Research and Development in the Stone Age: Technological Transitions among Hunter-Gatherers[1]

by Brian Hayden

THE TRANSITION FROM PALEOLITHIC TO MESOLITHIC and the homologous transition in the New World from Paleo-Indian to Archaic lifeways are among the most important developments in world prehistory. The Mesolithic and Archaic not only represent a major change in technology, but also constitute the staging ground for domestication. Their emergence is thus central to our understanding of cultural change. The following is an exploratory essay for explaining the emergence of the Mesolithic/Archaic. The analysis relies on the pioneering work and ideas of others. Though the basic premise—that stress makes change adaptive—is hardly new, I shall introduce several new variables and point out the shortcomings of current population-growth models.

[1] Some of the basic concepts proposed in this paper were first formulated during 1974 in a field school dealing with the Rogers site, a Paleo-Indian, Archaic, and Woodland site in the suburbs of Richmond, Va. I am grateful for the cooperation and assistance of the Department of Sociology and Anthropology of Virginia Commonwealth University and to R. G. Matson of the University of British Columbia for stimulating conversation. I owe special thanks to Brian Spurling (Simon Fraser University) for introducing me to the concept of *r* and *K* selection, to Johan Kamminga (La Trobe University, Victoria), Charles Morgan (University of Victoria), Michael Schiffer (University of Arizona), Aubrey Cannon and Peter Brobrowsky (Simon Fraser University), Richard Shutler (Simon Fraser University), and the referees for their critical comments and careful reading of early drafts. Thanks go to Linda Hale for upgrading the clarity and grammaticality.

BRIAN HAYDEN is Associate Professor of Anthropology at Simon Fraser University (Burnaby, B.C., Canada V5A 1S6). Born in 1946, he was educated at the University of Colorado (B.A., 1969) and at the University of Toronto (M.A., 1970; Ph.D., 1976). He has been a Fulbright-Hayes Postdoctoral Fellow in Australia (1971–72) and has taught at Virginia Commonwealth University (1973–74). His research interests are lithic technology, hunter-gatherer adaptations, cultural ecology, and ethnoarchaeology among hunter-gatherers and Mesoamerican communities. He has published "Population Control among Hunter/Gatherers" (*World Archaeology* 4:205–21); "The Carrying-Capacity Dilemma," in *Population Studies in Archaeology and Biological Anthropology*, edited by Alan Swedlund (Society for American Archaeology Memoir 30); *Lithic Use-Wear Analysis* (New York: Academic Press, 1979); *Paleolithic Reflections: Lithic Technology of the Australian Western Desert* (Canberra: Australian Institute of Aboriginal Studies, 1979); and "Subsistence and Ecological Adaptations of Modern Hunter/Gatherers," in *Omnivore: Gathering and Hunting in Human Evolution*, edited by G. Teleki and R. Harding (New York: Columbia University Press, 1981).

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The generally accepted characterization of Pleistocene (Paleolithic and Paleo-Indian) adaptations includes nonpermanent habitations, little in the way of true midden accumulation, and a relatively simple technology. Most indicators show that groups were highly mobile and that the exploitable resource base was much more limited than in succeeding periods. Consequently, populations were smaller, and marginal habitats may have been uninhabited. Once the Pleistocene hominid hunting adaptation was established, large to medium-sized game became the preponderant faunal staple (Isaac 1971:291); whether meat was more important than plant foods cannot be determined. Technological and stylistic traditions with very wide geographical distributions (e.g., the Clovis, Folsom, Mousterian, and Aurignacian traditions) often characterize these Pleistocene adaptations. The succeeding Mesolithic/Archaic traditions were characterized by two major trends: general *diversification of resources* exploited in areas of poor-to-moderate resource richness and a tendency toward *specialization in habitually exploited resources* in resource-rich areas (e.g., fish on the Northwest Coast, wheat in the Near East).

Resource diversification is reflected in many aspects of materials related to subsistence: (1) More varied and often smaller animals were exploited. (2) A ground-stone tool assemblage, probably used for processing seeds and other materials, emerged. Peterson (1968) has shown that mortars and pestles were important in extending the effective food supply by reducing otherwise inedible cartilage or masses of small bone to edible pulp, by mascerating foods for the aged and very young, and by pulverizing toxic starches such as acorns for leaching. (3) Fishing techniques improved, and fish came into regular use; the record includes bone fishing tools such as leisters, gorges, harpoons, and fishhooks. (4) Although shellfish provide relatively little protein (Osborn [1977:172] notes that it takes 83,422 mussels to make the protein equivalent of one deer) and although shellfish in sediment habitats (e.g., clams as opposed to mussels and limpets) are inefficient to collect with simple technology, they were regularly exploited in some areas. (5) The bow and arrow and the dog contributed to greater hunting effectiveness. (6) There was widespread burning of forests, ethnographically recorded among hunter-gatherers as a way of increasing prime grazing area and animal density (Suttles 1968, Jones 1969, Hallam 1975). (7) Judging from the occurrence of significant amounts of fire-cracked rock, boiling of foods may have become widespread; this could have rendered edible many plants containing toxins and could have been used to extract otherwise unavailable fats, marrow, bits of meat, and other tissue from animals, as well as to reduce calorie loss from charring. Many of these innovations appeared prior to the emergence of the Mesolithic/Archaic, but they were late and remained relatively infrequent until after the end of the Pleistocene (Mouer 1975).

As evidence for specialization in habitually exploited resources we find less diversity in faunal remains, massive amounts of shellfish remains in some areas, a reduction to only a few types of tools (generally those used in obtaining and processing the major staples), and the introduction of ground-stone processing tools such as ground-slate knives. It can be inferred from the presence of such knives in some areas that techniques had been perfected for capturing resources en masse (in many cases, probably fish).[2]

Other changes tended to occur with the Mesolithic/Archaic innovations, especially in areas of resource specialization: increased seasonal sedentism and regular reoccupation of sites in some areas, the appearance of edge-ground wood-cutting implements (axes, adzes, chisels), a more pronounced regionalization of styles, and regionalization of subsistence specialization. Later, in some Mesolithic and Archaic societies, purely ornamental artifacts, which can best be described as primitive valuables (Dalton 1975), often accompanied burials. These included copper, shell, and bone pendants, asphalt and carved-stone pieces, and pipes. With large-scale circulation of these valuables in Late Mesolithic and Archaic times, regional differences once again became less pronounced and the areal extent of individual traditions greater. In North America, the use of local raw materials, often poor in quality, as opposed to very high-grade exotics characterized the change in technological traditions. This change in material was accompanied by what has been called a "degeneration" of the quality of lithic workmanship characteristic of the Paleo-Indian stage (Willey and Phillips 1958:107–10).[3]

I shall argue that cultural ecology provides the most productive framework for explaining these terminal-Pleistocene transitions. Specifically, I shall suggest the following:

1. Populations attempted to maintain a certain equilibrium concerning the frequency with which they experienced stress and altered their behavior in response to deviations from this equilibrium.

2. The development of Mesolithic/Archaic technology cannot be explained by single episodes of climatic change or population growth. Resource bases were constantly fluctuating throughout the Pleistocene, and man:land relationships at a general level did not significantly change from the Oldowan to the Neolithic.

3. This periodic resource stress was the most important motive for patterned, directional change—i.e., toward minimizing the immediate effects of resource shortage and instability.

4. Although groups generally coped adequately with resource stress by using established strategies and techniques, on occasion new efficient ways of obtaining or using food were developed.

5. New efficient ways of obtaining or using food primarily involved increasing the effectiveness of obtaining species already used and/or increasing the number of species exploited. Initially, species diversification meant exploiting large-bodied resources that were dangerous and difficult to obtain.

6. Because use of increasingly diverse resources was closely linked with the use of technologically specialized equipment, overall technological complexity increased with staple-food diversity.

7. The strategy of diversifying resources eventually encompassed all the large-bodied species. At this point, further increase in resource reliability depended on the harvesting of *very* small species. These, however, were exceedingly difficult to procure economically with simple technology and required complicated processing or storage to make exploitation worthwhile.

8. Small resources have special properties of great importance to cultural evolution. Because small species were relatively inexhaustible and had much higher biological productivity than larger species, the resource base became significantly more stable and abundant in some areas, leading to major changes in other aspects of the cultural system: increased sedentism, regionalism, population density, competition, and ranking; reduction in most areas of the vast subsistence-related alliances of the preceding period; and occupation of habitats that could not previously support human populations.

9. Early domestication and horticulture can most profitably be viewed as logical extensions of the strategy of increasing resource reliability, as can many of the subsequent and contemporary changes in subsistence technology.

PREVIOUS EXPLANATIONS OF THE MESOLITHIC/ARCHAIC TRANSITION

Previous explanations of the transition from Pleistocene to Mesolithic/Archaic traditions have invoked the notion of population pressure—in its broadest sense, stress resulting from an imbalance between population and resources. Imbalances of this kind in Pleistocene populations are usually seen as stemming from one of two sources: climatic change or population growth. Advocates of each of these causes of the Mesolithic/Archaic transition tend to discount the other.

The climatic-change explanation suffers from the fact that, in the Old World, climatic and environmental instability throughout the Pleistocene is well documented. Not only were all types of climate represented, but dramatic fluctuations in climate and environment occurred at least ten times (Shackleton 1975; Isaac 1975:876; Butzer 1977:578) during the 1,500,000 years that man inhabited Europe (de Latil 1978) and the Middle East. Striking changes also occurred in wooded and savanna lands of North and Central Africa. If climatic change was essential to the cultural transition of the terminal and post-Pleistocene, why did that transition not occur earlier? Why should only the last of a series of climatic changes have resulted in the transition out of the Paleolithic stage? By itself, the environmental model is incapable of dealing with this question. Short- and long-term climatic changes do, however, constitute a source of stress, which is an important element of a more comprehensive explanation.

Perhaps because of the more limited duration of occupation in North America, the transition to the Archaic is frequently portrayed as a simple adjustment to changing environment. The post-Pleistocene period is viewed as permitting for the first time the exploitation of nuts, grasses, anadromous fish, and shellfish, of which people naturally took advantage. Statements such as "Environmental changes affecting species diversity led

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[2] Briefly, this interpretation rests on an argument I have developed to account for the appearance of ground-stone implements (Hayden 1977). I argue that the major variables determining the use of chipping vs. grinding resharpening techniques were (*a*) the amount of material being processed, (*b*) the number of resharpenings involved over given periods, and (*c*) the availability of good-quality chipped stone. When the amount of material processed required so many resharpenings that large amounts of chipped stone were needed, it became more economical to adopt a mode of resharpening such as grinding, which prolonged the life-span of tools. This is true no matter what the material being processed.

[3] For further documentation of the general characteristics of these periods, see Willey and Phillips (1958), Caldwell (1958), Ford (1974, 1977), Funk (1978), and Isaac (1971).

to a broadened food base" (Ford 1974:400) are extremely common. However, it must be pointed out that the same species would have existed in refugia or in lower latitudes during the glacial periods and that the technological innovations which characterized the Archaic were not to be found anywhere in the world until the terminal Pleistocene. Thus, in North America as in the Old World, simple, single-event environmental change at the end of the Pleistocene is insufficient to explain the change in technological traditions with which we are concerned.

The idea that population growth led to major stresses resulting in technological and subsistence developments has been made popular in recent years by Boserup (1965), but it has a number of major drawbacks.

The population-growth model has two common variants. The first is simply that there was no population pressure during the Pleistocene because population increased extremely slowly, finally filling the earth to capacity by the end of the Pleistocene. In the vast majority of publications adopting this view, the question of why populations grow is not even addressed. Where there is an awareness of the importance of the underlying assumption that population generally increases beyond carrying capacity, either the assumption is simply stated as an assertion or the reasons for population growth are considered a problem for others to solve (e.g., Boserup 1965). Statements such as that the authors "strongly favor the position that population growth is a general phenomenon and human reproductive behavior generally is unlike that of most other species only in its tendencies towards sustained growth" (Sanders, Parsons, and Santley 1979:364) typify the theoretical justifications. As Flannery (1976:225) has noted, "The pressure is simply there," and it has become one of the most overworked explanations in contemporary archaeology. Population growth seems to account for everything (see, for example, Carneiro 1958, 1970; Cohen 1977; Glassow 1972, 1980; Jolly and Plog 1979; Pfeiffer 1977; Sanders and Price 1968; Smith 1972; Young 1972). Following the suggestion that throughout the Pleistocene the world was filling up and population eventually overflowed capacity just prior to domestication some 12,000 years ago, this model can be called the "bottle" model of population growth.

The second variant assumes that with the establishment of sedentary lifeways, populations began to expand exponentially, creating such stress that the increased work associated with domestication and social stratification became adaptive. By direct statement or by implication, and given arguments on the inherent limiting factors of nomadism, readers are left to assume that population stress was of relatively little importance prior to sedentism (e.g., Smith 1972:11). This might be called the "homebody" model of population growth. The theoretical assumptions of this model have been more explicitly argued than those of the bottle model, but the implications generally do not fit observations of hunter-gatherer demography.

Despite criticisms by Bronson (1975), Brumfiel (1976), Cowgill (1975), Hassan (1973, 1978), Weiss (1978), and others, population-growth arguments persist. Some prehistorians even view interpretations based on factors other than population growth as reversions to nonexplanation. Further discussion of these arguments is therefore in order.

The bottle model fails to account for cultural developments at the end of the Pleistocene for a number of reasons:

1. Its proponents have not shown why or specifically how human population growth is different from that of other animals.

2. It would appear that many contemporary hunter-gatherers have maintained stable populations over long periods and have used both cultural and biological population control mechanisms to this end (Hayden 1972). If this is true, one must use elaborate auxiliary hypotheses to explain why Pleistocene and Holocene hunter-gatherers would not similarly have kept their populations in balance with their resource base.

3. The model implies that areas which had been colonized for only 20,000–50,000 years (such as the Western Hemisphere) should exhibit a crisis of population pressure at nearly the same time as areas which had been colonized for well over 1,000,000 years (such as the Near East), with the semicontemporaneous domestication of wild species in both areas. Even taking into account Cohen's (1977:62–65) "flux" effects, this scenario is highly unlikely when one considers the very limited access between the two hemispheres and the constant growth dynamics postulated.

4. As Weiss (1978) has pointed out, it is much more difficult and complex to maintain a constant but imperceptibly low rate of growth than it is to maintain either zero or rapid population growth. The model thus seems improbable because of its complexity. Even a rate of growth on the order of 0.1% per year would have saturated the world in a mere 15,000 years (Hassan 1978:82).

5. In the light of theoretical arguments (Hayden 1975) and ethnographic observations (Colson 1979, Hayden 1981), it seems clear that no group could avoid periodic imbalances with its resources. Dramatic short-term climatic changes, as well as long-term ones, may create famine conditions almost irrespective of population density. Thus it would seem that there was recurring stress throughout the Pleistocene and that stress at the end of the Pleistocene was new neither in kind nor in degree.

