
The Big-Game Focus

Reinterpreting the Archaeological Record of Cantabrian Upper Paleolithic Economy¹

by Marcel Kornfeld

Big game and big-game hunting play a major role in hominid evolutionary models, cultural evolutionary models, and ethnographic descriptions. In western Europe's Late and Terminal Pleistocene sites, the remains of big game, including red deer, are common, and these remains figure prominently in economic reconstructions. However, assumptions about the dietary role of big-game animals guide critical aspects of these reconstructions, eliminating from consideration alternative economic strategies. Since archaeological data are usually ambiguous, inferences and models about the past can be substantially altered by applying different assumptions and methodologies to the same data. The case of Pleistocene Cantabria is used to show how an economy focused on resources other than big-game may be modeled.

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It is a culturally induced assumption that hunted mammals were the main source of Mesolithic food supply and meat quantitatively the most important foodstuff.

DAVID L. CLARKE, 1976

Twenty years ago Clarke (1976) argued that our understanding of the Mesolithic-to-Neolithic transition is heavily biased by our misinterpretation of Mesolithic economies. Specifically, he pointed to an overemphasis on the role of meat from large mammals in Mesolithic subsistence and argued that the data should be reinterpreted from the perspective of plant gathering as a subsistence base. Although much has changed in our views on the role of fauna in prehistoric human subsistence and in human evolution (Binford 1985, Ingold 1995, Isaac 1984, Lovejoy 1981, Mellars 1989, Shipman 1986, Stiner 1993), the Upper Paleolithic continues to be considered a bastion of carnivory, with large herbivores taking dietary precedence over other food resources (e.g., Bahn 1983; Bailey et al. 1983; Bicho 1993:771-72; Butzer 1986; Clark 1983:98; Davidson 1983; Gamble 1984:249, 1986; Hahn 1979; Mellars 1985; Price 1985:341; Saxon 1978:337; Soffer 1990:239; Spiess 1979; Straus 1992; Sturdy 1975; Velichko and Kurenkova 1990:260; White 1982; Woodman 1985:325). Recently, the presence of other resources in Paleolithic diets has been suggested (e.g., Borzilyak 1993, Jackson and Thacker 1996, Jochim 1983, Krupa 1995), but the full implications of such studies for Upper Paleolithic subsistence have not yet been recognized. Consequently, understanding of the various changes during and following the Paleolithic is heavily biased by our misinterpretation of Paleolithic economies. Such interpretations are uncritically perpetuated in the literature (as they are for the Mesolithic [Clarke 1976:450]), hindering a comprehensive understanding of Upper Paleolithic cultural processes and Upper-Paleolithic-to-Mesolithic transitions. How can we understand the development of cultural complexity (e.g., Price and Brown 1985), the elaboration of "art" (Pfeiffer 1982), and the more general organization and evolution during and following the Upper Paleolithic (White 1982, Champion et al. 1984, Mellars 1989) if the infrastructural properties of that system are misinterpreted? Because of the holistic nature of anthropology, this bias affects the interpretations of nearly all researchers, whatever their theoretical perspective (historical materialist, symbolic, ecological, and so on). It is therefore important to know what confidence can be placed in the role of large herbivores in the prehistoric economy.

Archaeological data are frequently ambiguous. Neither pottery nor chipped stone, ground stone, bone, or any other prehistoric object whispers truths into researchers' ears (Binford 1972:5). Multiple inferences about past behaviors, lifeways, and societies are always possible with the same data. Although this is partly a methodological issue—developing unambiguous signatures of past behaviors and system characteristics being the *raison d'être* of middle-range theory (Binford

1981)—it is also a matter of assumptions about what the past was like. That is, it involves the social milieu of archaeological research, which, as Trigger (1989:379) points out, affects “both the questions [archaeologists] ask and the answers they are predisposed to regard as reasonable.” Trigger goes on to suggest that the acceptance and application of analytical techniques is also affected by the present conditions of the researchers (p. 385). The present thus has a powerful influence on interpretations of the past and often prevents exploration of alternative explanations. Archaeology is the only social science in which a truly different society can be discovered, thus adding to our understanding of cultural diversity. To discover (or, perhaps better, to model) a different society, we have to avoid culturally induced assumptions that might block such discovery.

In this paper I start with a different set of assumptions about the Upper Paleolithic economy and search for evidence of an economy that differs from that commonly presented by prehistorians. First, I examine three studies in which large herbivores are portrayed as primary subsistence resources and critique the economic inferences drawn from them. From this critique information emerges suggesting alternative settlement and subsistence strategies. This information is synthesized and expanded on in the form of a settlement and subsistence model for the Cantabrian Upper Paleolithic in which the economy does not focus on large herbivores. However, because of the abundance of large herbivores in the archaeological record, a method is suggested for interpreting the overall characteristics of the economy from their remains. I conclude by examining the way in which archaeologists use evidence to create models of the past and what Trigger (1989:301) has identified as the lack of equifinality in such models.

The Upper Paleolithic Economy: A Selective Critique

The interpretation of the Upper Paleolithic economy of southern and western Europe (and probably those of other regions) confronts three problems: (1) generalization from small samples, (2) inconsistency of interpretation of data, making predetermined conclusions more likely, and (3) the unwarranted elimination of possible alternative explanations.

SAMPLE SIZE

Inferences about southern and western Europe's Upper Paleolithic economy are based largely on empirical generalizations from recovered faunal remains. However, the robustness of such inferences can be jeopardized if the samples involved are small. For example, Straus (1977) summarized assemblages from 67 archaeological levels at 23 sites in Cantabria and presents calculated or estimated MNI (minimum number of individuals) counts for the large fauna. From this he generalizes

about faunal exploitation in Cantabria (Straus 1977:66–74). If the large ungulates are prominent elements of the Upper Paleolithic Cantabrian economy as Straus and others assume, it is essential to know what proportion of the diet is represented by their recovered and analyzed remains. To answer this, I totaled the minimum number of individuals (MNI) of each species per period (Aurignacian to Azilian) as reported by Straus (1977) and estimated the usable meat weight of each species. The total Cantabrian large herbivore sample account for 135,344 kg of meat (table 1) or 5.4 kg of meat per year for the approximately 27,000 years of the Upper Paleolithic period. Clearly, the meat represented by this sample could not have supported a large population. If we assume a population density of 0.03 person per square mile (one connubium—500 people—occupying Cantabria, a reasonable estimate according to a number of researchers [Jochim 1983, Straus 1986]) and a daily requirement of 3,000 calories (1.4 kg of meat [see table 1]) per day per person, then all of the large ungulates in the present sample represent about 0.0013% of the Upper-Paleolithic-to-Mesolithic dietary needs.

That all of the discussions of Upper Paleolithic Cantabrian economies are based on observations of 0.0013% of prehistoric subsistence raises the question how this sample can best be used to inform about the past. Unless we understand the size and other characteristics of our samples in relation to past cultural processes (processes occurring at the level of local groups, connubia, and so on), it is difficult to know how they may contribute to our understanding of the past. In this case, the samples form a very small part of past subsistence, and any generalizations from them are highly suspect.

INCONSISTENCY IN INTERPRETATION

A further problem arises when different parts of the study do not necessarily and consistently support the conclusions or models presented. This problem is illustrated by the La Riera site investigations reported on by Clark and Straus (1983), Straus (1986), Straus and colleagues (1981), and others (e.g., Straus and Clark 1987). Straus (1986:337, 354–55) indicates that La Riera is a special-purpose site or a “super-site” the residential phases of which were “long-term,” semipermanent occupations. Bands are considered to have spent a substantial portion of the year at these locations and returned to them year after year for millennia (Straus 1986:337). This interpretation is crucial for his construction of a logistical (see Binford 1980) settlement and subsistence model in which large herbivores played a prominent role. However, on a number of points this interpretation is inconsistent with the data.

To show this inconsistency I make the following assumptions: (1) that large herbivores were a primary subsistence resource (this is explicitly or implicitly evident in the La Riera studies); (2) that all the meat necessary for survival of a band was consumed at the site (this is explicitly a part of the definition of a logistical settlement system); (3) that all the meat consumed at the site

TABLE 1
MNI and Meat Weight of Cantabrian Upper Paleolithic Fauna

	Aurignacian	Gravettian	Solutrean	Magdalenian		Azilian	Total	Meat Weight	
				Lower	Upper			Average	Total
<i>Cervus elaphus</i>	44	31	136	128	66	37	442	128	56,576
<i>Capreolus capreolus</i>	15	12	12	15	23	18	95	12	1,140
<i>Rangifer tarandus</i>	1	1	5	2	12	—	21	55	1,155
<i>Sus scrofa</i>	3	2	5	—	11	15	36	113	4,068
Large bovines	21	12	25	14	13	5	90	413	37,170
<i>Rupicapra rupicapra</i>	15	25	29	6	33	8	116	15	1,740
<i>Capra ibex</i>	12	23	46	11	44	8	144	53	7,632
<i>Equus caballus</i>	13	15	46	26	10	1	111	233	25,863
Total									135,344

SOURCE: Straus (1977) and, for average meat weight, Clark (1983:103) and Clark and Yi (1983).

NOTE: 135,344 kg/27,000 years of the Upper Paleolithic = 5.0 kg per year; 3,000 kcal required daily per person (FAO 1957) or 547,500,000 kcal required per year per 500-person connubium (Birdsell 1968, Wobst 1974); 1 kg of venison = 1,400 cal (Chatfield 1954, Diem 1962; lean meat considerably less, Speth and Spielmann 1983); 5.0 kg \times 1,400 cal = 7,000 kcal; 7,000 = 0.0013% of the total required per year for a connubium; therefore, 5.0 kg = 0.0013% of the subsistence for the Cantabrian population.

is represented by the bone remaining (this is probably the case with the smaller animals; for the larger ungulates detailed taphonomic analysis would be required to make this assumption, but the site investigators implicitly do so); and (4) that raw material for chipped stone tools is not excessively restricted in the vicinity of La Riera.

First, the faunal assemblage at La Riera represents 813,220 kg of meat (extrapolated from a 10% sample of the site excavated, which recovered bone accounting for 81,322 kg [see table 2]). Using the daily caloric requirements presented in table 1, this allows for 31 person-days of a band's ($n = 25$) yearly subsistence over the potential 12,350 years of occupation (Straus et al. 1981) or 42 total years of subsistence. This is inconsistent with the interpretation of the site as a long-term, semi-permanent base camp occupied for many millennia (Straus 1986:337); not enough meat is represented to have sustained such a population.

Second, the size of the chipped stone assemblage ($n = 55,000$ pieces, estimated total = 550,000) suggests that 45 pieces of chipped stone per year (550,000/12,350) were deposited there, not enough for a base camp or a hunting camp. Although the use rate of chipped stone is controlled by numerous variables, tool manufacturing, use, and /or remanufacturing would result in the generation of thousands of pieces of chipped stone at either of these site types over a short period of time. The small amount of chipped stone recovered at La Riera seems to conflict with the proposed site function and/or the duration and intensity of occupation.

Third, as a demonstration of the multiple functions of the site, levels 4–6 of La Riera are said to represent an ibex-hunting field camp (Straus 1986:355). Here exotic flint, assemblage domination by ibex, and seasonality of occupation are said to justify this special-purpose

designation (Straus 1986:354). However, none of these attributes is unique to levels 4–6 (see, e.g., Straus et al. 1981:662, 671; Straus 1986:354), and therefore the differentiation of these levels from the rest is in doubt.

In summary, the analysis and interpretation of the La Riera site are inconsistent with the model for its function, a model that is a part of a more general Cantabrian big-game hunting economy. If La Riera was used as a base camp during a substantial portion of its history, then, given the quantity of meat represented, the large ungulates must be viewed not as a staple resource for its occupants but rather as only a supplement to other

TABLE 2
MNI and Meat Weight of Fauna from La Riera

	MNI	Meat Weight	
		Average	Total
<i>Cervus elaphus</i>	383	128	49,024
<i>Capreolus capreolus</i>	47	12	564
<i>Rangifer tarandus</i>	4	55	220
<i>Sus scrofa</i>	8	113	904
Large bovines	21	413	8,673
<i>Rupicapra rupicapra</i>	35	15	525
<i>Capra pyrenaica</i>	171	53	9,063
<i>Equus caballus</i>	53	233	12,349
Total			81,322

SOURCE: Straus et al. (1981) and, for average meat weight, Clark (1983:103) and Clark and Yi (1983).

NOTE: Using figures from table 1, this amount of meat will feed a population of 25 persons for 42 years, or 31 days each year during occupation of the site (12,350 years).

