness and love of life. These too, along with scores of major faunal analyses and excavations, are the living legacy of my mentor, colleague and friend, JESÚS ALTUNA.

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ACKNOWLEDGEMENTS

I wish to thank JANUSZ KOZŁOWSKI (President of the VIII Commission of the UISPP) and MARCEL OTTE (General Secretary of the XIV UISPP Congress) for permitting me to organize the symposium on the role of American archeologists in the study of the European Upper Paleolithic, all of whose participants I most warmly thank for their contributions and candor. The views expressed here are entirely my own and I apologize in advance for any misrepresentations or misinterpretations on my part. I have been privileged to learn from and work productively and happily with great prehistorians from many European countries—including JESUS ALTUNA—and hope that I have contributed my share to the store of knowledge about the Upper Paleolithic, despite my "curious" ideas about their—and our common human—prehistory. I am very honored to have been asked to participate in this *Festschrift* for DR. ALTUNA. And I do remember the flies emerging from the larvae-laden sand boxes in which JESÚS macerated his "road-kills" and zoo cadavers in his wonderful old lab in the Museo San Telmo in Donostia, in the now-distant winter of 1973-74...and in a very different Basque Country and a very different Spain!