6. Even taking into consideration biological variables which tend to limit populations, such as fat levels, lactation sterility, and the inability to nourish adequately more than one child at a time, there is still a clear net potential for substantial long-term population growth among nomadic hunter-gatherers (Peterson 1979:122; Birdsell 1953, 1957; Hassan 1978; see also Bongaarts 1980). If Pleistocene hunter-gatherers did not achieve their maximum growth potential, other factors must have maintained some sort of equilibrium. Why should these equilibrium-maintaining factors suddenly have disappeared at the end of the Pleistocene?

A more useful, realistic scenario of population growth and stress in the Pleistocene would probably be one in which stress is unavoidable, although its frequency can be controlled to some degree by culturally limiting population growth. The population growth cited by advocates of the bottle model can best be viewed as the *result* of an improved technology that increased resource bases over time while keeping population and resources more or less in equilibrium.

A number of archaeologists and ethnologists have argued that the nomadic life-style of hunter-gatherers, particularly the necessity of carrying infants, resulted in long child-spacing intervals, thereby severely restricting the growth potential of populations. Once groups became sedentary, the spacing of children could be reduced, resulting in higher birth rates, exponential population growth, and significant increases in population pressure (Binford and Chasko 1976; Dumond 1975; Ford 1974:392; Glassow 1972; Jolly and Plog 1979:298–99; Lee 1972; Sussman 1972). There are a number of basic problems with this "homebody" model:

1. The empirical data do not fit: we know of sedentary hunter-gatherers with no history of recent rapidly (or even slightly) increasing population. Neither historical, oral-traditional, nor linguistic distributional data indicate uncontrolled population growth, e.g., in studies of several groups in different parts of Australia (Mulvaney 1975, Lourandos 1980*a*) and coastal Florida (Goggin and Sturtevant 1964).

2. If the model were correct, the populating of the entire Western Hemisphere within a few tens of thousands of years would have been impossible because of the extreme growth limitations imposed by pronounced nomadism.

3. Similarly, mankind would have been uncomfortably close to potential extinction, with a maximum annual rate of possible growth of only 0–0.0001% (Pfeiffer 1977:67; Hassan 1978:72).

What if it had been necessary to increase nomadism and child-spacing intervals because of deterioration of resource densities? Such rigid population-control systems are extremely risky genetically.

4. According to the model, there should be a *direct* correlation between the degree of sedentism and the rate of population expansion. Expanding populations should produce large areas of linguistic homogeneity such as those resulting from the Bantu expansion in Africa and the Indo-European expansion in Eurasia. This relationship can be tested using Australian ethnographic data. Birdsell (1953, 1975) has demonstrated that Australian tribal territories vary inversely in size with precipitation and that tribes have a more or less constant number of members (400–500). Therefore, the smallest tribal ranges are correlated with the highest degrees of sedentism. Since the smallest ranges occur in areas with highest precipitation, we should find the greatest degree of population expansion and linguistic homogeneity in these areas. Harry Lourandos (personal communication, 1976) also affirms that sedentism was most highly developed in the well-watered areas, such as Arnhem Land and southeastern coastal Australia. Yet there is no linguistic indication of population expansion in these areas. As is apparent in figure 1, the greatest linguistic diversity in Australia occurs in the rich, well-watered, northern coastal regions, where groups are most sedentary, and the least linguistic diversity occurs among the highly nomadic desert Aborigines. Certainly, various conditions may give rise to linguistic homogeneity, but it is difficult to avoid the implication that if the rate of population growth increased with sedentism, the well-watered areas should be linguistically homogeneous at the family level because of population expansion.

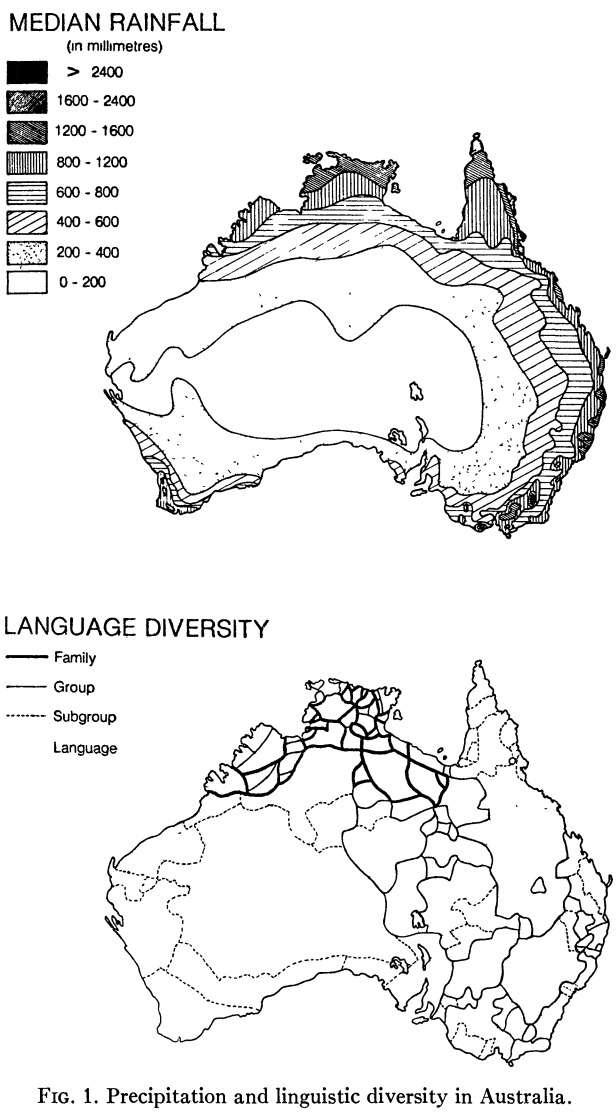

FIG. 1. Precipitation and linguistic diversity in Australia.

5. If nomadism were the only factor restraining population, then in the absence of nomadism one would expect a perfectly Malthusian situation, with populations at or beyond the limit of carrying capacity. Reality does not bear this out, since relatively sedentary groups in Arnhem Land and on the American Northwest Coast had little difficulty obtaining food (McCarthy and McArthur 1960, Sahlins 1968*a*).

6. While there is apparently a correlation between recent very rapid adoption of sedentism by hunter-gatherers in contact situations and population increase (Lee 1972, Binford and Chasko 1976), it is impossible in these circumstances to separate the effects of sedentism from the effects of the insured, reliable food supplies and improved medical facilities generally associated with the reserves, stations, and missions of welfare-oriented societies.

7. There are documented cases in which very nomadic hunting-gathering groups have increased their populations rapidly once they perceived the desirability of doing so (Birdsell 1957).

In terms of assumptions and expectations, both variants of the population-growth model accord poorly with the ethnographic and archaeological data at hand. Perhaps even more important, their proponents have been unable to come up with an unambiguous and operational measure of "population pressure." My own model of cultural development employs the concept of population pressure in the sense of environmentally induced resource imbalances leading to morbidity or mortality, but since this facet of the concept has been obscured in recent years by the association of the term with the population-growth model I prefer to use the term "resource stress."

THE RESOURCE-STRESS MODEL

My model of cultural change differs from the population-growth model in that the latter views population growth as constantly taking place even after carrying capacity is reached and under adverse conditions. Population growth is a prime mover. In contrast, the resource-stress model views populations as seeking equilibrium. Populations do not inexorably increase, but may remain stable or even decrease over long periods; population growth is not a prime mover, but at most only a contributory and not necessarily essential factor in basic technological change. The population-growth view, if sufficiently modified, may not ultimately be incompatible with the resource-stress view, but this is not now the case.

If the scenario of life and culture during the Pleistocene as depicted by the population-growth advocates is too heavily weighted on the effects of population growth, the scenario I would replace it with would include the following:

Within very broad ranges of population density, periods of resource stress inevitably occurred because of long- and medium-term cyclical fluctuations in natural resources. The frequency of resource stress experienced by a group was always a trade-off between *the cost of physical suffering* every so many years and *the cost of maintaining population controls*. The latter involved the effort of enforcement of such practices as infanticide and maintenance of strict sex and age division of labor, social roles, and dominance hierarchies as well as the defense disadvantages inherent in smaller numbers. The benefits of maintaining such a balance probably included maintaining the expansion potential (biological fitness) of human populations without much reproductive waste. The "optimal" balance between the two cost factors may have varied slightly under different environmental conditions, but the long-term attitude of bands toward what constituted an optimal or even acceptable balance of costs probably did not change much over time. I have outlined ways in which tests of this assumption might be made (Hayden 1975). Skinner (1980) has used many of these techniques and added others in a pioneering case study on the frequency and magnitude of malnutrition in a small Near Eastern population. According to the resource-stress model, research measuring rates of resource overexploitation using human skeletal remains should show little trend toward increasing stress over time.

Given the above, it is possible to envisage two important principles as guiding hominid behavior throughout the Pleistocene: first, keeping within acceptable limits the balance between morbidity/mortality related to resource stress and the cost of maintenance of population controls; and, second, minimizing the effects of resource stress *whenever* they occurred. The first principle was probably common to all primates and resulted in various means of regulating population. Only man, however, had the technological potential for making choices in accordance with the second. The fact that the densest Pleistocene populations seem to be associated with rich resource areas is consistent with the notion that populations grew to take advantage of high resource potentials whenever and wherever they occurred. This does not necessarily mean that they experienced resource stress any more or any less frequently than populations in poorer areas. It does indicate, however, that whenever technological improvements increased the resource base, the population could be expected to increase until the balance between costs of morbidity/mortality and population-control maintenance was restored.

Since subsistence concerns are of major importance to this account, a brief discussion of some pertinent ecological principles is warranted.

The principle of *maximization of resource reliability* derives largely from consideration of complex systems in an ecological context. To the traditional concept of efficiency in competitive situations must be added *reliability*. For cultures, reliability can be equated with minimizing resource stress.

Although a number of archaeologists have made passing reference to the importance of resource reliability (Cleland 1976:60; Ford 1974:392; Hassan 1978:80), the concept has not come to occupy a central position in the theorizing of prehistorians. In the ethnographic literature, resource reliability has generally been mentioned only briefly. For instance, Damas (1972:19) states that contemporary Inuit employ new technology to diversify and thereby make more reliable their resource base. Myers (1976:71) says that Western Desert Aborigines sought contact with European settlements primarily to increase resource reliability, and Bishop (1978) makes a similar observation for the Ojibwa. Colson (1979:23) has argued that the major reason new crops are often not accepted by subsistence-economy communities is that their reliability under varying conditions is either not as good as that of traditional, lower-yielding crops or cannot be assessed. I have argued that Mbuti pygmies (and other "commercial hunters") maintain close relationships with agriculturalists in order to increase resource reliability through the exchange of food and other forest products, even though in doing so they must work harder (Hayden 1981). It is clear that resource reliability is a major consideration in people's decisions concerning change and is capable of yielding directional trends over time.

Thus it is possible to suggest, as a general principle, that biological and cultural systems attempt to maximize the reliability of their resource base where such options are available. In the Early Pleistocene, the advent of technology provided a means of adapting to and modifying the environment not available to other animals, making it possible to manipulate the resource base and thus to increase resource reliability. Periodic resource unreliability and stress throughout the Pleistocene can be assumed to have provided the occasional motivation needed for technological innovation and environmental manipulation.

Increased resource reliability can be attained in two ways:

Under conditions of fluctuating resources (the conditions generally characteristic of the Pleistocene), resource reliability can best be increased by increasing the diversity of the exploitable base (Colson 1979:22–23; Hardesty 1977:43–44). This was the most common strategy for increasing resource reliability up to—and in some areas after—the end of the Pleistocene, with, of course, local changes in environment providing countervailing circumstances and masking the trend in specific cases. Most local resource stress throughout the Pleistocene was probably absorbed by other means, such as prolonged visiting in the context of social-economic alliances with other groups (Yengoyan 1976) and the numerous other strategies documented by Colson (1979) and Dirks (1980).[4] However, many of these strategies depend on the goodwill of neighboring groups and were therefore subject to some uncertainty and occasional failure. Thus, it would still have been adaptive on occasion to develop alternative, self-supporting mechanisms for increasing resource reliability. One such mechanism was technological innovation oriented toward the exploitation of more diverse animal and plant species.

Until recently it has been taken for granted that stability was largely a result of complexity (diversity). However, Hollings (1973) has pointed out the obvious—that constant environmental conditions also promote stability. They make it feasible and efficient to reduce the number of species regularly exploited, concentrating on those most easily obtained and most productive. If our interpretations are to conform to ecological theory, we must assume that a constant resource base allowed *some* Mesolithic and Archaic (and even Upper Paleolithic) groups to reduce resource diversity (e.g., in California, the Northwest Coast, Palestine). Since resource specialization is a major reorientation from the dominant Pleistocene strategy of increasing diversity, it remains to be shown how and why that strategy eventually led to constant resource levels and to a reversal of the diversification trend in some areas. This problem is the primary focus of the discussion that follows.

[4] Although reasons for the slowness of technological evolution in the Pleistocene are of interest, they are not critical to my main theme. Several explanations, not mutually exclusive, can, however, be offered:

First, the informational base may have been insufficient for complex innovations because of biological restrictions on information-storage capacity and the high memory requirements concerning precise locations and conditions of many specific resources required for survival (Butzer 1977:579). Moreover, although the tools remained the same, it probably required many thousands of years for men to acquire the capacity for employing the hunting skills that would enable them to obtain more than just young, infirm, or impeded individuals.

Assuming an occasional heavy exploitation of limited resources, any increase in the level of that exploitation in times of stress would have been more likely to destroy the resource base than to benefit the group in the long run. This fact, coupled with the relatively infrequent occurrence and generally brief duration of episodes of deprivation, would have made it preferable for the people involved simply to move out of an area rather than to attempt to devise new technological solutions that would have carried them through the stress period. In moving out, they may have joined other groups temporarily or, during times of major climatic deterioration, gradually become permanent parts of other bands and adjusted overall band populations downward. Such strategies are documented for hunter-gatherers (Strehlow 1965, Yengoyan 1976, Teit n.d.), and they could have dramatically reduced the frequency of real hardship, thereby slowing the rate of evolution. The stress-buffering strategies reported by Colson (1979) and Dirks (1979) would also have contributed to this effect.

It might also be argued that population was so regulated during the Pleistocene that resource shortages in most areas occurred only every several hundred or thousand years. Regions such as harsh deserts and arctic/subarctic habitats, where major resource shortages are more frequent and more severe (Odum 1963:70; Hayden 1981), were not inhabited until the end of the Pleistocene, and therefore we can probably take the apparent low frequency and magnitude of resource stress recorded among groups like the Hadza and !Kung as more typical of Pleistocene conditions (Hayden 1972, 1975).