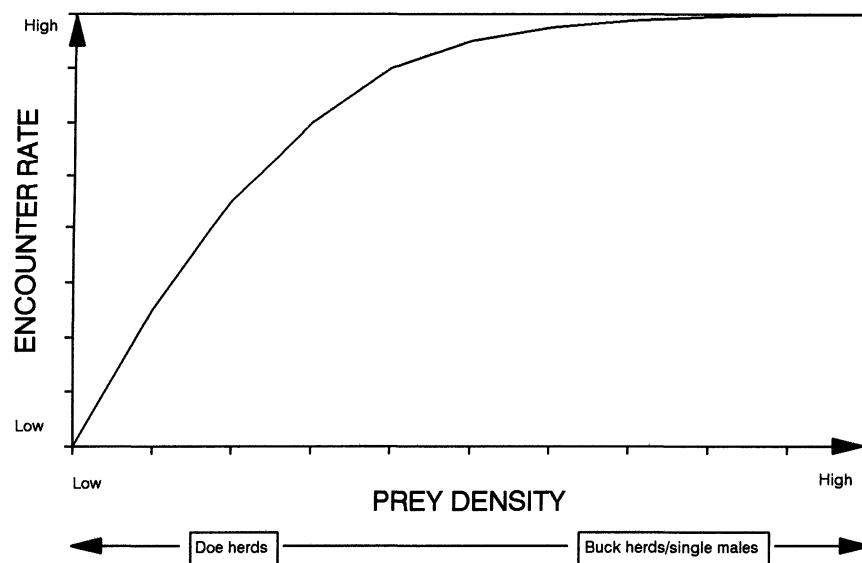


FIG. 1. Prey density and encounter rate.

resources. If, however, La Riera was only a special-purpose site (a more likely possibility for a fairly small cave), then a different economic model of Cantabria may accommodate these data more parsimoniously.

POSSIBLE ALTERNATIVE INTERPRETATIONS

Although archaeological observations can often be accounted for by reference to a number of different cultural processes, they tend to be used to demonstrate one such process or another. Jarman (1972), for example, assumed that the red deer was the focus of Upper-Paleolithic-to-Mesolithic economies but wanted to know more about the economic processes involved. He therefore analyzed sex ratios and age structures of red deer and explained the overrepresentation (from the natural population) of males and young animals by reference to prehistoric selective hunting or herd management. However, he too hastily dismissed other explanations of this faunal pattern (Jarman 1972:132; Boessneck, Jequier, and Stampfli 1963).

A number of red deer behavioral differences based on sex and age are relevant to human exploitation strategies. Clutton-Brock, Guinness, and Albon (1982:183) show that distances between animals vary, with mother-son spatial distance being greater than mother-daughter distance and nearest-neighbor distance greater in male than in female groups (p. 209). They also show that the amount of time animals spend in groups varies; young animals spend more time on the group periphery, membership in stag parties varies hourly, and aging stags are often alone (p. 191; see also Mitchell, Staines, and Welck 1977). Cumulatively, these behaviors result in more stag than doe patches, especially during rutting and antler-casting seasons (pp. 105, 213). Under certain procurement conditions, specifically encounter hunting, this population structure would be reflected in the prey

proportions. A model developed by Nicholson and Bailey (1935) predicts that encounter rate and prey density are positively correlated (fig. 1). Therefore, we can infer that while traveling over the landscape toward predictable resources, human foragers would encounter more males and young deer than females. Procurement of red deer at encounter rates may then account for the archaeologically observed population structure.

Thus Jarman's documentation of the prevalence and population structure of red deer in Europe's Terminal Pleistocene archaeological assemblages is not an unambiguous demonstration of management of these animals or their economic importance. If red deer are interpreted as a part of a broader subsistence base, the sex and age ratios can be interpreted as indicating encounter hunting, which has different implications for the hunter-gatherer economy.

SUMMARY

These critiques show that large herbivores, particularly red deer, do not clearly and unambiguously dominate the Upper Paleolithic economy. The patterns in the archaeological record are often inconsistent with the interpretations presented, and alternative explanations can sometimes account for them. What follows is an exploration of the possibility of a different economic strategy for Upper Paleolithic Cantabria.

Late Pleistocene Cantabria

Current models of logistically organized big-game hunters for Cantabria are based on the reconstruction of extremely cold (stadial) Pleistocene environments. This reconstruction emphasizes the colder stadials and generally fails to take the interstadials into account. Including

the latter allows the reconstruction of an average climate which should be more realistic for the period as a whole. Under such a climatic regime, resource characteristics are likely to be found which can be exploited opportunistically rather than logistically (Binford 1980).

Today Cantabria is defined as a coastal strip in northern Spain 400 km long by 20–70 km wide from sea level to 1,500 m (Naval Intelligence Division 1941:map). Its eastern boundary is formed by the juncture of the Pyrenees Mountains and the Atlantic coast (Butzer 1986:204–5), while the western boundary is arbitrarily assigned in terms of the absence of Paleolithic sites (Straus 1977:44). The northern boundary is formed by the Cantabrian Sea and the southern one by the Meseta del Norte (Straus 1986:333). From north to south, Cantabria can be divided into five landforms: (1) the low ridges paralleling the shore, (2) the narrow coastal plain (5–25 km wide), (3) low broken chains of foothills, (4) intermontane valleys, and (5) the Cordillera or Picos de Europa (Butzer 1986; Clark 1983:99; Straus 1986:333). Numerous south-to-north-oriented rivers form the intermontane valleys, which transect all the other landforms at 10–15-km intervals (Clark 1983:99; Straus 1986:333). This results in extremely rugged terrain with altitudinal variation creating major environmental zones.

Cantabrian physiography during the Late Pleistocene was largely the same as that of today, except that a 5–12-km-wide strip was added to the coast at the sea-level minimum, that is, during glacial peaks (Straus 1986:333). This resulted in a 10–40-km-wide coastal plain, transected about a third of the way from the coast by what are now low coastal ridges. The lower sea level increased the area of Cantabria by about 3,500 km² to a total of about 20,000 km².

Climate and environment for the whole of Late Pleistocene Cantabria (Wurm III and IV) cannot be effectively described without at least dividing the period into stadials and interstadials. Even this is an oversimplification, however, because the temperature and moisture cycles of the last 20,000 years of the Pleistocene produced warm and wet, warm and humid, warm and dry, cold and wet, cold and humid, cold and dry, and temperate and humid conditions (fig. 2; Clark and Straus 1983:139). These long cycles are further complicated by seasonal variation in temperature, precipitation, wind, and solar radiation, which has a great deal of impact on the environment including snow line and vegetation variability (Butzer 1986:211).

The regional snow line, at 1,500 m during glacials, receded during interstadials, yielding to a colonizing forest (Butzer 1986:209; Straus 1977:45). As a consequence, numerous summer snow-free passes over the Cordillera were present at 1,000–1,500 m even during the glacial stages (Bailey 1983:152), allowing access to the Meseta. The lowering of the snow line during the stadials resulted in a disproportional shrinking of the highest environmental zones. Thus, the ecozones were closer to each other during glacials and farther apart during interstadials.

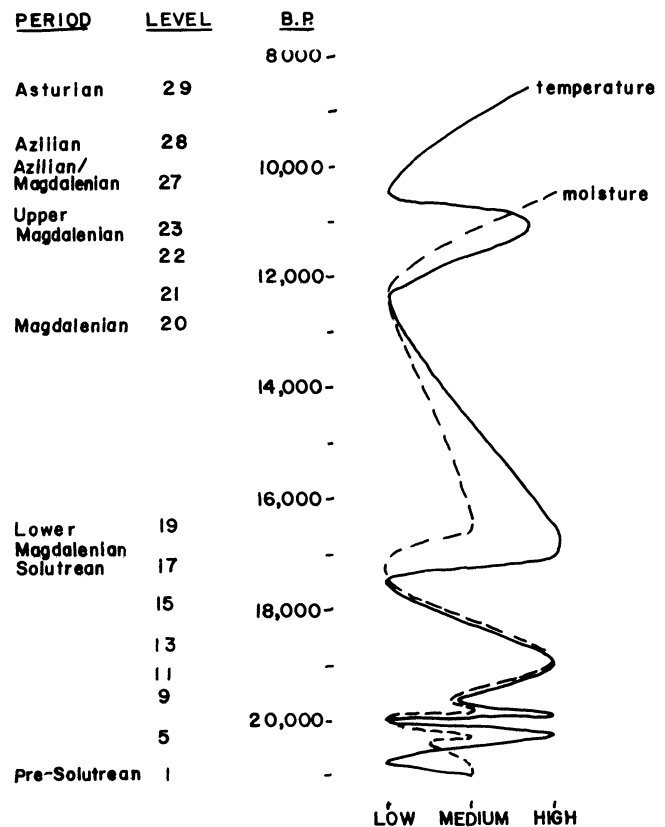


FIG. 2. Temperature and moisture variation in Late Pleistocene Cantabria as reconstructed from La Riera Cave (data from Straus 1983 and Straus et al. 1981).

Trees and other plant species expanded from their refugia in sheltered areas during interstadials (Butzer 1986:209) and then retreated during colder episodes (such refugia today can hold introduced subtropical trees such as fig, orange, and lemon [Fisher and Bowen-Jones 1958:131; Naval Intelligence Division 1941:138]). The most common recurring species in these refugia during the Pleistocene were pine, hazel, oak, and alder (Butzer 1986:211; see also Straus et al. 1981). Refugia were common in the valleys within 30 km of the coast and therefore were present throughout much of Cantabria at 10–15-km intervals (the distance between major valleys). Since the rate of spread of such tree species averages 150–200 m per year (Goudie 1977:57), it is clear that an immature forest could have covered much of interstadial Cantabria in 50 years.

Although researchers emphasize vegetative communities and resources important to animal species such as equids, bovids, cervids, and suids (Butzer 1986:212; Straus 1977:45), many of the same resources are also edible by humans or co-occur with other edible species (e.g., Belovsky 1987:51). For example, cervids such as red deer prefer to feed on greens (Clutton-Brock, Guinness, and Albon 1982:230) of various species of grasses and also eat berries, sedges, and fungi (Clark 1971:1249). Fresh green shoots are exploited by humans and form

an important part of subsistence, providing necessary vitamins, often in seasons when these are lacking (e.g., Jackson and Popper 1980:44; Wing and Brown 1979:39). Use of berries, sedges, and fungi by humans is also documented. Omnivorous suids subsist on "acorns, nuts, roots, bulbs, fruits, mushrooms, truffles, worms, snails, mice, young rabbits and other small animals, eggs and carrion" (Freeman 1973:12–13). Humans, also being omnivorous, can exploit many of these same resources, although because of various toxic substances (Stahl 1984, Wing and Brown 1979) the species used by suids are not always usable by humans.

Several other lines of evidence suggest that Pleistocene Cantabria may have had more edible plant resources than is commonly indicated in the literature. First, there is evidence that the environment was more equable on both a yearly and a daily basis, partly because of (1) solar radiation, which is greater in southern cool-period environments than in their modern northern analogues (Butzer 1971:463; Goudie 1977:42); (2) the summer high over Pleistocene Spain (Poser 1950, cited in Butzer 1971:272), which, given enough moisture, would have increased productivity substantially by further increasing the solar radiation at the crucial time of plant growth; and (3) the ameliorating effects of the ocean (Mellars 1985:274). A more equable even if colder environment would tend to have had more plant species. Second, the presence of more extensive woodlands is suggested by the number of woodland-preferring animal species (Butzer 1971:303). In particular, suids prefer a closed forest, and their diet would occur in the forest or on the forest edge. Thus the presence of suids implies a substantial quantity of forest habitat. Third, even during the glacials, when tundra was the dominant ecozone (Mellars 1985:275; Straus 1977), the edible resources may have been more abundant than has been supposed. The range of modern tundra productivity is great (Wiel-

golski et al. 1981:table 6.5); for example, some of the more productive tundras are estimated to yield over 5,000 g/m² of net primary production per year, a figure higher than for the average tropical rain forest (e.g., Whittaker and Likens 1973:358). A characteristic of tundra productivity is a high below-to-above-ground ratio (Whittaker and Likens 1973:358), and it may have been even greater in the Cantabrian nonpermafrost park tundra (Butzer 1981:135; Goudie 1977:42). Finally, the Pleistocene Cantabrian rivers, draining alpine glaciers, carried substantial quantities of water. These rivers would have provided moisture to sustain marshes and riparian vegetative communities the presence of which can be inferred from alluvial fans and bog clays (Butzer 1986:208). Such environments provide one of the richest habitats for human populations (Clarke 1976:table 2).

In summary, Late Pleistocene Cantabria may not have been as resource-poor as it is commonly reconstructed. Indeed, there are good reasons to think that the environment was substantially richer than many modern environments used as analogues.

