

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 Paleoecological 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.