A further principle important for understanding change in the Paleolithic is Zipf's (1949) principle of *least effort,* which leads us to expect that people will opt for sedentism and that, where food resources are stable, *habitually* used foods will be limited to those with the highest food return per unit of procurement effort. (For a discussion of the undesirable work load resulting from nomadism as perceived by hunter-gatherers, see Lee [1969:60].)

I have argued that infrequent but recurrent man:resource imbalances have been the primary motive for directional changes in resource utilization and that during most of the Pleistocene improvement in resource reliability was accomplished by increasingly effective use of resources and resource diversification. These changes were made possible by technological innovations and were accompanied by increases in overall technological complexity and specialization. While such considerations are essential parts of a theoretical foundation, they are not sufficient to account for specific sequences of innovations. They do not tell us why some types of resources (e.g., grass seeds or fish) were not used until the very end of the Pleistocene. To deal with the general sequence in which types of resources came to be used, some sort of developmental trajectory must be postulated, and the explanation of specific cultural changes must take into account the state of the culture along such a trajectory. A similar idea was expressed by Braidwood and Willey (1962:342) in rather simplistic terms: cultural developments occur when culture is ready. This oversimplification has been ridiculed by Binford (1968) but merits further analysis.

One type of sequential trajectory which has been used by population-growth theorists is based on food return per unit of work necessary to obtain the food: techniques yielding the lowest return per unit of work should be the last to be exploited. While this may eventually prove a useful model for agricultural and stratified societies, it is much less so for hunter-gatherers and most simple horticulturalists. Modern generalized hunter-gatherers typically spend only about two to five hours per day in obtaining food, even when employing many of the technologies the population-growth theorists consider work-intensive (Hayden 1981). There is no empirical support for the argument that the exploitation of fish along the Northwest Coast or the use of cereals in the Near East increased the average subsistence work load; in fact, experiments (e.g., Harlan 1967) indicate the opposite. Nor are there any data to suggest that hunter-gatherers earlier in the Pleistocene would have had to perform less work to obtain food; in fact, such a situation is difficult to imagine. Similarly, the idea that horticulture involved more work than hunting and gathering does not match data from horticulturalists, since groups such as the Kuikuru spend about the same amount of time in obtaining food, two to five hours daily (Carneiro 1968). It is much more likely that technological innovations made the harvesting of previously poor-return foods as productive as, or even more productive than, previous food-procurement techniques but that such increases in productivity were eventually offset by increases in population, which thus kept the total number of hours spent on subsistence pursuits more or less constant for given types of environments. Because the food return per unit of work from specific resources is entirely dependent on the technology used, it is impossible to rank resources in terms of it. It simply cannot be said that cereals give a poorer return per unit of work than nuts; it all depends on the technology used. If cereals *could* be so productive, we are left with the question of why the technology necessary to exploit them was not developed much earlier.

I suggest that there was a definite, far from random order in which major types of resources came to be used over the course of the Pleistocene. The broadest determinants of which species were to be used next and how they were to be exploited were the perceptions of people as to the species that would be most worthwhile to exploit next (given resource stress) and the technological base. Although not very tangible, the perceptions of hunter-gatherers are critical for understanding the sequence in which resources were added to the human larder throughout the Pleistocene. While the period of immediate interest is the terminal Pleistocene, it will be useful to begin illustrating this proposition in terms of a very provisional four-stage scenario (table 1) for the Pleistocene as a whole. Since data are most complete for faunal remains, I will concentrate on changes in animal-exploitation behavior, but it should not be forgotten that some parallel changes in the use of plant resources were probably taking place as well, particularly in the later stages.

Stage 1. Initially, given no or extremely rudimentary technology and situations in which it would have been adaptive to improve the reliability of the resource base, increased reliability could have been achieved by diversifying the species used for food. The most obvious, easily perceived, and practical additions to the diet would have been small, young, or infirm animals that could be run down or clubbed with sticks, scavenged meat, and roots that could easily be dug up. Because groups had negligible tools and only modest capacities for hunting, such food-procurement techniques may have provided no more than moderate daily supplements, although they were probably of critical importance during times of resource stress. This stage

TABLE 1

STAGES IN THE EXPANSION OF HUMAN RESOURCES, USING FAUNAL EXAMPLES

Stage	Diversification Strategy	Period	Suggested Characteristics
1	Begin sporadic and low-return use of small animals	Pre- and early Oldowan	Capture of any animals possible without tools or with unmodified or minimally modified sticks and stones, including exploitation of trapped, infirm, new-born, and already killed animals
2	Begin effective hunting of grazing animals	Later Oldowan and Acheulian	Exploitation of medium-sized and large grazing animals with spears, stalking, tracking, and prediction of location
3	Begin effective use of all large-bodied animals	Mousterian, Upper Paleolithic, Paleo-Indian	Effective hunting of omnivores, carnivores, and hard-to-obtain animals; all large-bodied species fully exploited by end of stage
4	Begin effective use of small-bodied species	Terminal Paleolithic, Mesolithic, Archaic	Use of nets, sophisticated traps, baskets, and weirs to obtain high food returns in the exploitation of small-bodied species; many small-bodied species efficiently exploited for the first time

may be represented by the sites with numerous frog, reptile, and rodent remains in Bed I at Olduvai Gorge and perhaps by some Bed II sites with possible scavenged or juvenile faunal remains (see Isaac 1971:289).

Stage 2. Because humans require large amounts of food per day and because they are large animals that find it easiest to locate and deal with large objects, concentrations of food (in the form of large-bodied fruits, tubers, or animals) were generally perceived as the most tangible and abundant sources of food with the best returns. While the procurement of such resources may have been seen as desirable in the first stage, the available skills and tools would have been largely inadequate to the task. In Stage 1, there was little or no technology; moreover, humans would probably not have possessed the mental, emotional, or motor skills required for stalking, ambush, tracking, throwing spears, and predicting where and when game would most likely be found. However, once small and scavenged animals began to be used, continued diversification of the resource base by including the effective and regular exploitation of larger animals would have been perceived as the desirable next step. Because of this perception, together with a good cultural potential for achieving the desired end, and infrequent but repeated situations of stress, techniques were eventually found for exploiting some of the medium-sized and even large grazing animals. Such strategies were probably not efficient at first and would have taken a lengthy period to perfect, especially given some need for genetic refinements. Moreover, the species initially exploited would probably have been the less aggressive and more abundant animals at lower trophic levels, e.g., grazing animals. Most of the evidence from Middle Pleistocene sites is consistent with this stage characterization (Isaac 1971:290–91; see also Butzer 1977:578).

Stage 3. Following the development of effective hunting of many grazing animals, populations would probably have continued to view large-bodied species as the best source of food. Since there were still many species of large-bodied animals which were not regularly used because of the difficulty or danger involved in their exploitation, continued motivation to diversify the resource base during times of resource stress might be expected to have resulted in attempts to exploit these other animals. Repeated attempts, even though infrequent, would eventually have led to the development of techniques making the regular procurement of these species practical. Upper Pleistocene trends toward the hunting of more dangerous and harder-to-get animals have been noted at least in Europe and South Africa (Butzer 1977:581; Freeman 1973; Klein 1977; Bouchud 1976:688; Straus et al. 1980). These animals included suids, bears, large carnivores, birds, fish, and animals inhabiting mountainous terrain. Paleolithic stone technology would not have required drastic modification for exploiting a wider range of large-bodied animals. Reliance would still have been on spears and devices similar to those used in the past. It may have become advantageous to begin using stone-tipped spears in order to inflict greater wounds when dealing with the more aggressive species, thus dispatching them more rapidly. However, most technological changes related to subsistence would probably not have been any more drastic than this. The most important changes may have been largely behavioral, or they may have taken the form of nonlithic innovations such as the use of fire to render more species edible and increase protein/calorie absorption; the use of spear-throwers, clubs, throwing-sticks, boomerangs, and wooden barbs to increase hunting effectiveness; the use of pronged spears to capture large fish; and the use of bowls for short-term storage and transport of collected foods. By the end of this stage, hunting of many large animals had probably become so effective that they could not be exploited more intensively without significantly diminishing the animal populations.

Stage 4. With virtually all the large-bodied animals (and plants) effectively exploited, different types of resources had to

be used if groups were to minimize the effects of unavoidable short-term stress. The perceptual focus of what was worthwhile exploiting in times of stress had to change from large-bodied, easily handled and captured foods to something else. Thus, groups were repeatedly forced to reexamine their estimates of the food value of such diminutive and unobvious resources as rodents, grass seeds, snails, fish, insects, toxic plants, and shellfish in sand matrices. They were subsequently motivated to find ways of effectively using such resources. Perhaps just as important, the technological base built up over the previous stages rendered the attainment of such goals conceivable and feasible.

It had probably always been recognized that small-bodied foods were edible; however, since they were so small and difficult to obtain, they were perceived as having little food value, and preference was given to trying to exploit larger-bodied food sources. Thus they may have been used occasionally during times of extreme shortage but abandoned as soon as better times returned. Effective use of the diminutive (e.g., grass seeds), unobvious, or hard-to-get (e.g., fish and mice) resources was dependent on the efficient harvesting and processing of such foods in adequate amounts, and for this special equipment was required. The development of specialized tools is generally adaptive only as a particular use-situation becomes increasingly frequent. Thus, as people were forced to consider small-bodied species as possible food sources more and more frequently, techniques were eventually devised for obtaining some of these resources more efficiently. These technological innovations are largely the ones that prehistorians sometimes associate with the very end of the Paleolithic and Paleo-Indian traditions and more commonly view as characterizing the Mesolithic and Archaic—mortars and pestles, seed grinders, nets, basketry, ground-stone cutting implements, boiling, canoes and sleds, the use of dogs for hunting, the use of fire to increase grazing areas, etc. They represent a major change in the direction of evolution of prehistoric technology, closely reflecting the radical change in resource utilization.

The effects of this change in exploitation strategy were much more far-reaching and fundamental than the number of species involved in this "broadening" of the resource base would indicate. To understand why the use of these resources had such a dramatic impact on other aspects of culture, it is useful to view it in terms of the ecological concepts of *r*- and *K*-selected species.

Ecologists use the concepts of *r*- and *K*-selection as descriptors of certain reproductive strategies and growth patterns of populations. These strategies and patterns tend to be associated with such other species characteristics as body size, relative abundance, biological productivity, and rates of reproduction (Pianka 1970).

K-selected species—for example, most large mammals—reproduce repeatedly but tend to invest heavily in one or a few offspring at a time, which typically take a long time to mature and are long-lived. These species tend to be larger than *r*-selected species of the same orders. This reproductive pattern is assumed to be most adaptive in relatively stable environments. Relative stability depends, however, upon the tolerance of the species for environmental fluctuations and upon what parts of the environment fluctuate. Thus, although periglacial Pleistocene environments seem unstable, many *K*-selected species existed in such areas, including tundra megafauna and man himself. These large mammals have very extensive ranges. It is probable that the ability to move over vast distances and to exploit widely separated and diverse locations counteracts the effects of local fluctuations, creating in effect a much more comprehensive and therefore stable environment. Because of limited offspring and long maturation, *K*-selected species are much more vulnerable to overexploitation or even local extinction and require much longer to reestablish optimum population levels. Because of these same factors, *K*-selected species have a limited potential rate of increase. This means that they have low potential biological productivity, defined as the increase in biomass over time. These are probably the main factors which render large-bodied animals so notoriously unreliable and unstable as resources (e.g., Meiklejohn 1974).

In contrast, *r*-selected species—for example, mice, many fish, most marine shellfish, many insects, grasses—typically are small, live less than a year and reproduce only once, produce prodigious numbers of offspring (often hundreds or even thousands), are extremely difficult to eradicate by exploitation, and can be phenomenally productive biologically. They quickly reestablish themselves even after major environmental fluctuations. (The differences between *r*- and *K*-selected species are summarized in table 2.) Because of the very high potential productivity of these resources and the relative difficulty in overexploiting them, the effects of efficiently using *r*-selected species went much beyond what would be expected from a simple "broadening" of the resource base. The differences between *r*- and *K*-selected species were so great in this respect that in areas where *r*-selected resources were abundant food became seemingly inexhaustible in most years in comparison with previous stages. (Some of the more important differences in the effects of the two patterns of resource use are summarized in table 3.)

An illustration of the contrast in food potential between these

TABLE 2

CORRELATES OF *r*- AND *K*-SELECTION (AFTER PIANKA 1970)

	r-SELECTION	*K*-SELECTION
Characteristics favored by selection	Rapid development	Slower development
	High maximal rate of increase	Greater competitive ability
	Early reproduction	Delayed reproduction
	Small body size	Larger body size
	Single reproduction	Repeated reproductions
Result	Productivity	Efficiency
Length of life	Short, usually less than one year	Longer, usually more than one year
Mortality	Often catastrophic, nondirected, density-independent	More directed, density-dependent
Survivorship	High early rates of mortality	Usually constant mortality or high survivorship to adult stages
Population size	Variable in time, nonequilibrium	Fairly constant in time, equilibrium
	Usually well below carrying capacity of environment	At or near carrying capacity of environment
	Unsaturated communities or portions thereof, ecologic vacuums	Saturated communities
	Recolonization each year	No recolonization necessary
Intra- and interspecific competition	Variable, often lax	Usually keen
Environment	Variable and/or unpredictable; uncertain	Fairly constant and/or predictable; more certain

TABLE 3

RESOURCE USE BY PLEISTOCENE AND POST-PLEISTOCENE HUNTER-GATHERERS

		POST-PLEISTOCENE	
	PLEISTOCENE	Areas Rich in *r*-Selected Resources	Other Areas
Resource type	Predominantly *K*-selected species	Predominantly *r*-selected species	Some *r*- with largely *K*-selected species
Technology	Simple	Complex and specialized, including ground stone	Complex and diversified
Potential for overexploitation	High	Low	High
Effects of severe short-term environmental fluctuations on resource density	Severe and long-lasting	Limited in duration and scope	Severe and long-lasting
Strategy for increasing resource reliability	Diversification	Specialization, often accompanied by storage and exchange	Diversification
Resource supply	Limited	Extremely abundant	Limited
Settlement mode	Nomadic	Semisedentary to sedentary	Nomadic
Energy return	High from *K*-selected species, low from *r*-selected species	Vastly higher than before from *r*-selected species, slightly higher than before from *K*-selected species	Slightly higher than before from all species
Sociocultural and technological evolution	Extremely slow	Rapid	Slow
Competition over resources	Extremely maladaptive	No major adverse effects	Extremely maladaptive

types of resources is Deevey's (1968) assertion that if hunter-gatherers in temperate deciduous forests who traditionally relied on deer had switched to the efficient harvesting of mice and other small rodents, the same area could have supported a human population a hundredfold larger. This remarkable situation depends on the much greater biological productivity of the rodent population and the possibility of harvesting it at very frequent intervals without diminishing its reproductive rate or potential. This second factor would insure that *r*-selected species could realize the maximum potentials of their high productivity. Even more extreme examples of the high biological productivity of *r*-selected species have been cited by Ortíz de Montellano (1978:612), who notes the heavy reliance on insects among preconquest Mesoamerican groups: "Insects are extremely efficient food converters and produce proteins comparable to those of herbivores. The food potential represented by insects is enormous. For example, if all of the offspring produced by one cabbage aphid during one season lived, the maximum collective weight would be greater than that of the earth's entire human population." Bodenheimer (1951) gives numerous other examples involving insects.