Plant Use by High-Latitude Foragers

Although glacial Cantabria has no modern analogues (Goudie 1977:42), examination of plant use by several high-latitude populations can be instructive. Whereas the animal contribution to the diets of high-latitude hunter-gatherers is stressed, a number of these groups, including the Yahgan, Ona, Kaska, Kutchin, Ainu, and Nunamiut, use plant foods (table 3). Two comments are necessary at this juncture. First, the documented limited importance of these resources can be questioned in several of the cases. For the Yahgan, Ona, and Ainu, who live in environments similar to but wetter than Pleistocene Cantabria, most of the ethnographers were men,

TABLE 3
Plant Foods Used by Selected High-Latitude Hunter-Gatherers

Group	Food	Season	Reference
Kaska	Berries		
	Soapberries	Summer/fall	Honigmann (1964:32)
	High-bush cranberries	Summer/fall	Honigmann (1964:32)
	Low-bush cranberries	Summer/fall	Honigmann (1964:32)
	Salmon berries	Summer/fall	Honigmann (1964:32)
	Raspberries	Summer/fall	Honigmann (1964:32)
	Strawberries	Summer/fall	Honigmann (1964:32)
	Currants	Summer/fall	Honigmann (1964:32)
	Blueberries	Summer/fall	Honigmann (1964:33)
	Roots		
	Fern	Spring	Honigmann (1964:33)
	Lily bulbs		Honigmann (1964:33)
	Wild onions		Honigmann (1964:33, 44)
	Wild carrots	Summer/fall	Honigmann (1964:41)
	Fungi		
	Mushrooms		Honigmann (1964:33)
	Fruit		
	Muskeg apples		Honigmann (1964:33)

TABLE 3
(Continued)

Group	Food	Season	Reference
Ona	Fibers		
	Jackpine		Honigmann (1964:33)
	Spruce		Honigmann (1964:33)
	Birch		Honigmann (1964:41)
	Other		
	Rhubarb		Honigmann (1964:33)
	Rose petals		Honigmann (1964:33)
	Spruce tree gum		Honigmann (1964:33)
	Birch sap		Honigmann (1964:33)
	Willow buds		Honigmann (1964:41)
	Poplar sap		Honigmann (1964:44)
	Fruit		
	Berries, 3 types	Late summer/fall	Moore (1980:68)
	Holly	Late summer/fall	Moore (1980:68)
	Roots		
	Wild carrot	Spring to fall	Moore (1980:68)
	Wild celery	Spring to fall	Moore (1980:68)
	Dandelion	Spring to fall	Moore (1980:68)
	Seeds		
	Hedge mustard	Summer/fall	Moore (1980:68)
	Fungi		
Kutchin	<i>Cyttaria darwinii</i>	Spring	Gusinde (1931:291)
	<i>C. hookeri</i>	Spring/summer	Gusinde (1931:291)
	Other		
	Beech sap	Spring	Moore (1980:68)
	Berries		
	Cranberries	Fall	Nelson (1973:40)
	Blueberries	Fall	Nelson (1973:40)
	Nagoonberries	Fall	Nelson (1973:40)
	High-bush cranberries	Fall	Nelson (1973:40)
	Other		
Ainu	Inner willow bark		Nelson (1973:39)
	Wild rose	Fall/winter	Nelson (1973:40)
	Rhubarb	Spring/early summer	Nelson (1973:41)
	Leaves	Spring/summer	Watanabe (1972:471)
	Stalks	Spring/summer	Watanabe (1972:471)
	Roots	Spring/summer	Watanabe (1972:471)
	<i>Cordiocrinum</i>	Summer (stored)	Watanabe (1972:471)
	Nuts	Fall	Watanabe (1972:471)
	Fruits	Fall	Watanabe (1972:471)
	Berries	Fall	Watanabe (1972:471)
Yahgan	Fungi		
	<i>Cyttaria darwinii</i> ^a	Fall/winter/spring	Gusinde (1961:276)
	<i>C. harioti</i>	Fall/early winter	Gusinde (1961:277)
	<i>C. hookeri</i>		Gusinde (1961:277)
	10–20 other types	Some all	Gusinde (1961:278)
	Berries		
	<i>Pernettya mucronata</i>	Late summer/fall	Gusinde (1961:278, 285)
	<i>P. pumila</i>	Late summer/fall	Gusinde (1961:278)
	<i>Empetrum rubrum</i>	Late summer/fall	Gusinde (1961:278)
	<i>Berberis buxifolia</i>	Late summer/fall	Gusinde (1961:279)
Nunamiut	<i>B. ilicifolia</i>	Late summer/fall	Gusinde (1961:279)
	<i>Rubus geoides</i>	Late summer	Gusinde (1961:279)
	<i>Ribes magellanicum</i>	Late summer/fall	Gusinde (1961:279, 285)
	Roots		
	<i>Osmorrhiza chilensis</i>		Gusinde (1961:280)
	<i>Armeria chilensis</i>		Gusinde (1961:280)
	<i>Apium australe</i>		Gusinde (1961:280)
	Leaves		
	<i>Taraxacum magellanicum</i>		Gusinde (1961:280)
	Berries, 12 types		Campbell (1968:8)
Nunamiut	Roots		
	<i>Hedysarum alpinum</i>		Campbell (1968:8)

^aPlant development determines perceived season (Gusinde 1961:285).

who often underrepresent women's activities (Conkey and Spector 1984:4). Plant gathering, usually a women's activity among these groups (Moore 1980:63), may therefore be underrepresented and its importance underestimated. Second, although these plant species are not documented for Late Pleistocene Cantabrian Spain, many similar and related edible species likely existed. These groups display significant use of plants foods despite inhabiting cold, harsh environments. The total nutrient intake from plant foods probably cannot be confidently estimated, but their importance in providing certain scarce nutrients, such as vitamin C (Keene 1981:174), and their relatively low risk (Jochim 1981:110) may make plant resources attractive and necessary in many cold climates. Together with other predictable, non-big-game resources (among the Ona including tuco-tuco, crabs, and shellfish [Stuart 1977]), plants can be an essential part of subsistence, playing a major role in structuring the hunter-gatherer economy.

Settlement and Subsistence in Pleistocene Cantabria

A major problem facing hunter-gatherers is the spatial and temporal distribution of basic resources (Ames 1985:157). Residential and logistical mobility are two strategies designed to deal with these problems (Binford 1980, Kelly 1983). In environments with relatively even spatial and temporal distribution of resources, for example, a tropical rain forest, hunter-gatherers tend to be foragers with high residential mobility; that is, they move base camps from one critical resource location to another (Binford 1980). Foraging parties leave such base camps daily (1–2 days) to procure nearby resources (75–100% of gathered food is procured 3–5 km from the base camp by the !Kung [see Silberbauer 1981:265–69]). After several days or weeks, resources become depleted and the distances traveled by foraging parties become too great to be practical, requiring base-camp relocation (Lee 1979:175; Tanaka 1980:65). In environments with incongruent resource distributions (the most extreme modern case being the high-latitude tundra), in contrast, hunter-gatherers tend to be collectors with low residential mobility; they stay in base camps close to one critical resource while other critical resources are brought to them by highly mobile task groups (Binford 1980:10). In such cases, consumers and producers are separated to some degree by time (Woodburn 1980, 1982). Most hunter-gatherers fall between these two extremes (Ames 1985:157; Nassaney and Lopinott 1986:216), and Late Pleistocene Cantabrian hunter-gatherers in the environment described above should have alternated between a largely foraging strategy and a more even mixture of foraging and collecting.

For hunter-gatherers in Late Pleistocene Cantabria, the most critical variables of the physical environment would have been long-term climatic cycles, short-term productivity cycles, and the spatial distribution of resources. First, long-term cycles in variation of major cli-

matic regimes were created by Late Pleistocene fluctuations in warmth, moisture, wind, and solar radiation. Second, the short-term productivity cycles or seasonality would have created a yearly cycle of resource availability to which hunter-gatherers had to adapt by scheduling food-getting activities (Flannery 1968). Third, a patchy resource distribution would have created spatial variation in resource availability that had to be worked into the scheduling of food-getting activities. How hunter-gatherers scheduled these activities is one of the most important axes of variation among these populations.

Long-term climatic cycles would have produced expansion and contraction of ecozones. During the stadials there would have been broad expanses of steppe in the foothills and the coastal plain, with woodland refugia in the more sheltered valleys, riparian communities along the major and many stream-banks, and marshes in major well-established valleys (old oxbows, low-velocity–low-gradient areas, and so on). The process leading from stadials to interstadials would have increased and then reduced the extent of the marshes and expanded the refugia first in the foothills and eventually throughout the region. In this process the difference between the woodland and the riparian zone might have diminished. In addition, the rapidity of climatic oscillations would have resulted in nonclimax woodlands, as a climax community would probably not have had a chance to develop. Such open woodlands would have contained more humanly accessible biomass than a closed forest (Belovsky 1987:51) and would have been an exceptionally rich habitat for hunter-gatherers. Altitudinal zonation would have included the coastal zone, the woodland zone (from the coast to the foothills), and several middle- to high-altitude zones. All these zones would have been considerably wider during the interstadials as the glaciers retreated. During much of the Late Pleistocene the environment would have been somewhere between these two extremes. Thus, it seems most useful to construct a hunter-gatherer model for such an environment and then vary it to accommodate the extremes.

Short-term productivity cycles (seasons) in an average Late Pleistocene Cantabrian environment can best be summarized as presenting five seasons relevant to hunter-gatherers: winter, early spring, late spring/summer, early fall, and late fall.

Winter would have been the time of least productivity and most restricted access to resources. Travel anywhere would have been periodically impossible (for several days during snowstorms), higher elevations would have been inaccessible for 3–4 months or longer because of snow, and travel in general would have been slowed by snow accumulations (even if skis or snowshoes were a part of the technology, access was not as easy when these tools had to be used). Because of the coastal ice (Butzer 1981:176), littoral resources would not have been exploitable in the winter. Large ungulates would have been concentrated in the coastal plain and low hills. Smaller animals would have been present, al-

though some of them would have been hibernating and therefore essentially sessile resources. Plant resources would have been generally nonexistent, although rare species may have held fruits or nuts (these would usually not have been very concentrated; for example, cranberries stay on the bushes into the fall even in modern subarctic environments [Helm 1972:61], and a variety of nuts are often available well into the winter). In addition, wood bark, sap, and various fungi may have been present in wooded areas.

In early spring, large ungulates would have begun dispersing into higher areas, smaller animals would have appeared over much of the landscape, and the first green grass shoots and tubers would have become available in most areas. Coastal ice would have melted, making shellfish accessible for exploitation. Increased spring moisture (Butzer 1986:215) may also have meant high fungus productivity during this season, especially in the wooded habitats. Travel would likely have been difficult because of wet, boggy ground.

Late spring/summer would have been the period of most intense primary productivity. Greens, various tubers, and fruits would have ripened more or less in that order, earlier at lower altitudes than at higher ones. Although the highest productivity is likely to have been in the woodland/steppe ecotone, valley slopes, and marshes, all ecozones produced during this season. Large ungulates would have been most dispersed at this time, many having moved into all (higher) ecozones as grass became available while not abandoning the lower-altitude habitats altogether. There may have been some concentrations of ungulates in the best upland pastures. Early in the season these animals would have calved, increasing their numbers substantially. Smaller animals would have produced several large litters during this season as well. Coastal resources would have been available the entire season.

During the early fall vegetable foods would have remained available, with productivity waning toward the end of the season. Greens would have diminished in availability and palatability, although many edible leaves would have been present the entire season. Some tubers would have still been in season, as would numerous fruits, while nuts would have begun ripening in the middle of the season. In addition, various fungi should have ripened beginning with the September precipitation increase (Butzer 1986:215). Most of these resources would have been confined to woodlands, woodland/steppe ecotones, valley slopes, and riparian and estuarine habitats. Small animals would have been at their biomass peak at this time of the year (after all the litters had been born and the animals had matured somewhat). Their highest concentrations are likely to have been in the same areas as the plant resources. Large ungulates would have been highly dispersed throughout the area, as their preferred resources would no longer have been productive. In addition, early fall would have been rutting season, and the animals would have been behaving erratically (Clutton-Brock, Guinness, and Albon 1982:105–39; Frison 1978:250) and therefore less pre-

dictable as resources. Coastal resources would again have been available the entire season.

Late fall would have meant the beginning of the cessation of productivity. However, some roots and fruits would still have been exploitable, and various nuts would have been just ripening. Small animals would have remained at their greatest densities. In addition, their movements would have been reduced because many were storing for the winter, either as fat (thus reducing their agility and making them more catchable) or as stores (keeping the animals close to their dens to protect the stores and therefore vulnerable, along with their stores, to predators). Large herbivores, although just coming out of the rut (early in the season), would have begun to move to lower elevations as soon as the snows covered higher pastures. By the end of the season, most would have been concentrated in the coastal plains and lower hills again. In their movement there should have been preference for regular routes, some of which were almost certainly the major valleys, hence some concentration of this resource. However, it should be kept in mind that this was not the high animal population density associated with migrating caribou. In addition, the coast would have been accessible most of this season, perhaps freezing toward the end.

A Settlement-Subsistence Model

By modifying the settlement and subsistence strategies outlined by Straus (1986), Butzer (1986), and Clark (1983 [model C for the Mesolithic]), a model for Late Pleistocene Cantabria can be proposed that is based on a different set of *a priori* assumptions about the resource base from theirs and therefore a different set of constraints and opportunities for hunter-gatherer exploitation strategies (table 4). The model I am proposing is based largely on the extraction of sessile and low-mobility resources (plants, littoral resources, and small animals), with consistent use of larger herbivores only in certain seasons and intermittently throughout the year. Such scheduling requires a series of residential moves ranging in frequency from several days to perhaps a month or so, a series of group aggregations and dispersals, and the use of the extractive locations and field camps (fig. 3). Residential mobility generally brings the consumers near(er) to the resources, that is, the model postulates reliance on foraging strategies. Generally, the seasonal models are developed independently of each other, with the exception of the obvious connection of late fall big-game procurement for winter survival. However, a more interconnected seasonal scenario can also be envisioned. Although logistical and foraging strategies are ideal types, with any real system containing elements of both, the two can be used as guidelines in modeling systemic characteristics.