The initial astonishment evoked by the proposition that mice can support more people than deer is mitigated by the recognition of the crucial role played by efficient harvesting. In the time spent obtaining a single deer one might be able to catch a dozen, or even several dozen, small rodents, but the rodent catch would be far inferior to the deer catch in energy return. Nevertheless, *if* it were possible to harvest as many rodents as one might like in a very short period of time, what Deevey has said would certainly be true. The viability of his proposition is determined by the development of means for efficient harvesting, processing, and storing. But how does one go about efficiently harvesting resources like mice? The answer is not at all obvious, even for the technologically advanced cultures of today. Small and medium-sized fish, grasses, and other such highly productive, diminutive resources capable of frequent mass harvesting all posed the same fundamental problem to prehistoric populations: how to develop the technology to make their exploitation worthwhile. People did not perceive the feasibility or the need of solving such problems as long as more obvious and more easily obtainable resources remained to be tapped.

Variations in social structure or ideology cannot explain the development of all or even most of the characteristics of the Mesolithic and Archaic, but many of these changes—among them increased sedentism, increased population density, reduction in band ranges, increased storage, and increased ability to keep material goods such as wealth items—can be logically linked to significant increases in resource density and reliability. I would like to draw attention to five other, less obvious changes which may be expected to have resulted from effective *r*-selected species exploitation where these resources occurred in abundance. In many respects, cultural changes had always been moving, although very slowly, toward these outcomes. With the use of *r*-selected species, a number of critical thresholds for many cultural systems would have been quickly attained.

1. Substantial increase in sedentism due to use of *r*-selected resources would have had some positive feedback effects. Because sedentary groups tend to deplete large-bodied sources of food, especially game, within a few hours' walking distance, there would have been even greater pressure to use smaller, possibly less desirable but more numerous and more productive food sources, especially for protein, e.g., terrestrial and aquatic molluscs, rodents, fish, and lizards (Meiklejohn 1974, Ross 1978, Hayden 1981). Another response to local game depletion due to sedentism might have been maintenance of very large hunting ranges and group territories. Since such ranges would have been far larger than the gathering ranges required by sedentary communities, they would have tended to insure that more than enough gathering resources would always be available to the community, thus enhancing even further the reliability of the

community-controlled resource base and increasing the relative economic independence of the community.

2. Because the increasingly complex food-extractive technology necessitated the manufacture of more wooden tools, as well as more permanent structures and facilities such as weirs, the wood-cutting requirements of groups would have increased enormously, and rejuvenation of stone-tool cutting edges by rechipping would have become wasteful and costly. The introduction of edge-grinding would have conserved material and meant fewer trips to quarries (see n. 2). Where very large amounts of meat or fish were processed, similar factors might have led to the use of knives made of ground stone.

3. The narrowing of regularly used subsistence resources, where not imposed by changing environmental constraints, must be viewed as the result of an increasingly stable resource base. This accords well with the characteristics that have been posited as stemming from the use of abundant *r*-selected species. With the efficient harvesting, processing, and storing of these resources, specialized economic adaptations would have become possible, and the principle of least effort would have made such a development inevitable where *r*-selected resources occurred in abundance. Specialization had generally not been adaptive previously because of the instability and low biomass of *K*-selected species. Moreover, because different types of specialized technological equipment were required to harvest and process different types of small-bodied resources en masse, tool assemblages would have begun to reflect more directly the specific types of *r*-selected resources being used and therefore specific regional adaptations.

4. In a very few areas where survival was largely insured by exploitation of reliable and "inexhaustible" resource bases, the need for sharing and cooperation within communities would have been reduced, and a certain amount of status competition revolving around wealth control would have been tolerated. Status ranking based on wealth would have emerged. Ranking based on subsistence and wealth had not occurred before this stage because, under conditions in which there was a serious danger of overexploiting resources (as with reliance on *K*-selected species), competition over resources would have resulted in severe undercutting of the resource base (as with modern commercial whaling and 19th-century bison hunting [Wynne-Edwards 1962]). Thus, in generalized hunter-gatherer contexts competition over resources was maladaptive and must have been systematically suppressed and/or redirected in one fashion or another—for instance, in the form of competition over women, ritual hierarchies, or age-ranking. Because resource competition was maladaptive, hunter-gatherers emphasized resource sharing both within and between groups. Partially as a result of this, periods of resource stress were relatively infrequent. In fact, strong emphasis on sharing continues to be one of the most distinctive characteristics of generalized hunter-gatherers up to the present (Hayden 1981). On the other hand, where the resource base consisted largely of small, rapidly reproducing species occurring in abundance, the risk of overexploitation was negligible, and there would no longer have been any need for repression or redirection of economically competitive behavior. As primatologists (Bernstein 1970; Carpenter 1942; Sahlins 1959:63–64) point out, dominance behavior is very widespread among Old World apes, including man. It therefore seems highly likely that at least *some* genetic tendencies toward egotism and dominance were maintained in the variability of all human populations. Such a genetic factor, together with the potential for accumulating goods due to increased sedentism and resource reliability/abundance, would have rendered the emergence of ranking and wealth competition in favored areas all but inevitable. The beginnings of such a trend occurred sporadically at the end of the Pleistocene in the

most resource-rich areas. In the Mesolithic and Archaic the trend was much more widespread and pronounced.

Communities with enough resources to support wealth competition would often have been characterized by a closed (vs. fluid or open) membership, with clans and clan cemeteries emerging. Chang (1962) in particular has drawn out these relationships in his distinction between Siberian- and Eskimo-type hunting-gathering bands, the former characteristic of resource-rich and stable areas. The matrix of feasting and primitive valuables accompanying the emergence of complex social features has been well documented (Sahlins 1968*b*, Dalton 1975) and need not be detailed here. It is worth mentioning, however, that such developments were present only among the most economically productive of contemporary hunter-gatherers, such as the Northwest Coast Indians. In at least some areas where relatively intense economic competition took place, we can envisage positive-feedback relations developing between social status systems and various material resource bases. Cowgill (1975) has pointed to the importance of economic-based competition, as opposed to population pressure, as a prime mover in cultural evolution. The fact that economically based status competition which had no major adverse repercussions on resources began only toward the end of the Pleistocene may well be the single most important factor in explaining the bewildering series of cultural developments that have occurred since, including organized warfare. And this development was largely an outcome of effective exploitation of *r*-selected resources. Thus, there have been at least two major forces behind directional cultural change—resource stress and economic competition—with the former almost completely dominating developments in the Pleistocene and the latter dominating post-Pleistocene ones.

5. Where *r*-selected resource abundance and reliability were not as extreme, cultural evolution would have continued to be governed by the infrequent occurrence of unavoidable resource stress and as a result would have proceeded at the same slow rate that had characterized previous millennia. Sedentism, wealth control, and status differences would not have been nearly as developed in these areas, either. In many cases, the group concerned would have become much less dependent on neighboring bands for assistance in times of scarcity; the wide-ranging economic-subsistence alliances and networks (Harris 1971:295–305; Yengoyan 1976) common to previous stages would have become less important and would generally have been reduced in size. Increased ethnic differentiation would be expected under these conditions. For example, Cleland (1966:93) has argued that the maintenance of widespread interaction in the Upper Great Lakes region was closely tied to relative economic instability and that subsistence stability was directly responsible for lowered rates of interaction and increased cultural fractionation. Alternative explanations of stylistic regionalization include Isaac's (1972) and Wobst's (1976) suggestion that regionalization may have developed in response to increased population density. However, there are several strong reasons for believing that population density has only a minor effect on cultural homogeneity over large areas (Hassan 1978:79; Hayden 1980*a*).

The general relationships of some of these variables are shown in figure 2.

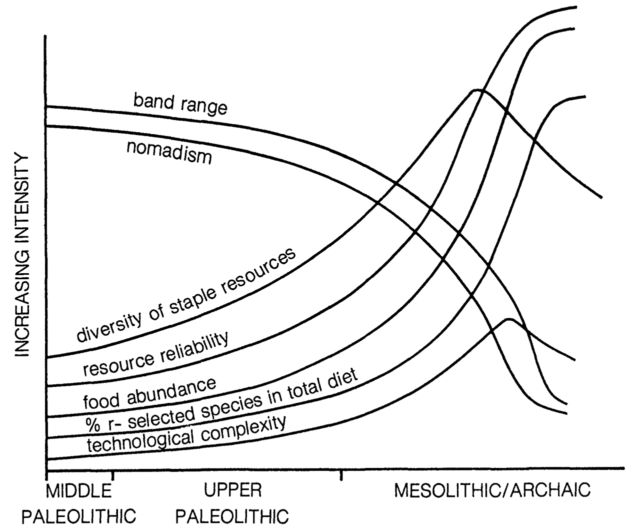

FIG. 2. Proposed general relationships between some of the variables relevant to the resource-stress model. Population density should follow the same general curve as "% *r*-selected species in total diet."

SUMMARY AND CONCLUSIONS

I have argued that episodes of resource stress occurred within the same range of frequency throughout the Pleistocene and that such events were the driving force behind attempts to increase resource reliability. Resource reliability was increased during the Oldowan and Early Acheulian by diversifying the resource base and exploiting herd animals. This was probably a long process, since changing from herbivores to effective predators must have involved considerable genetic change. Subsequently, in the later Acheulian to Upper Paleolithic periods, resource reliability was increased by further diversifying the resource base and including more dangerous and harder-to-get large species. The susceptibility to overexploitation of these species insured that there was a minimum of competition involving resources and of status differentiation based on control of resources. By the end of the Pleistocene, all these species were being used as effectively as possible. Continued recurrence of resource stress meant that increased resource reliability could only be achieved by further diversifying the species being exploited, and this meant using small, unobvious species such as grass seeds, fish, and mice. Because these resources were comparatively inexhaustible and highly productive, wealth/status ranking based on competition developed in areas where they were abundant and near-sedentary communities were established. Since differing major technological innovations were required to exploit each such resource effectively, regional technological specialization developed.

Predictions from this model are to a large extent very similar to the predictions derived from other models, but there are important differences. Nutritional stress is expected to remain more or less constant throughout the Pleistocene or, if anything, to decrease; in contrast, the population-growth model predicts an increase in nutritional stress over time and an especially dramatic increase in stress levels at the end of the Pleistocene. To the extent that the average life-span is a function of the frequency and magnitude of nutritional stress, a limited test of these different predictions is possible. Average age at death for the Paleolithic and Mesolithic can be calculated from data presented by Acsádi and Nemeskéri (1970:138, 148). Although such data may not be ideal in terms of sample size and representativeness, they tend to support the expectations of the present model. Average age at death in the Lower Paleolithic was 14.8, in the Middle Paleolithic 22.1, in the Eurasian Upper Paleolithic 20.6, and in the Mesolithic 20.8. It is also expectable that places and times of rapid major climatic oscillations will have more frequent episodes of unavoidable resource stress than places and times of environment stability. Because innovation is viewed as a probabilistic event dependent on episodes of disequilibrium, it should be most frequent where resource stress is most frequent. Thus, peripheral populations and populations of the terminal Pleistocene should have been highly innovative in exploring ways to increase resource reliability. The well-known inventiveness of the Inuit would be consistent with this proposal. It should be emphasized, however, that this hypothesis applies to the *relative* rates, not the presence or absence, of innovation.

The model I have presented accounts for aspects of the prehistoric record previously not accounted for—why the Pleistocene trend toward diversification of staples should have been reversed in some areas during the Mesolithic and Archaic; why technological specialization occurred in some areas but not others; why regionalization became more pronounced toward the end of the Pleistocene; why the exploitation of new resources followed a particular sequence; why wealth competition and primitive valuables began to appear at the end of the Pleistocene; and why there were differential rates of cultural evolution among generalized hunter-gatherers and communities with more stable and abundant resources. Because many of the trends and relationships postulated here are based on limited data, their confirmation by future research will provide a further partial test of the model.

The model also sheds some light on the broad simultaneity of major cultural developments worldwide by suggesting that basic adaptive innovations such as fire, the atlatl, boiling, and seed grinding probably spread rapidly because they increased resource reliability and reduced energy expended in nomadic travel—witness the rapidity with which the bow and arrow spread over large areas of the world (Chard 1975:155; Klein 1977:122) and Mesolithic technology spread throughout the Middle East (Flannery 1973:236). Sedentism and domestication probably emerged independently from similar, roughly contemporaneous technological bases created by rapidly diffusing fundamental adaptations including boiling, grinding, the manufacture of substantial baskets and/or weirs, and fishing equipment. Stylistic attributes, which had no clear technological adaptive value, would not necessarily have diffused along with these basic innovations.

The basic scenario I have presented is a refinement of a traditional theme, a variant of which has also been used to explain the biological evolution of man (Butzer 1977:580, 585). Yet an entire school of prehistorians would have us believe that neither "population pressure" nor stress existed—or at least that they were not significant for cultural evolution—for over 2,000,000 years. They argue that, for one reason or another, real "population pressure" only came to exist at the end of the Pleistocene. I maintain that this is logically untenable and does not accord well with the data at hand. Most important, it follows from my arguments that the concepts of "population pressure" and "carrying capacity" are sterile, black-and-white ways of looking at problems of cultural evolution. According to their proponents, the only important factor relevant to cultural evolution is whether a population is above carrying capacity or not, whether there is population pressure or not. This is a far too simplistic way of viewing cultural and demographic dynamics, and the resultant models work poorly. Moreover, such concepts are hopelessly nonoperational. In place of these dichotomous concepts, I suggest that no population ever was, or is, completely free of resource stress and that the variables it would be most useful to study are the frequency and the intensity of stress. Our primary concerns should be the causes of variations in these values and their effects. Stress frequencies and intensities can be measured (e.g., Skinner 1980) and are much more directly related to situations in which change would be adaptive than the very oblique and nonquantifiable concepts characteristic of the population-pressure and carrying-capacity approach.

For every model, there remain exceptions and relevant observations not adequately accommodated. Though I believe the model I have presented to be a theoretical advance, there are unresolved questions. Why, for example, were shellfish not exploited earlier and more extensively? (Here I refer specifically to the relatively large species attached to rocks exposed at low tides, such as large mussels and oysters; it is easier to explain why invertebrates such as the larger clams, living at some depth

in the sand and more difficult to obtain with digging-sticks, might not be exploited until quite late.) This, however, is not a problem peculiar to this model; it applies to *any* of the current theories pertaining to Paleolithic subsistence. A similar problem exists concerning land snails, where they were relatively large and abundant. The ultimate explanation may be relatively complex, involving factors such as boiling technology, energy returns for effort expended, and degree of sedentism and their relationship to degree of depletion of large game or other protein resources within regular foraging distance. Beyond this, there is the problem presented by the apparently sedentary, dense, wealthy, and ranked societies of Upper Paleolithic southwestern and eastern Europe, apparently based to a substantial degree on the exploitation of *K*-selected animals such as mammoth and reindeer. Although the characterization of these societies is not accepted by all prehistorians (see, e.g., Meiklejohn 1974), if it should prove accurate the model does not allow for their occurrence. I can only suggest that, if these groups were in fact relatively sedentary and wealth-ranked, they depended to a much larger extent than is generally recognized on *r*-selected resources, such as salmon or other fish, the faunal remains of which may well have been discarded without a trace in streams (see Desse and Desse 1976:700). I think that good arguments can be made that Magdalenian "harpoons" were used for fishing, and it may well be that other components of fishing technology will be recognized in Magdalenian assemblages. Recent excavations at Gare de Couze and Reignac have revealed that fish are present in abundance at some Magdalenian sites (Desse and Desse 1976:698; Delpech 1975; White n.d.) and rabbits in others (Célérier 1976, Tixier 1976).

Having taken the model of Pleistocene cultural change this far, it is worth asking whether it has any implications for subsequent cultural evolution. In particular, I wish to explore the possibility that horticulture merely represents the natural and logical extension of the trends which led to the Mesolithic and Archaic. In this respect, it would be a fifth stage of development, following the sequence presented earlier. I will first suggest that domestication resulted from attempts to increase resource reliability by increasing resource diversity in areas that were not well endowed with *r*-selected resources but in which some use and manipulation of such resources was possible. In this sense domestication was simply another technological innovation like stone boiling, seed grinding, and the use of nets. It is clear from ethnographic data that the number of resource strategies *available* continued to increase through horticultural phases, even where economies were based on a few staples. This is particularly true of early forms of horticulture, which used large amounts of wild staples (Hole, Flannery, and Neeley 1969:343; Helbaek 1969: 389–90; Ford 1977:174). Similarly, economically specialized Archaic bands probably maintained a wide array of resource strategies which were used infrequently, usually in response to the brief fluctuations characteristic of *r*-selected species. In fact, it was probably the knowledge of hunter-gatherer resource strategies as alternatives that made horticulture a viable proposition. During famines in Mesoamerica, such groups as the Yucatec Maya are documented as having lived by foraging and gathering wild foods (Tozzer 1941: 190–203; Ryder 1977:207; Spores 1965:967, 976, 984), while hunting and fishing continued to be important throughout the New World. In support of this general argument, it can be noted that the number of species recognized as edible is typically much greater for horticulturalists than for any contemporary hunter-gatherers in comparable environments. The maximum for tropical hunter-gatherers appears to be about 250 edible species of plants and animals, as opposed to 1,000 useful plant species and 450 animal types for tropical horticulturalists (Conklin 1969:229–30; Hayden 1981). Similarly, in the temperate Mayan highlands, where there is much less species diversity, 240 indigenous types of plants alone were used, many of which combined several species under a single lexeme (Berlin, Breedlove, and Raven 1974:98–103). Colson (1979:22) also has argued that horticulturalists subject to stress have a much better knowledge of wild foods than hunter-gatherers. Thus, in areas in which *r*-selected species were not abundant and subsistence specialization did not occur, the diversity of both staples and all usable species increased from Paleolithic through horticultural times (fig. 3).

With the advent of large-scale economic competition, intensive agriculture was established and forests were destroyed in order to support as large work forces and armies as possible. These developments, together with massive trade, resulted in a genuine widespread reversal of the Paleolithic and early horticultural strategies, leading to a reduction in *local* resource variability in terms of both staples and all species used. In time, competition led to the monocropping characteristic of contemporary agriculture. What is important here is that Early Holocene hunter-gatherers everywhere followed the long-term strategy of increasing resource reliability, whether by tapping small, highly productive *r*-selected resources, by beginning to plant crops, or, where these strategies were not possible, by continuing to diversify their use of wild resources whenever the opportunity presented itself.

It cannot be coincidental that the first domesticates began appearing almost contemporaneously with the first appearance of the Mesolithic and Archaic. Moreover, this is precisely what the present model leads us to expect.

It is especially notable that domestication did not first occur where the environments were rich enough to support sedentary, hunting-gathering-based ranked societies, with wealth competition and primitive valuables, such as those found in California, the Northwest Coast, Florida, and Palestine. According to my model, even though these areas had the highest population densities, their resource bases would have been much more stable and they would have experienced resource stress relatively infrequently. Because sedentism, wealth competition, and ranking in rich environments did ***not*** result in domestication in these areas, it is reasonable to conclude that such developments were not sufficient, or perhaps even necessary, conditions for domestication. Instead, domestication can be more usefully linked to the same Paleolithic processes which gave rise to the Mesolithic and Archaic: the effort to increase resource reliability in areas of frequent stress. A number of recent views on the effects of domestication have emphasized this aspect. Cleland (1976:60) considers the constant evolution of adaptive systems to be motivated by the search for economic security. Ford (1977), cited with approval by Judge (1978:292), argues that domesticates did not revolutionize Archaic subsistence, but instead acted as regulatory devices which helped stabilize the system. Singleton (1979:14) asserts that domestication in the Near East "is an integral part of the process of intensive, local exploitation and is not a consequence of it." Assuming that motivation to stabilize resources was pervasive prehistorically, one is hardly surprised to find stability a dominant concern among contemporary primitive groups (e.g., Colson 1979:22–24; Reina and Hill 1978:250–51).

If all this is so, it may be asked why domestication did not occur in Australia, which has wide environmental diversity and many marginal habitats. My answer is similar to the response to the question of why the New World did not discover the Old World: given more time, it would have. The Australians followed the same basic sequence of technological development as occurred in other areas of the world (see, e.g., Lourandos 1980*a*), including the late incorporation into their diets of *r*-selected resources such as seeds, fish, shellfish, and lizards, the development of sedentism, increasing diversification of the resource base, the use of edge-ground tools, and the advent of primitive valuables. There is absolutely no reason that this evolutionary sequence would not have continued in the same fashion as elsewhere in the world. Indeed, Lourandos (1976, 1980*a*) has recently drawn attention to the fact that many Australian groups *were*

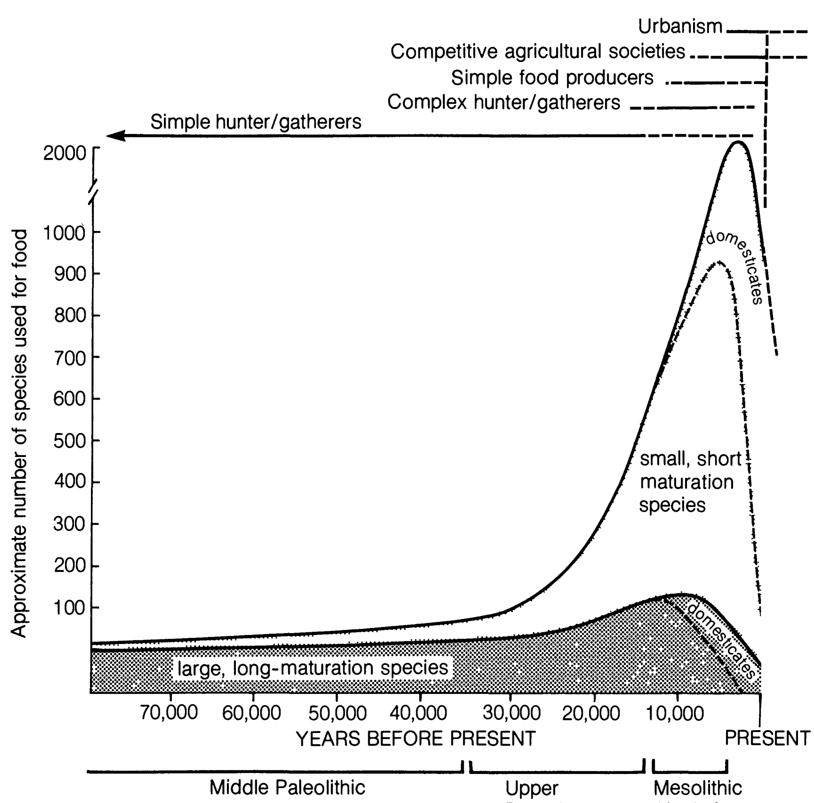

FIG. 3. Idealized representation of changes in maximum number of species locally exploited in the past. The absolute numbers used on the ordinate are only rough estimations.

engaged in various forms of food production, as this model leads us to expect.

The process of increasing resource reliability is still going on (Gall and Saxe 1977, Isbell and Schreiber 1978). Other means, such as participation of communities in regional, national, and worldwide trade networks, have been found, but the strategy adopted at the outset—technological innovation—continues to be heavily relied upon. The only difference is that the technological innovation of contemporary times occurs on a much larger and more complex scale than that of the Paleolithic. We are now dealing with weather control, highly sophisticated irrigation and dam systems, sophisticated genetic manipulation resulting in miracle crops, massive control of pests via chemicals, massive chemical fertilization, artificial insemination, environmentally controlled breeding and living conditions, artificially controlled hormone levels, and artificially controlled lighting conditions. To be sure, many of these practices also act to increase gross food production—a concern rendered important by postdomestication economic competition—but they also fulfill the goal of increasing resource reliability. It is apparent that there is no reason to assume that these trends (including post-Pleistocene economic competition) that have been driving forces in cultural evolution for so long are about to abate. They provide some of the firmest foundations we have for making predictions about the future evolution of culture.

Comments

by Sandra Bowdler

Department of Anthropology, University of Sydney, Sydney, N.S.W. 2000, Australia. 6 iii 81

Hayden's model is attractive in the way it combines the observed directional development of technology during the Quaternary with environmental-stress notions while avoiding many of the logical problems associated with both these sorts of notions. I am not sure, however, that it is in agreement with the evidence.

It comes as something of a surprise to find Hayden espousing the old chestnut that shellfish and fish only began to be exploited in Terminal Pleistocene times. The proliferation of shell-midden sites in the period when the sea reached its present level is more than coincidence; the most economical explanation is that older such sites either lie beneath the sea or have been obliterated by wave and storm action (Bowdler 1977, Hughes 1977). There are in fact Middle Palaeolithic cave sites, such as Haua Fteah (McBurney 1967) and Devil's Tower (Garrod et al. 1928), which contain substantial midden accumulations. Another cause for concern is Hayden's failure to take into account archaeologically invisible plant foods, with the exception of grass seeds, as the exploitation of these is signalled by grindstones.

In North America, the Paleo-Indian culture type was until recently known mainly from big-game kill-sites, in sharp contrast to later Archaic sites. It has been suggested, however, that such kill-sites may represent only one aspect of Paleo-Indian economy. The recent excavations at Meadowcroft tell a rather different story. Whether the basal occupation here is of the extreme antiquity claimed (Adovasio et al. 1978, Mead 1980, Haynes 1980, Adovasio et al. 1980) or somewhat younger, it appears to contain pre-Archaic materials. As far as one can tell from the preliminary reports, there is no great difference between the plant and animal species exploited in the lowest levels and those in the Archaic levels. These include deer, a variety of smaller game, birds, fish, aquatic and terrestrial molluscs, hackberries, chenopods, nuts, and other berries (Adovasio et al. 1978:647–49).

Hayden's comments on Australia provide the biggest surprise of all. We find that prehistoric Aborigines "followed the same

basic sequence of technological development as occurred in other areas of the world . . . including the late incorporation into their diets of *r*-selected resources such as seeds, fish, shellfish, and lizards, the development of sedentism, increasing diversification of the resource base, the use of edge-ground tools, and the advent of primitive valuables." This is not so. Australia was colonised by human beings at least 40,000 years ago (e.g., Mulvaney 1975). The oldest known sites with dietary evidence are at Lake Mungo, and here we find occupation of inland lakes dating to around 30,000–25,000 years ago, where people ate fish, shellfish, and small mammals in exactly the same relative amounts as they did in coastal sites a couple of hundred years ago (Bowdler 1977). Ground-edge tools have been dated to over 20,000 B.P. in northern Australia. Grindstones have been dated to about 18,000 B.P. in northern Australia and to 15,000 B.P. in semiarid southeastern Australia, where they were undoubtedly used in grass-seed exploitation (Bowdler 1977, Mulvaney 1975). Other major observable technological changes in mainland Australian prehistory did not take place until after 5,000 B.P., and in this late period the dog was introduced.

A slightly different model can be envisaged, using some of Hayden's useful ideas. This is prompted in part by his highlighting of the important role played by "resource reliability" in people's economic strategies. Meehan (1977) provides first-hand ethnographic data on the role of shellfish in the diet of one group of northern Australian Aborigines. She shows that while shellfish are not of great caloric significance in the diet, they are important because they are reliable. She sees the overall foraging strategy as having two components: (1) opportunistic activities involving luck, skill, and strength, such as hunting and some fishing, and (2) "low-key" pursuits such as shellfish and vegetable collecting and some fishing. The latter category is seen as providing dependable food, the former as being "more flamboyant and less reliable" (Meehan 1977:527). I have extended this argument to suggest that all hunter-gatherer diets may have (had?) this dual-component strategy (Bowdler 1981). It can be noted that there is agreement here with the usual division of labour; women's foraging tends to be low-key and dependable, men's to be more opportunistic and flamboyant. It can be further suggested that women tend to collect *r*-selected species, men to hunt *K*-selected species. Clearly, I cannot develop this argument here, but it does point to an interesting restatement of Hayden's model.

by Karl W. Butzer

Swiss Federal Institute of Technology, Sonneggstrasse 5, 8092-Zurich, Switzerland. 18 iv 81

This is a stimulating and important paper, although I do not agree with some of the assumptions or arguments. In particular, Hayden tends to polarize Paleolithic and Mesolithic lifeways to a degree that is applicable only in parts of northwestern Europe. I am also uncomfortable with the resource-stress explanation: in general terms, such stress has been part and parcel of year-to-year survival for most individuals of many populations at almost all times; in specific terms, accentuated resource stress cannot be demonstrated or plausibly argued for the Paleolithic/Mesolithic or Paleo-Indian/Archaic transition on objective criteria. On the other hand, I do believe that resource stress can be a critical factor in the modification of regional adaptive systems by behavioral or technological accommodations, but over much briefer time intervals than those invoked here (Butzer 1980). There is also considerable merit in Harris's (1977) model emphasizing the role of increased sedentism in a complex feedback system, while I would like to give greater importance to ongoing biotic changes that provided new or quantitatively different animal and plant communities at the close of the Pleistocene. Hayden's paper is particularly valuable because it explicitly highlights the nature of the end-Pleistocene adaptive transformation in terms of increasing emphasis on *r*-selected species, but I feel that it provides only a partial explanation as to why this transformation took place.

by Mark N. Cohen
Department of Anthropology, State University of New York at Plattsburgh, Plattsburgh, N.Y. 12901, U.S.A. 21 iii 81

I enjoyed Hayden's essay but found it a refinement and extension of the "population-pressure" model rather than a refutation. Two points must be made about population growth and regulation. First, there *is* reason to assume that population pressure is "simply there." Animal and human populations regularly demonstrate the capacity to reproduce in excess of replacement. Moreover, among mammal populations in the wild (Cohen, Malpass, and Klein 1980, esp. Tamarin 1980, Packard and Mech 1980) much population "regulation" takes the form of dispersal of members or subgroups into marginal or temporary niches where, ultimately, they die for lack of dietary and behavioral plasticity. Population "stability" is bought at the expense of constant probing of niche margins. The same phenomenon, moderated on the one hand by some cultural regulation of births and on the other hand by the plasticity necessary to exploit niche margins, would explain the human pattern of expansion and "pressure." Second, "population pressure" in the Pleistocene need not imply the absence of cultural or biological regulators of fertility; it only implies that the regulators are imperfect. Any long-term approximation of zero growth over a broad area must adjust not only for local demographic variables and accidents but also for periodic failure of other groups. Given this fact, the assertion by Weiss (1978) cited by Hayden that zero growth is more plausible than unspecified near-zero growth (representing the average of a number of different, local, zero and nonzero values) is untenable. *Imperfect* population regulation seems especially likely if, as Hayden asserts (and I believe him), (1) population regulation is costly and needs to be constantly reinforced and (2) regulation is related to periodic stress rather than to a fixed "carrying capacity."