RESIDENTIAL STRATEGIES

A foraging strategy with high residential mobility would be most common in the early spring, when sparse re-

TABLE 4
Variables Creating Seasonal Opportunities and Setting Constraints on Cantabrian Upper Paleolithic Hunter-Gatherers

	Winter	Early Spring	Late Spring/ Summer	Early Fall	Late Fall
Productivity	Low	Medium	High	Medium	Medium/low
Travel	Restricted, snow	Restricted, mud	Unrestricted	Unrestricted	More restricted through season
Large game	Concentrated in coastal plain and foothills	Begin dispersing to all ecozones	Dispersed, may be some high-altitude concentrations	Dispersed, except for the rut, erratic behavior	Some concentration during rut and at passes
Small game	Hibernating, sessile	Begin appearing	Increase in numbers/new litters	Biomass peak	Biomass peak, slow movement
Coastal resources (shellfish)	Inaccessible	Access increases through season	Most accessible	Most accessible	Accessible until coast freezes
Plant resources					
Tubers	—	Low/mid altitude	Available everywhere	A few still available	Few
Greens	—	Low/mid altitude	Mid/high altitude	Diminish through season	—
Fruits	—	—	A few available	Available everywhere	Few
Nuts	Some still available in woodlands	—	—	Available mainly in woodlands	Abundant in woodlands
Fungi	—	Low/mid altitude	Available everywhere in moist areas	Available in moist areas	A few
Other	Wood, sap	—	—	—	—
Ecozone productivity					
Woodland/steppe	Low	Medium	High	Medium	Medium/low
High-altitude	Low	Low	High	Medium	Low
Valley slopes	Altitude-dependent	Altitude-dependent	Altitude-dependent	High	Altitude-dependent
Riparian	Altitude-dependent	Altitude-dependent	Altitude-dependent	High	Altitude-dependent
Estuarine	—	—	—	High	—

sources are quickly exhausted and relatively low herbivore density results in infrequent kills (fig. 3, *a*). This strategy would place the group close to the resources, alleviating the need for daily long-distance travel over freshly thawed ground. Band dispersal would be the greatest at this season, with individual families spreading over much of the band territory, minimizing the need to move and the distance between base camps. In this way the sparse and scattered resources would sustain a group for the longest period of time, and decisions about moving camp would be easier because only one family would be involved.

Late spring and summer would be characterized by somewhat less residential mobility, as the resources would be more abundant and patches could be utilized for several weeks or more at a time (fig. 3, *b*). Aggregations of several families could take place at this season, at least in areas where resources were most abundant. Woodland/steppe ecotones would be the preferred base-camp locations, as many of the resources in both habitats could be extracted. However, foothills and lower

mountain slopes would also provide abundant resources at this time and would serve as excellent staging areas for travel into the high mountains.

In the early fall, resources would still be abundant in the woodland/steppe ecotone, providing an excellent location for base camps (fig. 3, *c*). Aggregation size could easily increase at this time without exhausting the resources or requiring frequent residential moves.

By the late fall, the entire band could again aggregate and begin procuring and storing resources for the winter. Base camps would again be located near woodland/steppe ecotones as well as near riparian and marshy communities (fig. 3, *d*). The greater the number of accessible habitats, the more resources could be procured for storage and the less need there would be to move base camp.

Winter resources other than the stores would be sparse. Fuel would be an important variable for base-camp location, as would the location of the stores and the few remaining resources. Base camps would be moved infrequently if at all during the winter (fig. 3, *e*).

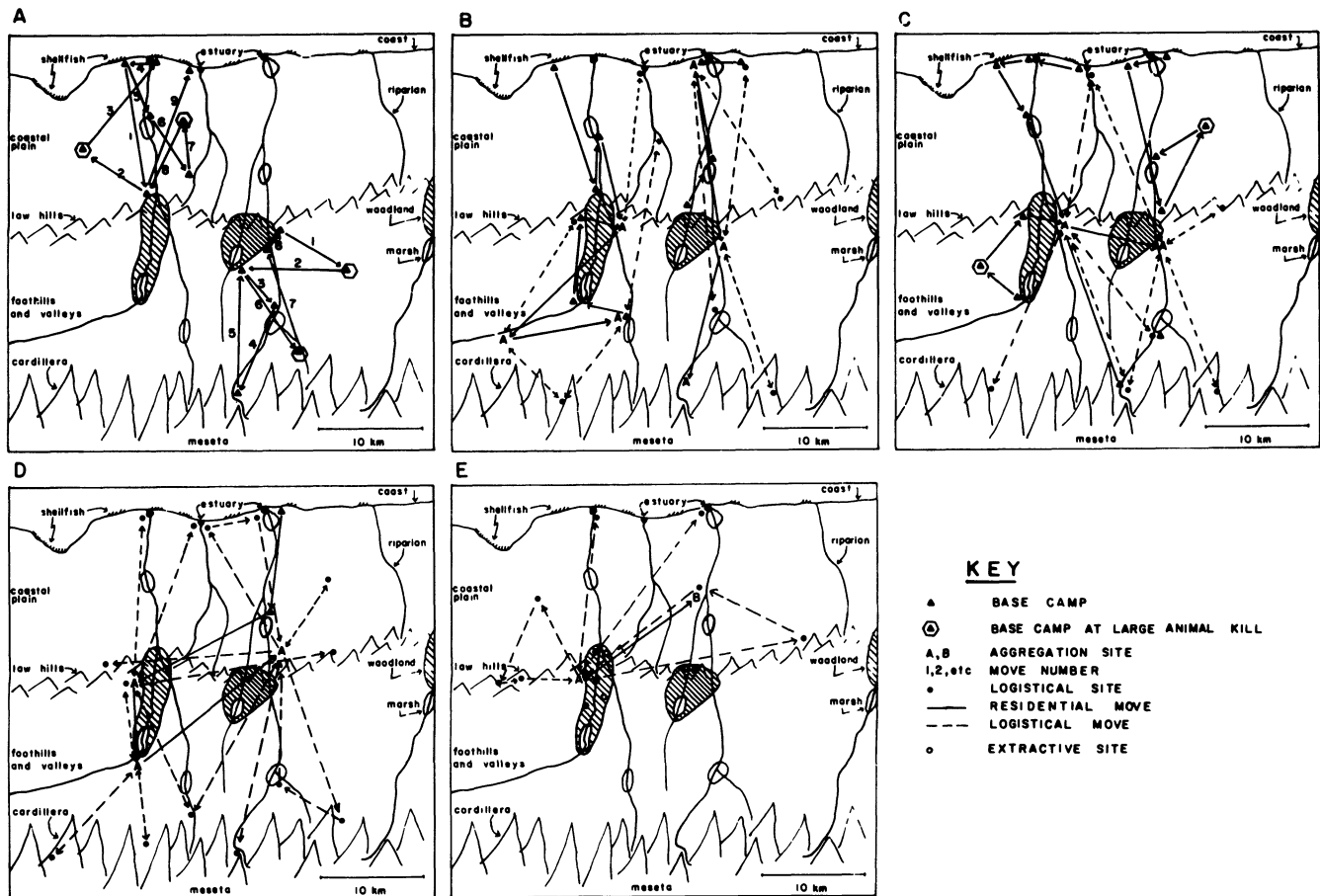


FIG. 3. Schematic drawing of movements through approximately 1,600 km² of the Cantabrian landscape and the ecozones discussed in text. A, residential movements of two hunter-gatherer family groups during spring; B, residential and logistical movements of several families during late spring and summer; C, residential and logistical movements of several family groups during early fall; D, residential and logistical movements of several family groups during late fall; E, residential and logistical movements of several family groups during winter. Logistical forays are undertaken only from camp A, while extractive forays are undertaken from both aggregation camps. Extractive sites are shown only for the winter strategies; they would be too numerous to be represented for the other seasons.

Aggregated (whole band) base camps would be critical, because in the event of the loss of stores other band members could be depended upon.

LOGISTICAL STRATEGIES

Like the residential strategies just described, logistical strategies would vary with the season. During the early spring there would be minimal reliance on logistical task groups; with maximum band dispersal it would be difficult to form such groups. The family units would be procuring resources mainly by moving between resource patches.

In the late spring and summer, logistical procurement of both sessile resources (mainly shellfish) and game would be likely. However, this strategy would be supplemental, providing minimum resources but a great deal of information. In addition, maintaining contact with

neighboring bands, including bands on the Meseta, might be especially important during this season. Such contacts would be especially important for coastal populations because the physiography restricts potential mating networks by cutting off one direction of possible contact (Wobst 1974). If the Meseta was inhabited (currently a debatable issue), this would at least have provided four directions of contact for mates instead of only two (assuming a generally hexagonal band-packing pattern). Thus, the social aspect of the logistical strategy may be more critical than its subsistence aspect, especially as the animals caught at great distances from camps are unlikely to have provided a significant dietary contribution because of transportation costs (ethnographically documented logistically organized hunter-gatherers secure large kills which they store and from which they procure concentrated processed resources and send small task-specific groups out to bring back

fresh kills only in “emergency” situations [see, e.g., Binford 1978a:268–320]].

This same pattern would intensify through the early and late fall, supporting the large base-camp aggregations. Again, gathering information and maintaining networks would be as important as procuring resources during early fall. Task groups would be most common and primarily concerned with resource extraction in late fall, procuring resources for winter stores as well as for large base-camp aggregations. Winter would see the continuation of the logistical strategy but with less intensity. In addition, winter task groups would be oriented toward procuring game or stores, since the shellfish beds would not be available because of sea ice.

INFORMATION

Information would be gathered by all group members during daily movements across the landscape and would be crucial in decisionmaking. Coastal resources would be checked after storms and winter ice, which could destroy shellfish beds. The population cycles and general density of small animals would also be monitored; their locations were probably permanent, but if the particular population was at a low point in the cycle it might not pay to move there to exploit it. The closeness of this monitoring would vary with size; smaller (more *r*-selected) animals would form smaller and more fluctuating patches than larger (more *K*-selected) ones and would require closer attention. Predicting the movements of large animals would call for observation of the weather; families or logistical parties going into high country would bring back information on the timing of fall and winter and on animal movements. Favorite passes would be watched at the expected time of the animals’ fall migrations. The maturation processes of plants would be observed for timing of exploitation, and the densities of competitor populations (for example, birds) would be considered in this connection.

Decisions of when and where to move camp would be based on the information gathered by logistical and foraging work parties continually scouting the entire territory in combination with the condition of the local resources. Keying in on any particular patch would be determined by the information gathered about resource conditions, by social relationships, by the location of other nearby bands, by consideration of length of camp occupation, and by the desire for visiting. In addition, this information would be used to decide on the size of the group to move to a particular resource patch and hence would be relevant to seasonal aggregation patterns. Thus, the difference between Clark’s (1983:104) model and mine is that the entire band would not stay together over the year but rather would aggregate and disperse seasonally.

SUMMARY

The model suggests that Late Pleistocene populations of Cantabria organized themselves flexibly to procure resources at the least possible cost and with the least

possible social tension. Just as aggregations and dispersals allowed populations to distribute themselves over the landscape to reduce procurement cost, they also allowed for new and different group construction over the seasons. In suggesting reliance on foraging strategies the model is rather different from other models of the European Upper Paleolithic. Foraging strategies place quite a different emphasis on large-herbivore exploitation as a part of the overall subsistence system. Simply put, if floral and small faunal resources provided the bulk of the subsistence and hence structured the economic system, large-herbivore exploitation can be seen as embedded in this system except for parts of late fall and winter, when it may have dominated the system. Certainly, large herbivores were exploited by task-specific groups in most seasons, but the structure and organization of such groups may have had as much to do with monitoring resources, other bands, and the condition of foraged resources as with the need for the logistically procured resources. In this sense, task specialization was embedded in the larger economic system and therefore can be characterized as more opportunistic.

Model Implications

In the proposed model, large herbivores were procured through both foraging and logistical tactics. The procurement, processing, transport, and consumption strategies favored by the two sets of exploitation patterns (table 5) are as follows:

Species composition. A logistical strategy will result in procurement of species which produce the greatest yields at the least cost. Herd animals, if they can be managed, are the best resource for this strategy. An opportunistic strategy will result in procurement of species in proportion to their encounter rates.

Procurement. Procurement in a logistical strategy is by task-specific groups that attempt to kill multiple animals and procure for stores as well as immediate consumption. With a foraging strategy, single hunters are as likely as multiple hunters. In either case, single-animal kills are common and multiple kills rare. A procurement strategy can be evaluated on the basis of kill population size. Although multiple kills are postulated for some European sites on the basis of population structure and other archaeological patterns (Altuna 1981; Klein et al. 1981; Straus 1983:210), these generally tend to be relatively small kill populations, and most sites are single-kill events (Enloe 1993a:111). In short, this evidence is consistent with a foraging strategy.

Primary processing. Primary processing in a logistical strategy includes processing for storage. Sharing occurs before storage, probably by distributing complete animal units, while stores, at least under certain conditions, are controlled by consumption units (most likely families) (Binford 1978a). Any additional material may be processed for immediate transport and/or consumption. In a foraging strategy, primary processing is for transport or on-the-spot consumption. In either case, all individuals present at the kill and/or those at the place to which

TABLE 5
Implications of Logistical and Opportunistic Exploitation Strategies for Large-Herbivore Exploitation

	Logistical	Foraging-opportunistic
Species composition/preference	Least cost per needed output; herd animals preferred	Dependent on encounter rates
Procurement	Large/band-size task group; large kills always attempted	Single or multiple hunters; usually single-animal kills
Primary processing	For storage first, then for transport of what is left	For transport or distribution for consumption
Transport	Transported to base camp from stores	Size-dependent: small animals complete, large animals choice parts or consumed without transport
Secondary processing	From storage for consumption by family-size group	For consumption by each family in base camp
Utilization/consumption	Near-complete consumption of parts transported from stores	Immediate; less complete consumption but situationally variable

the animal is transported share. The type of primary processing can be assessed in several ways: presence of stores, nature and character of the transported assemblage, and conditions of sharing. All things being equal, the presence of stores near locations of multiple kills containing body parts that store the best and provide the most nutrition after being stored should indicate logistical strategies, while the absence of such stores is a characteristic of foraging. In a logistical strategy a fresh transported assemblage should contain the most perishable part of kills (Binford 1978a:111), such as marrow bones (p. 102). Alternatively, in a foraging strategy a transported assemblage should contain body parts according to their transport costs.

Transport. Logistical strategies depend on resources transported from stores. Parts are taken from stores to minimize transportation cost. In a foraging-opportunistic strategy, transportation is from a fresh kill as opposed to stores or, frequently, in case of large kills, the consumption unit moves to the kill location and forages and hence no transportation occurs. The minimization of transport costs will result in a different assemblage pattern if the assemblage is from stores than if the assemblage is from a fresh kill. Since items left in stores are the ones that preserve the best, it will be these items that are selectively transported in a logistical strategy. In a foraging strategy, minimization of the transport cost of the entire animal can be modeled and assemblage patterns predicted.

Secondary processing. In a logistical strategy, secondary processing is of parts procured from stores and occurs at the level of the consumption unit, often a family. Secondary processing in a foraging strategy is for sharing and/or for consumption. In either case it is from a fresh kill and occurs both at the family and at the band level. Processing of transported stores will differ from processing of the same parts from fresh kills, as animal parts have different nutritive values depending on their state of storage. Assemblage characteristics can be modeled for these two situations by using the differences in nutritive value of stored versus fresh animal parts.

Utilization/consumption. Utilization, at least of those parts transported to the base camp, would tend to be more complete in a logistical strategy. Incomplete consumption of stores, however, would result from attempting to buffer against shortages which then do not occur. A foraging consumption pattern should be more variable, but what is transported should generally be completely consumed. Since there are no stores in this situation, nonconsumed parts and/or the parts most expensive to transport—that is, those providing the least nutrition for the most transportation cost—should be left at the kill site. Thus, stored and transported assemblages, which should already minimize transportation costs by the removal of bulky, low-nutrition items (e.g., bone with little utility), should be completely processed and used, suggesting a logistical strategy. A foraging strategy should be more variable in terms of intensity of consumption. It may involve intense processing, but this would be parts yielding greatest fresh nutrient value, or it may involve the use of choicest parts only in time of abundance of other resources.

These expected archaeological correlates can be observed in the extant assemblages and can yield information supporting or refuting the alternative model presented above. Several observations already mentioned support this model for the Upper Paleolithic Cantabria. In addition, several recent investigations of the Upper Paleolithic period suggest a more foraging subsistence strategy, although the main resources in these models are still big-game animals (Enloe 1983a, b; Pike-Tay and Knecht 1993).

Conclusion

The “big-game focus” implies two things: First, in a strict empiricist view of the archaeological record and of European prehistory, the abundance of large herbivore remains at archaeological sites is interpreted as a straightforward representation of past economies. Second, because of the availability of big-game remains in

the fossil record our interpretation must be based on them, but the economic implications we draw from this record are anything but straightforward. The first position is an example of the tyranny of the archaeological record and is untenable. As Myers (1989:91) has put it, "In attempting to address questions of subsistence and settlement beyond individual sites, we must escape from the tyranny of the primacy of faunal data." Although he seeks to use stone tools to get at such questions, the more inclusive implication of his statement is the potential ambiguity of any single line of evidence in isolation from others.

Interpretations and models of the past must rest on the archaeological record, but this record is never unambiguous. Among the many variables that intervene between the archaeological record and interpretations are assumptions about both the past and present, and many such assumptions are a product of the cultural milieu of scientific research. When the overemphasis on big game is recognized as a culturally induced assumption, a different interpretation of the archaeological record for Upper Paleolithic Cantabria can be considered.

This reinterpretation of Cantabrian prehistoric economy questions a number of models proposed to explain the development of cultural complexity (see, e.g., Mellars 1985, Price and Brown 1985). Whatever their theoretical orientation, all explanations have to deal with different infrastructural properties of the past cultural system. In addition, the development of complexity itself may be questioned. For example, what is the significance of the broad-spectrum exploitation in the Mesolithic if such a strategy already existed in the Upper Paleolithic? Does the evidence cited unambiguously indicate complexity, or could it indicate a change in subsistence unrelated to complexity or even explicitly designed to reduce complexity? For example, could the increase in the number of sites (e.g., Clark and Straus 1983:table 12.4) be seen as evidence of the maintenance of mobility and of egalitarian relations (Leacock and Lee 1982) rather than as niche packing and competition for resources leading to complexity? Could cave art play a role in such maintenance of equality in this system rather than in ushering in some form of hierarchy? That European cultural systems eventually became complex is not in question, but when, how, and why this occurred is still open to debate. European prehistory has come a long way from mammoth and cave-bear hunters, but much still needs to be done to place big-game hunters in a balanced perspective.

Comments

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In presenting his reinterpretation of "the archaeological record of Cantabrian Upper Paleolithic economy," Kornfeld attempts to prove that plant foods and other non-

big-games resources were much more important than the large-herbivore prey species which are conventionally considered to be staples. It is a brave attempt but one which is not altogether successful.

At its simplest level, the argument assumes that our present archaeological record is complete—that we will find no more sites in the future and even, it could be argued, that there are no more sites to be found. Kornfeld lists four assumptions in his discussion of La Riera, at least two of which are highly questionable. The second, that all the meat necessary for survival of a band was consumed at the site, is not realistic unless he has a different definition of "all." Some bone may never have reached the site, as some meat (necessary for survival) was in all probability consumed away from the site (at the kill?). The third, related assumption, that all the meat consumed at the site is represented by the bone remaining, will simply not stand up to scrutiny—for either large or small species. Kornfeld admits the need for detailed taphonomic analysis of the large herbivores to substantiate this assumption, but one is left wondering why he has either not done this or else not chosen a site where it could have or had been done. The title of the article suggests that the model proposed is envisaged for the region as a whole rather than just one perhaps atypical site. The probability of the assemblage's being complete is very remote. The passage of time alone will have had some effect on faunal assemblage structure. To assume that because 10% of the area of a site has been excavated the sample, lithic or faunal, comprises 10% of the total assemblage and can simply be multiplied by ten to obtain the total assemblage size is folly. Archaeological material is rarely if ever evenly distributed across a site, the areal extent or size of which probably remains unknown—at least until fully excavated at some point in the future. Furthermore, a potential 813,220 kg of meat may indeed yield only 42 years of subsistence, but the site of La Riera is not one for which we can assume continuous occupation for a period of 12,350 years.

A few good points are raised in this paper, especially in the coverage of some small-scale things, very important to the hunter-gatherer, which we have to bear in mind in studies of Pleistocene (especially cold-stage) hunting/foraging systems—for example, the role of winter ice in the possible destruction of shellfish beds. I agree in particular with the concept of late-fall aggregation camps with base camps at or near the wood/steppe ecotones (mixed transitional zones) or riparian and marshy communities. It is a picture seen clearly at about the same time in south-western France (Boyle 1996). The role of storage remains an unknown almost everywhere in the Palaeolithic of Europe.

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While it is nice to have one's work acknowledged in print, a portrait from life is always preferable to a carica-

ture, which is essentially what the Kornfeld essay offers the CA readership. For scholarship to be credible, there must be more-than-nominal acknowledgment of and some considerable familiarity with primary source material and with the conceptual frameworks that guided the research. Lest CA readers unfamiliar with the Cantabrian Late Upper Paleolithic and Mesolithic be inclined to take the Kornfeld essay at face value, I feel obligated to offer the following observations.

1. Kornfeld accuses Straus and me of a simplistic view of the importance of big game in Cantabrian Late Upper Paleolithic and Mesolithic adaptations and of a failure to consider economic strategies other than big-game hunting. However, even a cursory examination of the very sources he cites (e.g., Clark and Yi 1983, Clark and Straus 1983, Straus and Clark 1986) shows that we are most acutely aware of other resources (shellfish, plants, birds, small mammals) and have gone to considerable lengths to try to assess their actual and potential contributions to Late Upper Paleolithic and Mesolithic diets. Perhaps the single most important conclusion to emerge from the La Riera research was that the human food niche did, in fact, become broader over time, precisely because of the addition to the diet of more small, high-cost, low-yield species—the *very resources Kornfeld claims we ignore* (see esp. Straus 1977, Clark and Straus 1983, Clark 1987). I am utterly at a loss to explain how he could have overlooked all this fairly detailed argument.

2. The essay also ignores research by several generations of Spanish prehistorians and natural scientists who, far more than we, are responsible for what we know about long-term changes in the human food niche in Cantabria. Particularly astonishing is the omission of the major, seminal, monograph-length publications of Jesús Altuna (1972, 1980), Spain's preeminent vertebrate paleontologist, and the many and influential publications of Benito Madariaga and Jesús Ortea on northern Spanish marine malacology. These books and papers provide the foundation upon which all subsequent research must build. To omit them from an essay that supposedly addresses misinterpretations of pattern in Cantabrian archaeofaunas contributes to the impression that Cantabrian research is dominated by Anglophone workers, which is not now and never has been the case.

3. Kornfeld also ignores pertinent observations on the economic use of plants in our research (e.g., Straus 1979a, Clark 1987), in that of other American workers (e.g., Crowe 1985, and —generally—the published work of the Small Fraction Laboratory, Institute for Prehistoric Investigations, at the Magdalenian cave site of El Juyo, in Santander), and in that of Spanish scholars (e.g., Santos 1982, 1984, 1989). It is clear and definite that, even under pleniglacial conditions, Cantabria was never a “photosynthetic desert”—the *only* kind of environment in which human plant consumption is minimal and/or derived from secondary sources (e.g., the guts of marine mammals) (Clarke 1976). All workers familiar with Cantabrian data acknowledge the importance of plants in the diet, even though primary evidence for them is hard to come by prior to the Iron Age and even

though we are not yet in a position to quantify the roles they played in Late Upper Paleolithic and Mesolithic economies.

4. Based on the work of French and Spanish natural science colleagues, our paleoclimatic reconstructions represented the best evidence available at the time we were working at La Riera and have been revised periodically up to the present (e.g., Straus 1991a, 1992). During glacial maxima, Cantabria had little in the way of extensive tree cover, but that does not preclude the existence of refugia in the river valleys and in countless other sheltered localities throughout the northern Spanish coastal strip. Cantabria is very rugged topographically and is characterized, even today, by a mosaic of distinctive microenvironments the distributions of which are determined by altitude and by strictly local moisture, temperature, and soil regimes. We and others (notably Butzer 1981, 1986) have said all this repeatedly in print, but Kornfeld makes no use of this material.

5. Neither Straus nor I nor the Spanish codirector of the La Riera Paleoeological Project, Manuel R. González Morales, has ever stated, suggested, or implied—anywhere—that the economic faunas from Late Upper Paleolithic excavations in northern Spain were to be taken in any sense as a complete sample of what might have been available to Late Upper Paleolithic foragers at any particular place or time interval. To claim, as Kornfeld does early in the essay, that we think that La Riera was a semipermanently occupied “supersite,” despite the fact that the fauna, lithics, and contextual information do not support this view, is a position we have never articulated in any of the 30+ publications on the research.

Finally, to be labeled a “strict empiricist” is ironic, since Straus, González Morales, and I have all written papers that address epistemological issues in precisely the type of research under discussion here (e.g., Straus 1987a, 1991b; González Morales 1991; Clark 1990, 1991a, 1994; in fact, I edited a book on this topic [Clark 1991b]). However, even strict empiricists (people who think that meaning comes from nature and that “the facts speak for themselves”) have some familiarity with primary data, which is perhaps the most glaring deficiency of the Kornfeld essay.

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Kornfeld has presented an interesting alternative model for interpretation of subsistence in the Upper Paleolithic of Cantabrian Spain. This model addresses some of the problems fundamental to archaeological inference, particularly for inferences based on incomplete preservation of material remains from subsistence activities. He cites Meyers's (1989) exhortation to escape the tyranny of faunal evidence in determining subsistence and argues that our understanding of changes during and following the Upper Paleolithic is based on a misunderstanding of Paleolithic economies.

The “incorrect” model that is criticized refers to the Upper Paleolithic as a logistical collecting economy focusing on large herbivores as the subsistence base. For Upper Paleolithic Cantabria, Kornfeld uses Straus’s (1977) estimations of MNI of large game from 23 sites. From these he calculates the total meat weight these individuals would have made available as food for Upper Paleolithic hunter-gatherers. The conclusion that this sample would provide only a minuscule fraction of the amount of food necessary for a breeding population over a period of 27,000 years is interpreted to mean that an inadequate sample cannot provide a sound basis for interpretation of past subsistence.

Where, I must ask, do we ever have large samples of the total economic or material remains of any past society? The archaeological record is naturally very fragmentary and incomplete. That does not mean, however, that we can never have any confidence in our observations. Following Kornfeld’s argument to its logical end, what evidence do we have of the use of other resources? Do they represent an even more minuscule sample of necessary food? I ask these questions somewhat facetiously. Of course, large fauna is much more likely to be differentially preserved in many archaeological situations. But why must all evidence (bones) of all consumption (meat) be preserved at all sites?

The criticism of Straus’s (1986) interpretation of La Riera as a residential site in a logistical system is, if not unwarranted, inconsistent with Kornfeld’s own arguments. Again, calculations of meat weights compared with duration of occupation are problematic. The explicit definition of a logistical settlement system does not require that all meat necessary for survival be eaten at one site. Even if La Riera was a special-purpose site, that is consistent with logistical rather than foraging settlement.