Hayden proposes a useful model for the interaction of population growth and technological advance: populations culturally regulate themselves in the face of fluctuating resources to maintain a balance between the costs of regulation and the costs of periodic shortages, but technological advances result in progressive expansion of the food base whose purpose and result is to reduce the danger of such fluctuations. It seems to me that in technological expansion, as in population regulation, costs and benefits must be balanced. A group will expand its resource base to buffer itself against severe and frequent shortages; but when such shortages become less frequent or less severe as a consequence, and when expansion is perceived as involving additional effort or reduction of cultural standards, efforts at expansion will diminish. A group will not simply go on perfecting its homeostasis as the possibility of serious shortfalls gets more remote.

What will happen, I believe, is this: Populations will expand their resource base to buffer against serious crises, but as such shortages are dealt with successfully there will be compensatory tendencies (1) to reduce efforts at economic expansion and (2) to relax enforcement of cultural mechanisms of population regulation. Population will again grow to the point at which periodic fluctuations (or chronically diminishing returns) of the new resource base again impinge, and the cycle will begin again. The result is an alternation of "pressure" and "progress" motivated by the fact that population will tend to creep up to the level where stress is felt.

The model helps explain the difference in growth rates between Old and New World populations. The rate of growth is limited by the success of technological advance; but a small population entering an open environment with a technology already well "advanced" will grow very rapidly to the point where it "pushes" at the same level as populations and technologies of the area and culture left behind.

I would suggest also that Mesolithic/Archaic economies reflect primarily "pressure," not "progress." *r*-selected species are abundant, but they are costly to exploit; are generally not preferred foods; and are, by definition, relatively vulnerable to environmental fluctuations. If, as I believe, Mesolithic populations were increasingly forced to rely on these resources because of growing population and increasing spatial limitations, they should show signs of increasing biological stress. The data are scattered and not entirely consistent, but there is a fair amount of evidence for a decline in the quality of life among Mesolithic and more recent hunter-gatherers; and there is considerable evidence that agriculture itself initially involved a serious and progressive increase in stress (Cohen 1980, Lewin 1981). The changes in wealth and social organization which begin in the Mesolithic are probably more an adjustment to the vulnerability of the new economies than to their productivity.

by Mark Druss
Department of Anthropology, Idaho State University, Box 8215, Pocatello, Idaho 83209, U.S.A. 27 iv 81

Hayden has written a stimulating article which will engender useful debate. His innovative model attempts to explain the crucial Paleolithic/Paleo-Indian to Mesolithic/Archaic transition. Models are of necessity simplifications of complex situations (Clarke 1968:32), and Hayden's is no exception. However, in some cases the issues are oversimplified.

For example, problems arise with Hayden's several points of criticism of the "bottle model" of population growth. Hayden mentions that contemporary hunter-gatherers have maintained stable populations over long periods of time by practicing population control. However, the ethnographic present is not a long period when compared with the Pleistocene and Holocene. Furthermore, contemporary hunter-gatherers are living in marginal environments, and therefore demographic extrapolations from such populations to Pleistocene hunter-gatherers should be used with caution. It is possible that hunter-gatherers living in prime environments could, over a long period of time, have experienced steady population increase as Cohen (1977:16) suggests. Concerning Hayden's fifth point of criticism of the bottle model, it is clear that periodic resource stress occurred as he suggests, but, while famine conditions may have been created by adverse environmental change, Harris (1977:192) has pointed out that hunter-gatherers can respond to such reverses by out-migration. For example, such out-migration may well have occurred in northern Chile during the Final Preceramic period (Druss 1980). It is also possible that Terminal Pleistocene environmental changes were even more severe than those of previous periods of deglaciation and thus posed unprecedented problems for plant and animal populations (Guilday 1967:121) and presumably for the hunter-gatherers exploiting them. Further problems arise with the central premise of Hayden's argument.

Hayden's argument is based on the "generally accepted characterization of Pleistocene (Paleolithic and Paleo-Indian) adaptation." However, a revision of this characterization of Pleistocene adaptations as relatively lacking in diversification and specialization has been under way for quite some time. For example, seed-grinding implements, generally considered the cornerstone of Mesolithic/Archaic adaptation, are judged characteristic of Middle Paleolithic culture by Carter (1978: 11). At Levi Rockshelter, Alexander (1978:21) reports grinding stones in association with Plainview points dating from 9,311–7,300 b.p., and small-animal bones at Levi suggest that Paleo-Indian subsistence was significantly diversified and not so different from Archaic subsistence (Flannery 1966:800). Also, the apparent increase in Mesolithic/Archaic shellfish gathering can be questioned. Clearly, many coastal Paleo-Indian sites are probably now underwater on the Atlantic coastal plain, having been inundated by eustatic sea-level rise (Emery and Edwards 1966). Thus we do not know whether coastal Paleo-Indians were, in fact, gathering shellfish (cf. Salwen 1967 for the Archaic). Finally, much of the Pleistocene occupation in such areas as the Yuha Basin is still lying deeply buried, awaiting chance exposure and discovery (cf. Childers and Minshall 1980). Thus it appears that we do not have a representative sample of Pleistocene sites and that some of the sites known suggest that the "generally accepted characterization" of the Pleistocene/post-Pleistocene transition is overdrawn. To the extent that this is true, Hayden's argument is weakened: if significant specialization existed during the Pleistocene, then we cannot rely on it so heavily to explain post-Pleistocene culture change. Clearly, *further* specialization and diversification did take place after the Pleistocene, and there must be a reason for it.

In this regard, Hayden is correct that resource stress and resource reliability are critical. However, these can be easily subsumed under the population-growth model: resource stress is difficult or impossible to manage when resource reliability fails and/or when population growth has exceeded optimal densities for given cultural-ecological conditions. Under such circumstances, cultural innovation, out-migration, and intensification of subsistence strategies are all well-known results. Hayden has reminded us of this in his cogent presentation of his model. However, without population pressure of some sort, the inherent flexibility of hunter-gatherer groups documented by Hayden would seem to be sufficient to counteract resource stress. Thus the population-growth model is complemented rather than replaced by Hayden's argument.

by ROBERT C. DUNNELL
Department of Anthropology, University of Washington, Seattle, Wash. 98195, U.S.A. 9 IV 81

Hayden's paper is a valuable and, in many ways, an exciting contribution to our understanding of the gross temporal patterns evident in the archaeological record. His systematic collection of arguments against population growth as the "motor" of cultural change is valuable in itself. The general description of the order in which resources are added to subsistence systems conforms closely to the so-called economic approaches taken by, for example, Christenson (1980) and Earle (1980) and is a good account of the evidence. Similarly, his most important point, that increasing resource diversity, evident throughout the Pleistocene, is reversible under stable environmental conditions, is in good accord with the record and certainly the most parsimonious account yet advanced.

Hayden's model, however, is a relatively crude first approximation that can be articulated with the archaeological record and biological theory only in a general fashion. Two features, subtle but pervasive, seem to be the major stumbling blocks. Throughout the paper and in common with most archaeological and anthropological writing on such topics, he assumes that cultural change is fundamentally Lamarckian (e.g., Gould 1979) by seeking cause in some ill-defined conscious human action. Phrases like "populations attempted," "the most important motive," and "resource stress experienced by a group was always a tradeoff between *the cost of physical suffering* . . . and *the cost of maintaining population controls*" belie an assumption that the "motor" of cultural change is in some way human perceptions and reactions to them. Cultural change is thus a consequence of human motivation, desire, and perceptions; cause is mystical, much as it is in the population-growth models. Yet clearly none of Hayden's arguments require such a "motor." The principal reason for employing this kind of argument seems to stem from his assertion that cultural *ecology*, rather than evolution, is the proper framework for studying *temporal* patterns. Thus he is compelled to focus attention on adaptation in a context in which cause is less than clear, even in the parent discipline. What brings about adaptations? Hayden's answer is apparently "people" rather than selection, and thus he is at fundamental variance with both ecological and evolutionary theory (e.g., Coombs 1980, Dunnell 1980, Rindos 1980). Yet his admirable insights into the general patterns are conformable with a selectionist explanation. The operation of selection alone is sufficient to produce the documented patterns, irrespective of human motivation or perception. Populations do reach equilibrium levels, whether they *want* to or not. Resources will be added to systems in the order of increasing initial costs, whether or not the populations *are aware* of those costs or *can measure* them. His commitment to this approach leads him to use a series of tertiary empirical generalizations (e.g., "least cost") which are clearly subject to challenge in a behavioral context but readily understood as the frequent outcome of the operation of evolutionary mechanisms.

His discussion of *r*- and *K*- reproductive strategies is also highly useful but similarly marred by imprecision. These terms are relative (e.g., no mammalian strategy is *r*-selected when compared with smallpox). Classically *r*-selected populations, while abundant when they occur, are likely to have highly irregular temporal/spatial distributions. Yet there is a substantial grain of truth in his argument. Resources that employ *r*- reproductive strategies relative to *Homo sapiens* are probably the only resources that can be the focus of specialized subsistence systems (Cleland 1966, Dunnell 1972, Segraves 1974). The reverse, however, is not true. Generalized subsistence systems can make use of any resource within their technological competence, irrespective of the reproductive strategy of the resource itself or its patterns of abundance. Indeed, that would appear to be the major long-term advantage of such systems.

Hayden's paper is extremely valuable. His conclusions are unnecessary weakened, however, by his retention of a rather traditional perspective on cause that is neither essential to his argument nor compatible with ecological and evolutionary theory.

by ALBERT C. GOODYEAR
Institute of Archaeology and Anthropology, University of South Carolina, Columbia, S.C. 29208, U.S.A. 6 IV 81

The worldwide transition from Paleolithic to Mesolithic or Archaic adaptations at the end of the Pleistocene in Old and New Worlds remains one of the most intriguing series of events recorded in prehistory. Hayden joins a list of distinguished scholars (e.g., Caldwell 1958) who have attempted to identify causal factors underlying this important trend in cultural evolution. In spite, however, of what he perceives to be the novelty of his thesis and the attempt to provide comprehensive, wide-ranging examples to support his model, I find it implausible and to some extent contradictory.

The primary thesis of the model, "populations attempted to maintain a certain equilibrium concerning the frequency with which they experienced stress and altered their behavior in response to deviations from this equilibrium," is so general and characteristic of all living systems that it has little deductive value. Hayden posits further that "this periodic stress was the most important motive for patterned, directional change—i.e., toward minimizing the immediate effects of resource shortage and instability." Again, what living system is exempt from stressful perturbations in a variable world? Granting that stresses of various types are sources of motivation for change in the past (and the present), the challenge for theory building is to identify the character of the stress as related to various modes of organizational change. In an effort to play down the role of an obviously increased population level, incredibly, he asserts that man:land relationships were not significantly different in Oldowan and Neolithic times. From this I must conclude that differences in demography and biocultural systems over a million or more years played little or no role in human evolution. If this is true, there is no need for paleoanthropology.

The definition he provides for "resource stress" is too narrow to have predictive value. Fortunately, primitive groups are intelligent enough to foresee immediate and probably to some extent long-term resource shortages. This allows them to make alternative food choices and locational changes long before disease and death arrive. I suspect that much of the culture change observable in the archeological record relates to groups altering their demographic, social, and economic situation in ways less costly than allowing direct threats to health. I agree that all systems, including those of Paleolithic hunter-gatherers, undergo stress, but I question just how dramatic the impact ("famine") was on Late Pleistocene populations. Modern hunter-gatherers living in what are traditionally considered marginal environments, such as the desert dwellers in Africa and Australia, seem well buffered by back-up food-getting strategies (Lee and DeVore 1968, Lee 1980). These alternatives, including part-time horticulture, usually require more work and are thus avoided as much as possible.

Hayden is unimpressed with population-growth arguments because it is not clear to him why population grew slowly over a period of a million or more years, culminating with a surge at the end of the Pleistocene. *Why* populations grow under such widely varying conditions as foraging and agriculture can be separated analytically from the obvious fact that population *did grow* tremendously throughout the world during the last 50,000 years. It is not reasonable to compare the niche-expanding capabilities (culture) of Early and Middle Pleistocene hominids with those of *H. sapiens* of the Late Pleistocene, the latter equipped as they were with modern intelligence and technologies that reflect analytical capacity. Given these abilities, Late Pleistocene peoples rapidly colonized practically every major region of the earth. Hayden has underestimated man's ability to migrate and expand by budding off at what had to have been a rapid rate (cf. Martin 1973). Why this did not happen earlier in the Pleistocene, with much simpler biocultural groups, should be apparent.

While explanations based on simple statements such as "population grew" and "the climate changed" are not really explanations, the interactions of climatically induced resource changes relative to population densities are not irrelevant. The end of the Pleistocene saw more people occupying more landscapes than in any previous period, specializing to a significant extent in large-mammal hunting. The onset of the Holocene meant the rapid loss of many of these key species, forcing groups to select smaller, less cost-effective resources. To this must be added the stress created by the relatively rapid loss throughout the world of a few million square miles of terrestrial environment because of sea-level rise, which would have displaced groups and permanently removed many habitats from exploitation (see Perlman 1980:295–96).

In general, Hayden has vastly underestimated the total cost of employing the technologies that "allowed" man to become sedentary. The energy required to make and use technologies such as grinding implements, while perhaps substantial, was rather small compared to the time and energy cost of procuring the seed resources processed with these tools. Conversely, he has overestimated the benefits of decreased mobility and sedentism. To argue that, by Zipf's principle of least effort, people should naturally choose sedentism over mobility strategies is to be unfamiliar with the hunter-gatherer literature. Where population densities are relatively low and property portable and light, mobility is a highly cost-effective means of gaining subsistence security (see Sahlins 1972, Binford 1980), but when population densities go up mobility options go down (cf. Ford 1979:236). He has also misinterpreted the work of Lee (1969:60) when he refers to the "undesirable work load resulting from nomadism." What the !Kung dislike are the increasingly longer and more inconvenient trips that must be made from a residential camp the *longer they stay at that site* (Lee 1969:60–61). Yellen (1977:64) is even more explicit in his description of the !Kung's desire to minimize the round-trip distance of operating from one camp too long. The costs of moving the residence group to a new locality are comparatively small in relation to the benefits gained by access to new and fresh resources.

by DONALD L. HARDESTY
Department of Anthropology, University of Nevada, Reno, Nev. 89557, U.S.A. 17 IV 81

I find Hayden's model appealing. There is an additional test implication: stylistic diversification. The logic is as follows: As environmental stability is improved by behavioral mechanisms, each group demands less resource diversity to cope with unexpected fluctuations—a point made by Hayden. As a consequence, the resource "pie" can be partitioned into smaller shares; in the parlance of ecological theory, niches can be more tightly packed. But the principle of competitive exclusion implies that cultural "styles" should emerge in each niche to minimize competition. Thus, one would expect stylistic diversification to accompany increased environmental stability. That archaeological data fit this test implication is obvious.

by FEKRI A. HASSAN
Department of Anthropology, Washington State University, Pullman, Wash. 99164, U.S.A. 1 III 81

Hayden has provided a concise and clear ecological explanation of the worldwide economic changes during the Terminal Pleistocene prior to the emergence of agriculture. His central thesis emphasizes the shift toward a diversified resource base as a means of stabilizing the relation between population and food resources. I am in general agreement with this thesis, which I expressed in 1977 in my formulation of a theoretical model of agricultural origins in Palestine and again in 1978 in my review of demographic archaeology:

> A broad subsistence base would minimize the effect of fluctuations in the productivity of any particular animal or plant and would thus cushion the population against unpredictable short-term fluctuations from one year to another or from season to season. [Hassan 1977:593]
>
> The unpredictability of resources may also have led to the expansion of the subsistence base to include minor resources such as small animal game or wild cereals, which were previously underemphasized in the diet because of their relatively high cost of extraction and the high cost of processing cereals. The emphasis on more predictable riverine and coastal resources is another feature of the Mesolithic-Epipalaeolithic that fits this model. [Hassan 1978:80]

My comments therefore will deal with the clarification of some issues.

1. The economic changes occurring during the Terminal Pleistocene were undoubtedly a necessary precondition for the emergence of agriculture (Hassan 1977:593), and the tendency to explain agricultural origins by reference to climatic conditions or events at 10,000 B.P. overlooks the importance of factors of "historical necessity," i.e., the amplification of preexisting trends that is necessary for the maintenance of the preexisting standard of living. Hayden's choice of the problem of Terminal Pleistocene adaptation (some will object to the use of the term "Mesolithic" outside Europe) is therefore laudable. His explanatory model of Terminal Pleistocene economy shows a sound understanding of the noncausal role of population growth and the calculus of economic change (resources that are least in benefit/cost ratio will be the latest

additions to the subsistence base). However, his emphasis on resource stress reflects a theoretical bias that is not tempered by his cursory reference to Braidwood's concept of cultural preadaptation and the "perceptions of people."

2. We should begin to focus on major changes in subsistence as cybernetic transitions involving a multiplicity of mechanisms and adjustments. If resource stress is inherent in the hunting-gathering mode of life, as Hayden argues, why then should it trigger the Terminal Pleistocene economic transition? Resource stress, like population increase (which some argue is inherently independent), is incapable of explaining the timing of the Terminal Pleistocene or the following agricultural or urban transformations. Hayden recognizes the role of developmental trajectory and economy, and I should think that he would recognize the need for an explanatory factor that does not reside solely in the external environment and the response to its fluctuations, without reference to the previous state of the cultural system (providing *necessary* or *material* conditions, i.e., a circumstance in whose absence an event cannot occur) and an *efficient* or precipitating cause(s). It is thus useful to look at the Terminal Pleistocene adaptation in the light of the biological, cultural, and demographic conditions of the Upper Palaeolithic. These include the emergence of *H. sapiens sapiens*, whose cognitive abilities in comparison with previous forms, including *H. sapiens neanderthalensis*, should not be underestimated. I think it is important that we reexamine the role of increasing cognitive abilities during the Upper Palaeolithic in the light of technological diversification, accelerated rate of innovation, art, and probably elementary mathematics at that time that cannot be totally explained by noncognitive factors. Greater cognitive ability would have increased the ability to learn from experience and a creative approach to the environment. Also, the Upper Palaeolithic was a time of rising world population (but not *population* pressure). Thus, though climatic fluctuations had occurred previous to the Pleistocene, it was not until the end of the Pleistocene that these climatic conditions would have had an impact on subsistence economy that could be met by creative solutions based on the recognition of new opportunities for environmental exploitation, reorganization of subsistence strategy, storage, interregional integration of resources, and technological innovation, in lieu of dispersal or stringent population control measures (Hassan 1978:77). That climatic fluctuations during the Terminal Pleistocene were violent is well documented (Butzer 1971; Hassan 1977:593 and references). These fluctuations, especially where the climate was metastable (as in the Near East), would have increased the spatiotemporal unpredictability of resources. It is here that the attempt to increase *economic* reliability (not resource reliability) may be regarded as a viable *response* to economic troubles. Climate was thus the precipitating cause, resource unpredictability the proximate cause (or *process element*). Demographic conditions, a high cognitive level, and the cumulative summation of technological innovations were the *necessary* conditions.

3. Hayden's distinction between *r*- and *K*-selected resources is useful in that it underscores the importance of the biotic potential of exploitable resources. The distinction between resources of the same strategy but different biotic potential is also important. For example, this influences the number of individuals that can be removed from a herd of deer compared to that from a herd of bison or mammoth (Hassan 1981).

4. I also concur with Hayden's emphasis on the likelihood of the emergence of certain specialized economies from an initial exploitation of certain resources. He attributes specialization to dependence on "abundant *r*-selected species." However, this statement must be rephrased in terms of the probability of locating prey items, which implies a short mean search time per item (Pianka 1974). Thus "abundance" has to be expressed in terms of local or concentration "density," which indicates the expectation of yield from a known place. It is with this reorientation of Hayden's statement that we can more readily understand the "pull" of wild cereals (which was also reinforced by their storability). This also explains the emergence of specialized hunting of big-game herds which are not *r*-selected species, such as reindeer in western Europe, mammoth in eastern Central Europe, and bison in the southern Ukraine (Butzer 1971:477). In these instances the search time is reduced by making use of aggregation and predictable migration routes. In these instances, also, the large size of the herd or animal (e.g., mammoth) compensates for their low reproductive potential.

by JOHAN KAMMINGA
Division of Prehistory, La Trobe University, Bundoora, Vic. 3083, Australia. 7 IV 81

In his criticism of the so-called homebody population-growth model, Hayden makes a number of questionable assertions and assumptions. He proposes that the model implies a direct correlation between increasing degrees of sedentism and spatial expansion of a human population. My own understanding is that the implication is not expansion in this sense, but population *increase*. If so, Hayden's ethnographic test of the hypothesis is inapplicable. Even if the test were relevant, the underlying assumptions are suspect to say the least. Proposing the corollary that expanding populations "produce large areas of linguistic homogeneity," Hayden uses Birdsell's data to demonstrate increased population density in areas of higher precipitation; he then questions why Arnhem Land, being an area of high precipitation and presumed high sedentism (compared to the desert), displays considerable linguistic diversity. My responses are these:

1. It is notoriously difficult to define an Australian Aboriginal tribe (Peterson 1976:1–2), estimates ranging from 500 to 900 (see Mulvaney 1975:65). Tribes do not necessarily represent discrete linguistic units and need not be wholly endogamous.

2. Hayden has misread Birdsell; the figure of 400–500 for a tribe is not a constant, as he believes, but a mean, the range being from about 250 to over 750 (Tindale 1974:31).

3. The presumption that decreasing tribal range can be correlated with increasing sedentism is simplistic. Aboriginal groups could be highly mobile within relatively compact territories and could exploit areas outside their boundaries.

4. It is well-known that language diversity and change are controlled by the complex interplay of historical, topographical, cultural, and biological factors, not simply the rate of population expansion. The Arnhem Land situation is explained by Wurm (1978:205) in terms of historical factors, while on the basis of detailed biological studies White and Parsons (1973; White 1978, 1979; Parsons and White 1976) have demonstrated the relevance of genetic and sociocultural factors.

The development of Hayden's resource-stress model can be traced through some of his earlier papers (Hayden 1972, 1975, 1977), although, as he points out, it also relies substantially on the theoretical work of others. I find his model unconvincing. Many of my reservations hinge on the quality of his archaeological data. It seems to me that there is a lack of substantive data to back up many of Hayden's assumptions.

A fundamental premise is that *r*-selected resources were not heavily exploited in pre-Mesolithic/Archaic times. I maintain that simple technology may well have been adequate for effective harvesting of these resources. In any case, if technological innovations such as nets, bark canoes, and sledges were essential for optimum exploitation, the archaeological record probably does not reflect their earliest appearance, since these sorts of artifacts are made from organic materials. Most of the innovations Hayden lists have broad-spectrum functions, and there is no need to invoke a late change in subsistence strategy to account for their appearance and large-scale use.

While proposing that shellfish, fish, land snails, and similar small-bodied resources were only substantially exploited at the end of the Pleistocene, Hayden acknowledges that evidence from a few early sites indicates otherwise. To his list we can add Devil's Tower (Garrod et al. 1928), Grimaldi Caves (Clark 1948:84), Haua Fteah (McBurney 1967:99), Klasies River Mouth Cave (Klein 1974:267), Lake Mungo (Bowler et al. 1970:52–55), Salzgilter-Lebenstedt (Cohen 1977:112), and Terra Amata (Bryant and Williams-Dean 1975:100–109). Despite recent criticism (Cohen 1977:94–95), the long-held belief that Pleistocene middens were destroyed or made inaccessible by rising sea levels has a good deal of merit. In addition, avian, small mammal, shellfish, and especially fish remains (see Williams 1979:109–10) are the first to disappear in adverse depositional environments.

I know of no quantitative data which support Hayden's claim that ground-stone tools are more efficient than flaked-stone tools for woodworking tasks. Ground-stone hatchets are well represented in the Australian Aboriginal toolkit, but they were multipurpose implements (Dickson 1978:34–39). Despite knowledge of grinding, Aborigines continued to manufacture wooden artifacts with flaked-stone tools. Hayden's assumption that the bow contributed to greater hunting effectiveness is also speculative. Cundy (1980) has demonstrated that it has no mechanical advantage over the spearthrower, and the reasons for its increased popularity may well relate to such factors as its suitability for warfare, it being lightweight and requiring less skill to operate.

Seemingly as an afterthought, Hayden reflects on the prehistory of the Australian Aborigines. Archaeologists in this part of the world will be surprised to learn that dietary additions such as fish, shellfish, and lizards, reflecting increased diversification of the resource base, as well as sedentism, ground-stone tools, and (unspecified) primitive valuables appeared late in Australia. Such a conclusion is an astonishing overinterpretation of negative evidence and bias in interpreting the available data (see Mulvaney 1975:147–52, 155, 158–59, 192–93, 199).

by HARRY LOURANDOS

Department of Prehistory and Archaeology, University of New England, Armidale, N.S.W. 2351, Australia. 10 IV 81

By adopting a straightforward evolutionary approach, Hayden succeeds in freeing the debate concerning hunter-gatherers from the milieu of environmental and demographic determinism in which it has become embedded. The main question is no longer "Why or why not agriculture?" but "Why change?" Hunter-gatherers, of whatever economic level, are viewed here as part of one historical economic cline stretching from the Pleistocene to the present. Explanations which tend to see hunter-gatherers as special economic cases constrained by such variables as "carrying capacity" and self-imposed demographic checks or, alternatively, stimulated by environmental pressures must now confront the wider issues of change within all economies.

Supporting evidence for Hayden's model comes from Australia, as he has suggested (see also Lourandos 1980*b*). An evolutionary sequence is beginning to emerge, although the evidence is still preliminary and much of it still to be published. The archaeological data from Pleistocene contexts, which stretch back some 40,000 years B.P., are of small, low-intensity sites, few in number. Even allowing for the effect of preservation factors, this pattern suggests mobile and transient economies and small populations. It is only in the Terminal Pleistocene that sites increase in size and number and marginal habitats (e.g., deserts, highlands) are more intensively occupied. This process, in general, can be seen to continue throughout the Holocene in most environments. Evidence for the exploitation of grass seeds begins ca. 15,000 years ago, that is, approximately synchronously with its appearance in other continents. By contact times such cereal-based economies dominated arid and semiarid Australia (the greater part of the continent) and took on highly sophisticated forms, including storage and staggered harvest practices. Ethnohistorical data point to marked regionalism in economies during this last phase throughout Australia, and this regionalism is reflected in the stone-tool record. Australian economies at contact were complex, sophisticated, and based upon a wide series of land management strategies involving fire, a host of plant and animal species, and intensive exploitation of wetlands. Sedentism was also a feature of the well-watered areas of northern and southeastern Australia. Thus late 18th-century Australia fitted much more Hayden's Mesolithic/Archaic model than that of the Pleistocene.

My own interpretation of the causes of intensification in Australia concurs with that expressed by Hayden, which, in a parsimonious vein, lays emphasis on conservative practices such as the regulation of resource yields rather than their increase. As to population expansion and its relationship to linguistic homogeneity, I had in mind a much less intensive long-term process whose effect would have been minimal if not negligible.