Kornfeld proposes a more diversified model, suggesting that a greater mix of foraging activities, including substantially more plants and small animals in the diet, more correctly characterizes the economy of the Cantabrian Upper Paleolithic. He proposes a settlement-system model for exploiting those diverse resources. What is missing is a similar quantitative estimation of total quantities of food available from plant resources. We know that Pleistocene climatic conditions did not simply consist of shifting existing zones north or south or up and down in altitude. There was a reorganization of meteorological phenomena, and vegetative communities which were the base of the food chain neither were located in the same areas as today nor had the same constituents. Guthrie (1984) has suggested that the differences may be conceived of as “plaids” of diverse resource patches rather than “stripes” of current altitudinal or latitudinal zones. It is difficult to model the structure of a diverse resource base without further evidence of the nature of the patchiness or dispersion of plant and animal resources. A basic question remains about the ultimate vegetative productivity of this region under Pleistocene conditions.

Admittedly, midlatitude Pleistocene environments

may have received more annual solar radiation than do high-latitude modern tundras, but solar radiation during the growing season is disproportionately greater in those modern tundras. As a result, annual vegetative productivity may well be just as great in high latitudes as it was in Pleistocene Cantabria (note Kornfeld’s own reference to modern tundra productivity up to 5,000 g/m²). One must ask, however, what proportion of that plant productivity is edible by humans. Kornfeld provides a substantial list (table 3) of plant food documented for high-latitude hunter-gatherers but is reluctant to estimate total nutritional contribution to human diet; he asserts that plants must have played a major role in structuring Upper Paleolithic hunter-gatherer economy.

The presence of more plant and animal species, consistent with Guthrie’s “plaids” of disharmonious floras and faunas, does not necessarily equal greater productivity. Descriptions of species richness are qualitative, referring to diversity, not quantitative, referring to greater productivity or biomass. The quantity of potential sessile food resources and the structure of their availability are inadequately developed for unquestioningly accepting the alternative model for Upper Paleolithic subsistence. In the case of the Mesolithic, evidence is present for greater diversity of plant and animal resources. That evidence is present not just in the food remains themselves but also in the technology for extracting and processing them. Such evidence is not apparent in the Upper Paleolithic.

As Kornfeld states, multiple inferences are always possible with the same data, but how do we choose? For a scientist, the choice cannot be based on a political position or desire; we must choose on the basis of methodological considerations. What necessary linkages have been established between the data observed and the inferences drawn? Binford (1977, 1978a, 1981) has argued repeatedly for the development of methodology to provide the linkages for inferences about the past. Despite Trigger’s (1989) suggestion that present conditions affect the questions asked of the archaeological record, the methodological middle range of how we interpret the data we have is nonetheless the arbiter of whether we can accept the answers proposed for those questions. The present has a powerful influence on the interpretation of the past, but it does not strictly determine it. As for the prevention of exploration of alternative explanations, those alternatives must be methodologically soundly based.

We need better models and, more important, better methodologies for interpreting and understanding the settlement, subsistence, and society of the Upper Paleolithic. Faunal studies offer some very direct avenues to approaching these goals, given methodological development. We must not accept the mere presence of big game or single-species dominance to interpret the Upper Paleolithic as logistical collecting. The degree of mix of logistical and foraging characteristics varied greatly across time and space for the Upper Paleolithic. Exchanging one simplified model for another does not necessarily enhance our understanding of evolutionary process.

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Kornfeld's proposal of much greater flexibility in procurement strategies in the Upper Palaeolithic deserves particular attention. Greater complexity of dietary resources—not only big game but also fish, plants, and other things—seems certain, especially in southern Europe. Increasingly complex methods of analysis of cultural stratigraphy using plant macroremains, soil micro-morphology, and phytoliths and more and more sophisticated analyses of use-wear and organic residues on tools confirm the importance of hitherto underestimated dietary resources and the complexity of the prehistoric diet. In view of this I would have preferred that Kornfeld, who proposes new models of exploitation of the environment based on a very general reconstruction of the natural environment and the rhythm of the annual cycle of seasonal changes, had grounded his conceptions to a greater extent in analyses of the archaeological record and its natural context. If he had done so, instead of a logical construction of what might have been he could have produced an empirical model based on exploitation of the environment and a congruent form of social organization.

I cannot concur, however, with Kornfeld's arguments against subsistence strategies focusing almost exclusively on large herbivores. He rather arbitrarily calculates a biomass corresponding to the minimal number of animals in faunal samples from Cantabria, divides the result by the number of years the Upper Palaeolithic lasted in that territory, and finally divides the result by the tentatively assumed Cantabrian population. From this he concludes that the calories yielded by the calculated meat quota could not have sustained a population of this size. This seems a rather naive distortion of reality; the number of known sites is but a small fraction of the real settlement systems in the Palaeolithic, and even in "permanently occupied" sites such as caves occupation was not uninterrupted. Indeed, investigations into cave sequences indicate that the relation between mineral and anthropogenic sedimentation rates was highly variable. Often layers with uniformly distributed, dense archaeological finds are "telescoped" habitation floors separated by hiatuses of thousands of years. The concept of "long-term semipermanent occupation" cannot be taken literally as referring to the existence of a place where people returned every year for many millennia. Unfortunately, the limited precision of radiocarbon dating methods and the inexactitude of distinguishing particular habitation floors make it difficult to identify occupational hiatuses, especially in cave sites. Similarly, attempts at estimating constant sedimentation rates in clay-debris cave sediments are bound to fail.

Another issue is the problem of reconstructing long-term climatic cycles, especially in the area of the Iberian peninsula. One need only compare the work of Hoyos Gómez (1994) on interstadial fluctuations (Laugerie,

Lascaux) in Cantabria with that of Ramil Rego (1994) on the interpretation of palynological sequences for the Iberian Solutrean to see that the disparity between the sedimentological and the palynological data can hardly be explained simply in terms of differences in the climatic record between karstic environments and other ecozones. Indeed, the palynological data lead to an exactly opposite conclusion, namely, that, during the last glacial maximum (25,000–15,000 years B.P.) there was no clear-cut alternation of stadial and interstadial conditions. Paleozoological data are not sufficiently diagnostic to be used in the reconstruction of long-term cycles.

Finally, both the increasing stability of settlement systems and the predominance of "residential mobility," resulting from complementary exploitation of various landscape zones and different types of dietary resources, in the late-glacial environments of southern Europe have already received attention in the literature.

Kornfeld's contribution is a useful reminder of the complexity and flexibility of subsistence and settlement strategies. One cannot but regret that it is not based on deeper and more intensive analyses of archaeological data in their natural settings.

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Kornfeld's contribution reviews some important topics. The Cantabrian Upper Palaeolithic archaeological assemblages are very interesting because of the great quantity of information available on the environment and on human behaviour. They have also been the subject of study of various scholars, giving rise to broad archaeological debate (see Bailey 1983). These studies have produced an approach to the adaptive process that provides a framework (although, as the author points out, a highly empirical one) for the interpretation of the material "evidence." From this perspective, the idea of hunting's being the key element for the reconstruction of human economy during the last glacial period is unsatisfactory. That "archaeological data are frequently ambiguous" must be considered axiomatic, and therefore they should be examined in terms of various theoretical and methodological approaches. The archaeological evidence as a whole is subject to processes that transform it into something static. Our main interest is in the dynamics—the retrieval of the human behaviour that caused the formation of the archaeological deposits. The interpretations obtained from the "archaeological data" are in fact categories that are open to discussion. They may be artifacts of the analysis rather than reflecting the presumed "objective" reality beyond the data. For example, how can interpretations with regard to human subsistence be reliable if the role of resources considered "secondary" is not taken into consideration? And how do faunal patterns reveal adaptive strategies? Both questions are crucial.

Regarding the first question, we must recognise our lack of knowledge related to the role of gathering among the daily activities of Upper Pleistocene communities. Differential preservation greatly limits our capacity to understand the role that plants may have played in human diets. New techniques may be of help. For example, the retrieval of charcoal, seeds, and other vegetal remains, with particular attention to their contribution to the understanding of human selectivity among these resources, should be standard practice in Pleistocene "hunter-gatherer" sites. Studies of use-wear and organic elements (phytoliths and starch) on tools and isotopic and dental-wear-pattern analysis of human remains may also contribute to the assessment of the role of plant resources in the human diet. It is true that the information that can be obtained by these methods is limited, but it can be very suggestive.

As Kornfeld stresses, mobility patterns must be related to subsistence activities. To limit ourselves to the available material evidence—faunal remains—leaves the prospect ambiguous. Mainly, this is the result of the examination of archaeological evidence from a non-taphonomic perspective (Villa and Courtin 1983). The majority of archaeological deposits are palimpsests, and therefore many problems related to site formation remain unsolved, especially those concerned with the rhythm of introduction of lithic artifacts and faunal remains into the fossil assemblage.

It seems to me extremely unsatisfactory to assume that the animals or lithic remains found in a given chrono-cultural level are the result of one or a few temporal events. This assumption needs to be supported by techniques that allow its evaluation. Stone tool refitting and its application to faunal remains (Enloe 1991) can help solve this problem, forcing us to think about the variable of time. Unfortunately, such techniques have not been employed at the Cantabrian sites, and therefore the question of how the deposits of this period were created remains unanswered. This question, by no means of secondary importance, strongly affects the way we understand Upper Palaeolithic adaptive strategies. The main criticism of the derived picture from the "palaeoeconomic" perspective (see references in Bailey 1983, Sheridan and Bailey 1981) is largely the result of superficial attention to site-formation-related implications, especially the formation of the archaeological assemblages. If this argument is rejected, we may end up with pristine interpretations supported by "logical" arguments that do not necessarily correspond to the intention of past human groups' subsistence behaviour.

This procedure is apparent in the scenario for the analysis of the Cantabrian Upper Palaeolithic proposed here. It has been considered that these groups developed a logistical strategy, highly planned, of which big-game hunting was the key element. Straus's (1986, 1992) interpretation seems to me to be based on the existence of such a strategy (as proposed by Binford 1979, 1982a). Although I believe that this interpretation is not fully justified, examining it in the required depth would mean analysing a large number of variables. It is not too unrea-

sonable to think of foraging strategies based on small groups that cyclically provided a reduced number of individuals for self-sufficiency as an explanation for a significant amount of the Cantabrian archaeological evidence. In any case, the scenario based on large-scale hunting conducted during a limited number of events by hunter groups that intercepted a large quantity of animals for storage purposes (an idea inferred from Straus) is not supported by the archaeological data. Other mechanisms leading to the formation of these archaeological assemblages should be examined. Within this reevaluation, a deeper knowledge of the depositional context—long overlooked by archaeological practitioners—will be necessary. I would encourage Kornfeld to go on to consider other possibilities that might allow us to move beyond the pristine interpretation resulting from the mechanical application of the "collector model" (Binford 1982a). Archaeological assemblages must be examined from the point of view of formation processes as the results of complex interactions. This is the only way to develop new interpretations as we abandon the traditional practice of fitting the archaeological "evidence" to our expectations in favor of other explanatory procedures. Although faunal remains are important for the understanding of human behaviour, they are insufficient and ambiguous when we are dealing with the interpretation of archaeological assemblages (Gifford 1991).

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Discussion and debate are the lifeblood of any scientific discipline—especially anthropology and most particularly paleoanthropology—but they must be informed by knowledge of the materials in question. Speculation is essential to the "advance" of our understanding of prehistoric human adaptations; I would be the last to deny its value, having (cautiously) done a fair amount of it myself over the years. Yet when blatant speculation is published in the pages of CA, it often ends up being cited as if it were fact. My fear in the case of the Kornfeld speculations is that this piece of tertiary musing will be taken as somehow representative of Upper Palaeolithic "truth" for a region with which the author apparently has scant (if any) firsthand knowledge and whose incredibly rich record (produced over 120 years of archaeological research by Spaniards and the occasional foreigner) is given very short shrift in this piece. Kornfeld repeatedly states that the archaeological record is incomplete and ambiguous, yet he contributes nothing to its completeness or clarity. As I was sent this manuscript for comment with a very short deadline and at a very busy time of the year and had no prior knowledge thereof, my observations will be brief and somewhat disjointed. I will refrain therefore from commenting on the numerous small orthographic and factual errors or on the fact that figure 3 seems to be adapted without ascription from my

1986 article in the *Journal of Anthropological Archaeology*. While certainly flattering, I suppose, it is regrettable that so much of this piece is apparently based on my work and that of G. A. Clark, with virtually no reference to the much more significant research on the topic by numerous Spanish and French colleagues (e.g., Jesús Altuna [e.g., 1972, 1986, 1990, 1995], Federico Bernaldo de Quirós [1980, 1982], Joaquín González Echegaray [e.g., 1984; González Echegaray and Barandiarán 1981], Manuel González Morales [1995], Cesar González Sainz [1989, 1992, 1995] Manuel Hoyos [1995], Henri Laville [1986], Arlette Leroi-Gourhan [1971, 1986], Moure [1992, 1995], and others [see Straus 1992 for further references up to ca. 1991]). Also staggering is the lack of mention of Leslie Freeman's longtime insistence on the importance (environmental conditions permitting) of plant foods—as he and his collaborators have been demonstrating in fact during their long-term excavations with González Echegaray of El Juyo Cave (Freeman et al. 1988, Crowe 1985). Similarly amazing is Kornfeld's lack of reference to Benito Madariaga's many publications (e.g., 1971, 1976, 1994; Madariaga and Fernández 1985) on the exploitation of shellfish in the Paleolithic of Cantabria (and, for that matter, Ortea's [1986] and Menéndez, Straus, and Clark's [1986] detailed studies of the significance of marine molluscs and fish respectively at La Riera, and Anne Eastham's study of the role of birds not only at La Riera but also at many other Vasco-Cantabrian, other Spanish, and southern French sites [Eastham 1984, 1985, 1986a, b, 1990, 1995]). In no particular order, some of my comments are as follows:

1. Kornfeld seems to forget that it was precisely Clark's and my research at La Riera Cave that highlighted the fact that "Mesolithic-like" broad-spectrum subsistence had begun as early as ca. 20,000 B.P. in Cantabrian Spain, notably with our demonstration of the importance (occasionally contested but true) of gathered shellfish to the diet in the Solutrean. We began arguing for "intensification" (not just specialized big-game hunting) long ago, and such evidence for diversification already existed in the literature at least for the Lower Magdalenian (for example, from the original excavations at El Juyo by P. Janssens and González Echegaray [1958; see Freeman 1973, 1981; Straus 1977]). To assert that only "recently, the presence of other resources in Paleolithic diets has been suggested" is to misinterpret the (much-published) results of the La Riera Project (e.g., Straus et al. 1980, 1981; Straus and Clark 1986) and the Cantabrian record as a whole. This kind of misstatement cannot be allowed to stand. This is not to say that our original studies were without faults. It is, after all, the very fact of subsistence diversification in Cantabrian Spain that has been the basis of my (again, much-published) pointed comparisons with contemporaneous systems in reindeer-rich southwestern France (notably at the sites of Duruthy and Dufaure, the latter of which I excavated in the 1980s, just to the north of the Pyrenees) (Straus 1983, 1993a, b, c; Altuna et al. 1991). The theme of Late Upper Paleolithic subsistence diversification has been taken up and built upon in Iberia not only by Can-

tabrian colleagues but also in Levantine Spain (e.g., by Emili Aura and Manuel Pérez [1992, 1995], Jordi Estévez [1980], Valentín Villaverde and Rafael Martínez [1995], and others) and in Portugal (by João Zilhão [1992, 1995a]). Kornfeld is right to warn against overemphasizing large mammals in the diet simply because of the prominence of their preserved remains, especially in regions whose records are dominated by cave sites, but the building of straw men is not really helpful.

2. Kornfeld's knowledge and reconstruction of "Late Pleistocene" environments in Cantabrian Spain are strikingly limited and oversimplified. Yes, there were probably plant foods edible and actually eaten by humans, especially during the more temperate oscillations (and I and others have published on the presence of chopping tools and grinding stones in the record [e.g., González Echegaray and Barandiarán 1981, Straus 1986]—although many of the grinding stones were used on ochre) but probably not many during the glacial maximum. But for which *specific* climatic phases is he giving his "optimistic" reconstructions? Even the existence or relative "lushness" of some of the traditional "interstadials" ("Lascaux," "Laugerie") has recently been challenged (in, among other places, this very journal) by the Basque palynologist María Fernanda Sánchez Goñi (1991, 1992, 1994). Indeed, where in the copious regional palynological literature by Arlette Leroi-Gourhan, (e.g., 1971, 1986), Anais Boyer-Klein (1976, 1980, 1981, 1984, 1985; Boyer-Klein and Leroi-Gourhan 1985), and Michèle Dupré (1984, 1988, 1990) (last-glacial interstadials included) can Kornfeld find evidence for such luxuriant vegetation as he purports to present for the Late Pleistocene of Cantabria? Many spectra have no arboreal pollen other than a few pine grains (and they could be extraregional, due to long-distance transport). Granted, there probably were trees and shrubs in refugia in the mountains, but let's not minimize the existence of very rigorous conditions especially during the last glacial maximum (amply demonstrated by CLIMAP [1976] and Karl Butzer [e.g., 1986]), and let's not simplify too much! Provocative speculation can be useful, but not in the face (or ignorance) of abundant relevant facts. To proclaim that medium-large ungulates were merely secondary food resources in the Ice Age environments of Western Europe is patently absurd and very different from quite reasonably suggesting that, especially in mountainous, mosaic, ecotone-rich habitats of southern Europe (especially along coasts), plants, fish, shellfish, and birds could have been secondary foods of particular *seasonal* significance to humans *when available*. The latter is what we have been arguing for years and tried to help demonstrate at La Riera by getting Nicolas Shackleton and his then-student, Margaret Deith, involved in using oxygen isotope analysis to determine the seasonality of mollusc gathering (Deith and Shackleton 1986, Deith 1983).

3. In developing his so-called model, Kornfeld pays no attention to the Upper Paleolithic seasonality data that Altuna and others (summarized in Straus 1986) have been developing for this region over the past 25 years.

How can an article be taken seriously if it merely speculates and does not try to anchor itself in at least some of the facts that are presumably relevant to such model construction?

4. Kornfeld uses my very incomplete faunal counts from a 20-year-old article (Straus 1977) as somehow being thought by me to be an accurate representation of *all* animals hunted in the region during the *whole* Upper Paleolithic. This is utter nonsense. The number of sites that (1) have been found, (2) have been excavated, and (3) have had meaningful faunal analyses done is still very small (but much larger now than in 1975, when I wrote the article for Lewis Binford's book). Clearly, just as we are only seeing the tip of the iceberg in terms of gathered foods (even shellfish), so too do we lack anything at all close to a total sample of sites or of mammalian faunal assemblages for even this relatively well-studied region over a 30,000-year time period. Yet one must work with the data at hand, and ungulate game was a very significant resource in this region, even if it was increasingly being supplemented by other foods that were fished or gathered when and if available.

5. Similarly, Kornfeld seems to think that Clark and I believe La Riera to have been continuously occupied. This is also nonsense. There are obviously significant hiatuses in the stratigraphy (as a careful reading of the monograph will show). The site was not occupied "year after year for millennia," and in fact, on the page of my 1996 article that Kornfeld cites, I state that sites like La Riera (which is part of a cluster of sites) would have been abandoned for such possible reasons as vermin accumulation or death taboos. To say, as I have, that some of the occupations of La Riera (and other caves) were "long-term" (while others were clearly "short-term") is not to say that occupations were "semipermanent" as Kornfeld claims. Again, we know that the record we excavated at La Riera is very partial. But Kornfeld should not project his apparent lack of knowledge about the record upon us.

6. I am mystified at Kornfeld's dismissal of our suggestion that some of the chronologically early Solutrean occupations at La Riera may have to do with ibex-hunting parties on the nearby cliffs. Such sites are well documented throughout the Cantabrian Cordillera and Pyrenees, as I and others have shown (Straus 1978b, Delpech and Villa 1993), although the other sites are right on the mountain slopes. The La Riera levels in question (4–6) are quite distinctive in their large quantities of weapon tips, relative scarcity of other tools, high percentages of ibex remains, nonlocal lithics, scarcity of primary lithic manufacturing debris, lack of features indicative of anything other than very short-term stays in the cave, etc.

7. Naturally, the record in such a steep, coastal environment as Cantabrian Spain is incomplete, most open-air Upper Paleolithic sites having been eroded away or deeply buried under colluvium with the result that 120 years of archeology in the region have failed to find them (though, curiously, there are open-air Mousterian and Acheulean sites [e.g., Montes and Muñoz 1993, Morlote

and Montes 1992, Rodríguez 1983], most probably dating to interglacial or interstadial times). Yet during the documented cold, stormy times of much of oxygen isotope stage 2, the abundant caves of this region—especially those with favorable solar exposure, which were consistently chosen for human occupation—may have been *the* best places for *any* overnight stay, long or short. Thus they are likely to provide a decent sample of many/most (though admittedly not all) of the kinds of activities conducted by Upper Paleolithic people in this region.

8. Kornfeld may not like the notion of carnivory and may think that we have been misinterpreting Upper Paleolithic "art" as a result of a misperception of its importance in overall subsistence, but the inescapable fact is that both rupestral and portable art are heavily dominated by animal themes (and mainly game species at that [Apellániz 1982, Bahn and Vertut 1988, Leroi-Gourhan 1971, Bosinski 1982], even if the one-to-one correspondence between eaten animals and represented animals at individual sites has rightly been challenged [Altuna 1983, 1994; Delporte 1985; Straus 1976]). From the very beginning of this kind of art (the figurines of southwestern Germany [Hahn 1993] and the parietal figures of Chauvet Cave [Chauvet, Deschamps, and Hillaire 1995]) until its very end in the late Magdalenian, game animals were of critical importance to the survival of Ice Age peoples in Europe, so it is not surprising to see them so prominently and frequently represented on tools, weapon elements, ornaments, cave walls, and open-air ledges (e.g., Mithen 1990, Zilhão 1995b). Some would see plants as represented in Upper Paleolithic art, but these possible, ambiguous representations are relatively few (Bahn and Vertut 1988:133, with references).

Readers of the Kornfeld article would do well to (1) consult the primary literature on Cantabrian prehistory and (2) use their common sense. I have no *a priori* desire to defend orthodox views of the Upper Paleolithic of Europe (see Straus 1987a, 1991); I do, however, believe strongly that alternative scenarios must be informed by serious reference to the empirical record which generations of prehistorians have worked so hard and conscientiously to produce.

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Models of the past are very much like models of the future, because scientists are people and build their conceptions from the point of view of current psychology and practice. It is difficult to escape the traditional archaeological framework for the Paleolithic even where there is an abundance of faunal, lithic, and paleoclimatic data (as in Cantabrian archaeology). It will be much more difficult to do so for regions where our sources are limited, for example, the Great Plains, where Kornfeld lives and works and where there are many Paleo-Indian

kill sites with rather modest remains of stone industries. We do not know enough about Clovis-Folsom blade technology or about the variability of scrapers, burins, and woodworking tools. We are also confused about the Paleo-Indian way of life, types of dwellings, and storage and cult constructions. Mostly we are only in the beginning stages of such reconstructions, and Paleo-Indians are still being represented as hunters. We need to find and examine permanent/semipermanent camps for an accurate interpretation of Clovis subsistence strategy. It will be not so simple to reconstruct the paleoeconomy in the Russian Far East, mainly because of the absence of faunal remains in acidic soils. Having been involved in Far Eastern archaeology for more than 13 years, I have seen the conception of the Final Pleistocene–Early Holocene transition (11,000–9,000 B.P.) evolve from one of hunting alone to one of purely salmon fishing. Only during recent years have we begun to build a complex model of the economy based on the combination of hunting, seasonal salmon fishing, and forest and coastal gathering. The Russian Far Eastern maritime region is very similar in landform to Cantabria's intermontane valleys, and the geographical position (lat. 45–40° N) is similar too. Recent investigations have revealed not only the complexity of the transitional economy but the strong role of local climatic and faunal features in its origin. Agriculture appeared here only in the 19th century; natural resources had been exploited traditionally since the Final Paleolithic because of the lack of alternatives. For many years Russian and Siberian archaeologists employed a concept of big-game (mostly mammoth) hunting as the basis of the Paleolithic economy, but today we recognize that the large kill sites, bone/ivory remains, and “dwellings” made of mammoth bones can be explained differently. What was the main reason for killing mammoth? Food? Building materials? Ritual? Or were there several reasons? If we insist on the conservative position of the primacy of faunal remains as data and the concept of Final Paleolithic people as big-game hunters, we can interpret the transition to the Mesolithic or Neolithic (in the Far East) only as “shock therapy.” People would not have been able to elaborate a highly developed economically and socially stratified world. The work of Kornfeld opens a window onto such a world in ancient Cantabria.

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Kornfeld's paper questions current interpretations of the Cantabrian Upper Paleolithic economy. Most of his criticisms seem appropriate and valid, although his alternative model is not free of inconsistencies.

The yearly accumulation rates calculated for La Riera faunal and lithic remains illustrate quite well the point that such a small cave cannot possibly have played the role of a residential site with long-term semipermanent occupations. Kornfeld also suggests that this may indi-

cate, alternatively, that big-game hunting was not the basis of the Cantabrian Upper Paleolithic economy. Such an alternative interpretation would also be reinforced, in his view, by the fact that the total amount of large herbivore bones recovered in all sites known represents a marginal percentage of what would have been needed to feed a group of 500 people for the 27,500 years of duration of the period.

This presentation of the issues overlooks a major problem of the Cantabrian record: that it is entirely based on cave sites. It is a common assumption among researchers working in the area that, given the abundance of caves and their even distribution across the landscape, people would have systematically preferred to seek shelter in such locations. Thus, instead of indicating that big-game hunting was not the basis of the economy, the pattern exposed by Kornfeld can also be interpreted, perhaps with a greater degree of likelihood, as a reflection of the fact that this assumption is wrong: although they are the only type of site currently known, caves probably represented, originally, a minor component of the settlement system of Cantabrian Upper Paleolithic groups. Such is the case, at least, in Portugal, where open-air sites are far more common than cave sites (Marks et al. 1994, Zilhão et al. 1995b).

It is possible that the lack of open-air sites in Cantabria is due to visibility problems caused by patterns of modern land use or by Holocene sedimentation. Since the Upper Paleolithic record of Mediterranean Spain suffers from the same problem, however, one has also to consider the hypothesis that the differences between Portugal and Cantabria are not due to environmentally induced preservation factors but instead relate to traditional research biases favoring the excavation of sites with good preservation of bone and, consequently, of mobiliary art. Whichever explanation one favors, it is in my opinion quite clear that the main aspect of the problem of sample representativeness in Cantabria is not one of quantity of sites or of bones but one of kind—only one type of site is represented, and it is one that in all likelihood corresponds to special-purpose, temporary occupations. Residential camps must have been located in the open, and, since their characteristics are entirely unknown, many different models can indeed be presented to accommodate the evidence from cave sites, including that presented by Kornfeld.

In principle, I would tend to agree with Kornfeld that, considering the reconstructed paleoecology of Cantabria, a more forager-oriented settlement-subsistence system makes more sense than has hitherto been admitted. I have argued along similar lines for the Portuguese case (Zilhão 1995a) on the basis of an explicit parallel with the Ona of Tierra del Fuego, who are known to have preferred to move the camp to the kill instead of moving the kill to the camp. In that model, the cave occupations known in Portuguese Estremadura were interpreted either as special-purpose hunting sites, caches and traveling shelters, or sites of single-family residence during seasons of band dispersal. From the characteristics of extensively excavated open-air sites it was also inferred

that such sites probably represented spatially dispersed palimpsests of short-term occupations by relatively small groups, indicating a relatively high level of residential mobility, as would be expected in the context of a foraging strategy.

I am not sure, however, that such a model would accommodate the Cantabrian evidence equally well. To begin with, there is one aspect of Kornfeld's model that is testable against the extant archaeological record: the role played by small animals. In Portugal, as is also the case in Mediterranean Spain, rabbits make up the vast majority of bones recovered from Upper Paleolithic sites. In Cantabria, however, lagomorphs, although present in the environment, do not seem to have been exploited by humans. In spite of the importance of aquatic resources, particularly after the Solutrean, Cantabria differs significantly, in this regard, from the rest of Iberia (and from the Ona, among whom the tuco-tuco, a lagomorph-sized rodent, also represented a major source of food).

A related issue is that of the confusion between economic importance and percentage of dietary intake. Kornfeld seems to assume that the two variables are directly related: if big game did not supply most of the food, then we are not dealing with a big-game-focused economy. I do not think that this is valid reasoning. Even contributing to the diet less than plants and small animals, big game may still have been the most important resource. Kornfeld implicitly acknowledges this when he notes that exploitation of large herbivores may have dominated the system in late fall and winter, that is, in the critical seasons of the year. If so, they were the key to survival even if, in terms of year-round dietary inputs, they were not as important as the other food sources. The overwhelming presence of large herbivores in the domain of artistic representation also points in the same direction. Since such overwhelming presence is characteristic of Upper Paleolithic art in all of southwestern Europe (including Portugal and Mediterranean Spain), I would tend to accept that the economies of the time were indeed big-game-focused despite regional variations (determined by differences in environmental conditions and correlative settlement-subsistence strategies) in the percentage of overall food intake that large herbivore species actually represented.

Reply

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The purpose of my paper was twofold: to discuss the ambiguity in archaeological data, specifically the fact that observed patterns can be interpreted in different ways and this interpretation is partially based on a priori assumptions and the questions we ask, and to present an alternative scenario for Cantabrian Upper Paleolithic

settlement and subsistence strategies. The commentators generally address these issues. My reply is divided into three very general topics: (1) middle-range theory or methodology, (2) generalization, the completeness of the archaeological data, and empiricism, and (3) broad-spectrum subsistence strategies and problems of site formation.

1. I did not imply, as Enloe suggests, that the social milieu determines the past, only that it influences our perceptions of the past, the questions we ask, the answers we are willing to accept, and other significant steps in the scientific process. I certainly acknowledged the role of methodology in my introduction by suggesting that middle-range theory plays a role in "developing unambiguous signatures of past behavior," and, in fact, I see this paper, especially the section on model implications, as contributing to this endeavor. As Mora notes, "the interpretations obtained from 'archaeological data' are in fact categories that are open to discussion." I hope that this paper has broadened the discussion to include a potentially greater role for plants than has heretofore been suggested. Again in Mora's words, "how can interpretations with regard to human subsistence be reliable if the role of resources considered 'secondary' is not taken into consideration? And how do faunal patterns reveal adaptive strategies?" In the model implications I suggest a means of interpreting the archaeological record consistent with the proposed model. That the role of non-big-game resources in Upper Paleolithic subsistence strategies is well accepted, as Straus and Clark suggest, is simply not credible. Consider the leading paragraphs of Straus's discussion of Solutrean subsistence (1992:111, emphasis mine):

We have no evidence of plant foods from the Cantabrian Solutrean. Although at times there *probably were small numbers of nut-bearing trees* in the region (particularly during the Laugerie and Lascaux oscillations), the open heaths and grasslands were *unlikely to have provided substantial food for humans*.

The main sources of food were five taxa of ungulates. . . . In addition, Solutrean levels . . . for the first time contain substantial marine mollusks. . . . The shellfish may have served as a winter "tiding over" resource. . . .

Some sites . . . have small number of bird remains. These, along with rodents, lagomorphs, and insectivores found at some sites, *may not necessarily have been entirely the result of capture by humans*.

In the discussion of the Magdalenian, Strauss again stresses the role of big game in subsistence 1992:146–47, emphasis mine):

There are abundant data to support the view that intensification of subsistence continued apace during the Tardiglacial. Most of the same animal species hunted (or gathered) in the Upper Pleniglacial were also taken by Magdalenian humans, although by this time rhinos and mammoths were nearing or had

reached extinction in northern Spain. . . . *There is no escaping the overwhelming importance (even if underestimated) of two medium-size ungulates in Tardiglacial subsistence: Cervus and Capra.* To obtain some idea of the importance, *consider a few striking values for number of identified specimens (NISP: Cervus, 1639 in La Riera Level 18 . . .).* . . . The killing of large numbers of red deer and ibex, usually at different times and places . . . was a regular and key part of Magdalenian subsistence.

These represent fairly typical statements of many prehistorians of Upper Paleolithic Cantabria and almost universally of prehistorians from other parts of Eurasia. Straus's comments in particular manifest a resistance to more than a secondary role for plants. I do not see much of a role for non-big-game resources in these statements, and I do not see an attempt to model their potential role or to suggest how such a role may be visible in the archaeological record, which is dominated by fauna.

I see nothing wrong with modeling subsistence strategies in terms of the use of non-big-game resources, when there is a good reason to do so, and deducing implications about the faunal record from such models. As Tabarev notes, in the Russian Far Eastern maritime region fauna is not well preserved or entirely lacking in much of the archaeological record. Are we unable to discuss subsistence at all in that case? Of course not, but the means by which we discuss subsistence in such cases is based not on "direct" evidence but rather on implications of subsistence strategies for the settlement system or other aspects of the archaeological record.

2. Straus, Clark, Kozłowski, Boyle, and Enloe bring up the incompleteness of the faunal record and issues concerned with making generalizations about the subsistence system from the Cantabrian fauna. Straus says, "One must work with the data at hand, and ungulate game was a very significant resource in this region," but the overwhelming presence of ungulate game remains in Cantabrian sites is no guarantee of their significance as a prehistoric resource, given what we know about the archaeological site formation and the vagaries of preservation (Gifford 1991).

In the past few years there has emerged an abundance of information showing the non-big-game component of the Cantabrian Upper Paleolithic subsistence, as Straus and Clark correctly point out. This is exactly the kind of information we need. However, to use this information to understand the past, we must place it in the context of non-big-game-oriented settlement and subsistence strategies, which is precisely what I attempted to do. What can now be hoped for is that (1) the details of resource exploitation as seen in these new data will be integrated with the model I have constructed, (2) the model will be refined in terms of the new data, and (3) still newer and different data will be sought in the archaeological record from the perspective of this model. A clearer understanding of Cantabrian Upper Paleolithic will undoubtedly emerge from such a process.

Although Clark and Straus are correct that their in-

vestigations at La Riera identified widespread use of non-big-game resources, the reader comes away with the impression that big game was the staple throughout the Paleolithic and the rest were insignificant supplements. This point of view is, as Zilhão notes, affected by overreliance on cave sites. Other types of sites, only a few of which are present, have neither the degree of preservation of cave sites nor their 120 years of investigation history, and although we are learning new things about the Upper Paleolithic from these sites, cave sites still dominate our perception. The other problem with cave sites is the role that nonhuman carnivores may have played in bone accumulations. These carnivores live in caves, whereas humans generally use them as temporary shelters. It is absurd to think that carnivores did not have a significant effect on bone accumulations in Cantabrian caves, and that effect has been noted by a number of researchers (e.g., Binford 1982b, 1983; Straus 1979b), although Straus clearly underplays its significance.

Clark protests being labeled a "strict empiricist," since he has written many "papers that address epistemological issues in precisely the type of research under discussion here." I did not label him a strict empiricist; I only suggested that making empirical generalizations from the Cantabrian Upper Paleolithic fauna about settlement and subsistence strategies is inappropriate. However, by arguing that we need more detail, as in his statement that "we are not yet in a position to quantify the roles they [plants] played in Late Upper Paleolithic and Mesolithic economies," he is relying on an inductive argument (e.g., Binford 1978b). The nature of the faunal sample and the making of empirical generalizations is also a point misunderstood by Boyle, Enloe, and Kozłowski. I never said or implied that I consider the archaeological record complete. My point was simply that to present a plausible reconstruction of past subsistence we must understand the nature of the archaeological record (the sample we are dealing with). One aspect of this understanding is some notion of the relationship of our sample to a population. In the case of Cantabria, the faunal sample is very, very small indeed. Empirical generalizations are unsatisfactory explanations (Binford 1978b), even under the best of circumstances. My only point is that deductive expectations about the archaeological data provide an explanation about the past while generalizations do not. Furthermore, expectations can be deduced only if possible alternative models (scenarios) are available. My paper provides one such model for the Cantabrian Upper Paleolithic.

3. Mora and to some degree Zilhão seem to agree with me about the possibility of settlement strategies with some emphasis on non-big-game resources for the Cantabrian Upper Paleolithic. Mora thinks it "not too unreasonable" to interpret the archaeological record as being produced by small, mobile groups, that is, by people practicing a foraging strategy. Zilhão has argued for a foraging model based on the Ona for the Upper Paleolithic of the Portuguese Estremadura. He is not, how-

ever, sure how appropriate his model may be for Cantabria. And Kozłowski suggests that hunter-gatherer settlement and subsistence strategies may be more flexible and complex than commonly presented and that demonstrating such variability would be useful.

Mora correctly suggests that the recovery of charcoal, seeds, and other vegetal remains and studies of use-wear and organic elements on tools, various isotopic analyses of human remains, and examination of dental wear patterns are necessary for the interpretation of subsistence strategies and thus for the evaluation of the proposed model, and I am sure that such studies are currently under way. These types of analyses, however, have to be put into systemic context, and the model I proposed provides one such context.

Mora's comment about site formation is even more significant. The lack of such investigation puts inferences based on Cantabrian sites in an uncertain position. We simply do not know if the cave levels from which fauna were recovered represent one or many events and how the time axis affects the assemblages. Citing Enloe (1991), Mora suggests refitting as a means of gaining a better understanding of the time and tempo of deposition. To this I might add other approaches that will yield a more complete understanding of site formation (Schiffman 1987). This is certainly a powerful argument for a reanalysis of curated collections and one that I hope will be seriously considered by future Cantabrian researchers.

Finally, Tabarev's view from the Russian Far East maritime region is instructive. For one thing, there are geographical similarities between Cantabria and the Russian Far East. Second, Pleistocene–Holocene-transition models have undergone a hunting-to-salmon-fishing transition in this region, with the most recent models combining the several resource exploitation strategies into a more complex model of subsistence. Thus, in the Russian Far East maritime region as elsewhere, single-species specialists (usually hunters) are replaced by broad-spectrum foragers (Kornfeld 1994:1). It is in examining this trend that data collected by Straus and Clark, among others, will be useful. However, many models (scenarios) of the past may accommodate these data; only by presenting different plausible scenarios can we begin evaluating their utility for understanding the past. My paper presented one scenario that I and several commentators think may represent a plausible view of Cantabrian Upper Paleolithic subsistence.

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Errata

Bob Janes's comment on Haas's "Power, Objects, and a Voice for Anthropology" in the February 1996 supplement contains a typo that changes the meaning of the sentence in which it appears. On line 18 of p. S14, "need to be dichotomous" should have read "need not be dichotomous."

Emiko Ohnuki-Tierney points out that the (temporary) mailing address given with her review in the June 1996 issue is not her institutional affiliation, which is Department of Anthropology, University of Wisconsin–Madison, Madison, Wis. 53706-1393, U.S.A.

Serials

Dolní Věstonice Studies is a series focusing on the complex archaeological and paleoanthropological record from Upper Paleolithic sites in the Czech Republic. Initiated by the Department of the Paleolithic and Paleoethnology of the Institute of Archaeology of the Academy of Sciences of the Czech Republic at Brno, the volumes published so far have contained a synthesis of the Paleolithic in the general area (vol. 1) and detailed

descriptions of the finds from Pavlov (vol. 2) and Dolní Věstonice (vol. 3). Because of financial difficulties, publication of the second and third volumes was kindly undertaken by ERAUL in Belgium. Beginning with the fourth volume, publication will return to the Czech Republic. For further information, write: *Dolní Věstonice Studies*, CZ 691 29 Dolní Věstonice 25, Czech Republic.