My main disagreement with Hayden's explanation, however, relates to causality. He draws a distinction between Pleistocene changes, which he sees as mainly due to resource stress, and post-Pleistocene changes, which involved social factors such as status, etc. While I am prepared to accept that there may have been an increase over time in the latter, we are presented here with a dichotomy very reminiscent of those criticised by Hayden and contrary to his broad clinal model. Why is competition seen as "maladaptive" for "general" hunter-gatherers when it has long been accepted as a primary evolutionary force? In all species competition results in selection of more favourable traits (in humans presumably both biological and cultural) and is thus a highly adaptive mechanism. Are post-Pleistocene adaptations being resurrected again here for special consideration, or should they be seen merely as the tip of the technological iceberg, highlighted by the early appearance of complex coastal economies revealed to us through the process of postglacial changes in sea level? Hayden also adds that ranked societies can be observed in the Upper Palaeolithic of Europe. What I am suggesting is that while environmental and demographic pressures are ever present, so too are social forces. Hayden acknowledges basic higher-primate dominance patterns, so why are Pleistocene hunter-gatherers exempted from such pressures? Godelier (1977:110–11) argues that social competition provides key incentives to increases in production. Cannot this argument also apply to hunter-gatherers? Food production is known to hunter-gatherers, as Hayden argues, and so are food surpluses. Reciprocity is a mechanism of exchange involving "storage" and regulation of resources; what is given today will be regained tomorrow, when times may be lean. Social competition at this level, especially during stress periods, would be a potent disruptive factor even in the smallest of societies. It is therefore not surprising that the holding of ceremonies for extended periods in Australia was closely connected to food production and storage. Such ceremonies were also associated with the full expression of reciprocity, social networks, and their related intergroup power plays. We should not be bemused by the more obvious expression of social competition evident in horticultural and agricultural economies. It should also be borne in mind that while reciprocity historically may give way to private ownership as economies increase in complexity, as Hayden states, it is still very much a conservative constraining force in developed horticultural economies such as those of New Guinea and wider Melanesia.

My final statement is a call for a wider recognition of the above social forces in both Pleistocene and Holocene periods and, with Hayden, for a clearer evaluation of land and resource management practices in all hunter-gatherer and hunter-horticultural economies, both ethnographic and prehistoric.

by R. G. Matson
Department of Anthropology and Sociology, University of British Columbia, Vancouver, B.C., Canada V6T 2B2. 23 IV 81

Hayden has provided us with a stimulating and provocative view of one of the most important transitions in human prehistory. I find his discussion of human population pressure and the recurrent nature of stress (actually a version of Liebig's law of the minimum) to be the best I have read, particularly concerning the relationship with culture change. Watching Hayden's argument evolve over the years, I find less and less to disagree with; either he has shifted towards my positions or over time he has become convincing. There are a number of points, however, that I do not think logically follow from his premises.

One of the crucial parts of Hayden's argument is the shift from K-selected resources to r-selected resources. He suggests that the latter are more reliable, but in fact the opposite is the case. The r-selected resources are the ones that go through boom-and-bust cycles. The K-selected forms, which do not track or map environmental changes as closely, do not become either as abundant or scarce. Therefore another explanation, or rather another mechanism, for this shift in resources must be developed.

The K-selected resources in human history are basically large mammalian herbivores. I suggest that hunting one of these (particularly among the herding ungulates) is much like hunting another, and therefore I see Hayden's Stage 3 as essentially a single adaptation—the hunting of the locally available large mammals. (An example of the general interchangeability of large mammals is the rapid diffusion of Clovis-like cultures from Canada to the tip of South America.) Given the relative stability of the large mammalian resources, why the switch in adaptation? I see the process he describes—avoidance of stress (or going to the "carrying capacity" of an adaptation)—operating, but in a slightly different way. Rather than initially replacing the K-selected resources, the r-selected resources supplemented them. It was only later, when procuring r-selected resources conflicted with the exploitation of other resources, that it was necessary to make choices between them ("scheduling," in Flannery's [1968] terms). A point not discussed by Hayden is that many of these r-selected resources are collected by women; the initial use of r-selected resources probably consisted of women's gatherings insuring against an unsuccessful hunt or supplementing a successful one. Following Hayden's logic, a supplementary source of food will allow the population to increase until periodically stresses again occur. During the shift to r-selected resources, some scheduling decisions must be made. Only when r-selected resource procurements are more productive and efficient than their competitors will they replace the K-selected resources. Usually replacement is not necessary, particularly since land mammals, unless migratory, can be obtained at various times and thus the hunting of them would not conflict with the exploitation of more seasonal resources. Once this addition of resources occurs, there is no going back, as the population is now more numerous than can be maintained by the previous adaptation alone. Since the r-selected resources tend to be more localized than large mammals, their use cannot be as easily transferred to areas in which they do not naturally occur. The means to obtain them vary more than does large-mammal hunting, causing the regional specialization seen in the Archaic. The irreversibility of this process is due to the addition of resources to the existing ones rather than their replacement. My own work on the Northwest Coast (1976, 1981*a*) shows that the large mammals are exploited there from the earliest to the most recent times but proceed from being the dominant source of food to just one resource among many. There is no indication that any fewer large mammals were obtained in more recent times (Matson 1981*b*).

by Philip Miller
Department of Urban and Regional Planning, University of Toronto, 230 College St., Toronto, Ont., Canada M5S 1A1. 8 VI 81

This paper summarizes the population-growth models and puts forward an alternative, the resource-stress model. Because of the lack of formal development, the alternative is open to many of the same criticisms as the original. The logistic equation $dy/dt = ry(1-y/k)$, where y = population and r = intrinsic rate of increase, seems implicit in both. On the one hand, logistic growth embeds a limiting value K, which is taken as synonymous with the idea of carrying capacity. On the other hand, according to this equation, population can never exceed this limiting value. Therefore, in population models, the terms "population pressure" and "overpopulation" are logically inconsistent.

In the resource-stress model, the ecological oversimplification that diversity causes stability is avoided. Many of its desirable features can be incorporated in the Lotka-Volterra equation $dy/dt = ry + a_i xy$, $dx_i/dt = sx_i - b_i xy$, where a,b = exploitation rates, r,s = intrinsic rates of increase, and x_i = quantity of resources. Weiss (1972) has applied it to the competitive-exclusion hypothesis in hominids. By changing the signs before the b coefficients, we can also model resource use with it. These coefficients can be taken to embed the technical efficiencies of production. In an ethnographic case of a contemporary fishing village, random variation in resources has produced interesting results in a simulation model of this general form (Miller 1981).

Resource variability may require greater economic capacity or migration. The resource cycle affects the distribution of strategic investment in equipment and such population variables as residential stability and dependency ratio. On an archaeological time scale, the paleoecology of species distribution and change is significant; the viability of the resource base may depend on the extent to which species are spatially localized or widely dispersed. Extending the Lotka-Volterra model into a spatial context, including refuging and specific distribution patterns, has produced interesting mathematical results. In light of such studies as Wobst (1974), it may well be of interest to archaeology.

Such differential-equation models imply continuity in change. However, representation in the form of a graph in Euclidean space is not necessarily continuous. Mandelbrot (1977) gives many examples of simple, rule-generated curves that are nowhere differentiable. It is such curves that are representative of many natural forms.

It may be a social comment that the significant correlation between mortality rates and air pollution (Lave and Seskin 1977) is of interest to archaeology. Debate over the carrying-capacity orthodoxy may influence thought in such policy-relevant fields as economics and demography.

by G. C. Mohapatra
Department of Ancient Indian History, Culture, and Archaeology, Panjab University, Chandigarh, India. 13 III 81

It is well known that there is a close relationship between the typo-morphology of hand tools and the physical characteristics of their users. This is very pronouncedly so if the user happens to be the maker as well. This being the case during the whole of the Stone Age, the author's theory of resource diversification cannot afford to overlook the morphological and anatomical characteristics of the different races of fossil hominid that appeared and disappeared in the course of prehistory, leaving behind the only reliable evidence of their subsistence patterns in their artifactual outfits.

While the actual mechanism of the technological transition among hunter-gatherers at the end of the Pleistocene was a complex phenomenon involving several factors among which no single one can be considered primary, for the sake of rejecting the climatic theory the author need not have posed the irrelevant question "Why did that transition not occur earlier?" Had he given the matter of the biological capabilities of various extinct hominids a bit of critical consideration, he would probably have been able to give a better answer to his own question. The anatomical and artifactual difference between the Australopithecinae and *H. erectus* in the Early Pleistocene and between *H. sapiens neanderthalensis* and *H. sapiens sapiens* towards the close of this epoch sufficiently indicate the limits of the technological inventiveness of the respective species for the purpose of resource diversification, with or without resource stress. For instance, that average life-span was longest during the Middle Palaeolithic should mean according to the author's model that resource stress was minimal in this period, and therefore there should have been less diversification of the resource base at this time than in the Lower Palaeolithic. This not being the case, it is obvious that the author's resource-stress model overlooks the fundamentals of the biocultural evolution of man.

In light of this, the role of certain very developed physical attributes of Holocene man (modern *H. sapiens sapiens*), among them the precision grip, wide-angle vision, and highly sensitive reflexes, which were probably absent in the non-*H. sapiens sapiens* extinct species, appear to have no relevance for the author in his resource-stress/resource-diversification paradigm. Apparently he assumes that biologically there was no change in hominids from the Lower Pleistocene to the Early Holocene. We are given to believe that a single trait explains the development of subsistence pattern and technological innovations during the whole of prehistory. To me this appears as lopsided as the population-growth or climatic-change theories he criticises.

The article, which is wholly an exercise in dialectics based on empiricism and lavishly laced with jargon, chases many a mirage. We are told, for example, that "resource reliability was increased during the Oldowan and Early Acheulian by diversifying the resource base and exploiting herd animals" without being told how this situation differed from the pre-Oldowan condition, if there was one.

by Per Persson

Department of Archaeology, University of Göteborg, Box 2133, S 403 13 Göteborg, Sweden. 15 IV 81

Hayden's starting point is a well-motivated disagreement with the population-growth models so predominant during the last decade. I can only agree completely with his criticism of these models.

Hayden considers technological improvements to be the result of periodic resource stress. This is partly right, but improvement in skill and knowledge is a continuous process not necessarily connected with stress. It originates in the fact that the human being is capable of transmitting experience to succeeding generations. This ability makes it possible for behavior to change entirely in accordance with the environment within a very short period of time, this being the main advantage of the human race. The ability to learn from others and from daily experience is a quality that makes the human race superior. The biological evolution of the human race has mainly to do with the development of the resources of the brain. The slow evolution of the mode of subsistence during the Lower Paleolithic is dependent on the level of brain capacity, which permitted only slow development. As skill, knowledge, and tools grew better and better, it became possible for man to make use of a larger part of his environment and to expand to cover nearly the whole of Africa, Europe, and Asia.

From the beginning, man used a great variety of species for his subsistence, among them many of Hayden's so-called *r*-selected species, which were in fact to a great extent the easiest to use. Not much technology and knowledge are required, for instance, to collect wild grasses such as wheat and barley. It is definitely not a day's work to collect with one's bare hands the amount of seeds needed to feed one that day. I do accept that some of the "*r*-selected species," for instance, fish, are more complicated to collect, but it is apparent that the problem of using these species to their full potential is not a technological one.

In the process of production, human beings use tools, skills, and knowledge. Labour is performed in the framework of an economic organization. From the beginning the organization of society has probably been mostly a question of securing the lives of the members and their access to the resources of a territory. Knowledge about social organization, like technical knowledge, accumulates over the generations. Knowledge of social behaviour that increases members' security is transmitted to succeeding generations and turned into social rules of behaviour. These behavioural rules in the first place affect the mode of subsistence. Cooperation, sharing, and differentiation of tasks are such rules of behaviour in the economic relations between human beings. Once such rules are established, the economy of human society consists of both relations between the society and its environment and economic relations between its members. Perhaps the first economic relation, besides the members' common access to a territory, was the provision for children by adults, promoting an extended childhood and improvement of learning ability. It is possible that the rather complex economic structure of division of labour between the sexes appeared as early as the Middle Paleolithic.

I believe that the concentration on big-game hunting during the Middle and Upper Paleolithic was made possible in the first place by improvement in economic organization and not by technical invention. Big-game hunting calls for an economic structure that allows leadership, planning of production, division of tasks, storing, sharing, and so on. By the end of the Pleistocene, a new kind of human society had developed, and this society was the one that was confronted with the stress that the decrease of big game implied. To use "*r*-selected species" to their full potential, a developed economic structure is necessary. Simple rules for sharing are not sufficient. The harvest may take place in a short period of time in which a large amount of work has to be done, and there must be rules for how the products of this work are to be shared among the members of the society throughout the year.

by Richard Pittioni

Institut für Ur- und Frühgeschichte, Universität Wien, Universitätstrasse 7/I, A-1010 Wien, Austria. 13 III 81

The author puts forward a whole range of theoretical speculations on the question of technological transitions in Epipalaeolithic cultural groups. He also raises questions for which, owing to lack of sufficient evidence, we can provide only partial answers or no answers at all. Nevertheless, his article furnishes important food for thought and is thus likely to promote pertinent research.

by Karel Valoch

Anthropos Institute, nám. 25, února 7, 659 37 Brno, Czechoslovakia. 4 IV 81

Basic changes in the fauna—the disappearance of the cold-loving and the appearance of the forest-steppe (postglacial) community—took place in the loess areas of central Europe as early as the late glacial (Alleröd). The regression of the Upper Dryas (Dryas III) did not influence the composition of the fauna. This means that the beginning of the Holocene was not accompanied by any substantial changes in the ecology.

The Alleröd saw the beginning of a new phase of the Palaeolithic—the Late Palaeolithic represented by the Azilian in western Europe, the Epigravettian and Romanellian in southern Europe, the Epimagdalenian and Tarnovian in central Europe, the "Federmesser" and tanged-point groups in northern Europe, and the continuing development of regional groups in eastern Europe. In principle, all of these are characterized by a trend towards the microlithisation of tools and the occurrence of short and circular end scrapers and slightly arched backed knives. Most of these technocomplexes (in western, southern, and central Europe) are the result of the adaptation of the hunters and gatherers of the Late Palaeolithic to the new ecological conditions. The technology of the "Mesolithic" is a necessary continuation of the development of the Late Palaeolithic (microlithisation) in the *same* ecology; in fact, it is the Final Palaeolithic (cf. Rozoy 1978).

The Magdalenian people, predecessors of the Late Palaeolithic in central Europe, specialized in hunting large animals (horse, reindeer—*K*-selected resources), although they also hunted small ones (hares, birds). No skeletal remains of fish have been discovered here (in Czechoslovakia and in the German Democratic Republic), and harpoons are also very rare. The people of the Late Palaeolithic (the Epimagdalenian of Kůlna Cave in Moravia) hunted large mammals (bovids, elk, horse, deer). Unfortunately, in Czechoslovakia finds of animal remains from the Final Palaeolithic ("Mesolithic") are still very rare; at Smolín, in southern Moravia, however, horse predominates and bovids, elk, and beaver abound; there are also sporadic finds of remains of pig, fox, and red deer (Musil in Valoch 1978). The Final Palaeolithic in Bohemia is not limited to the vicinity of rivers; i.e., it does not document the importance of fishing (Vencl 1971). In the caves of southern Germany, which do contain remains of fish, the hunting of large mammals was also the main source of subsistence (Taute 1980). Thus in the Final Palaeolithic in central Europe there was outstanding specialization in *K*-selected resources. A special adaptation to fishing could have occurred only on the seashore and in the vicinity of the large seas of northern Europe.

At Smolín we also found grindstones (Valoch 1977), but such artifacts are very rare in central Europe (including the extraordinarily rich "Mesolithic" of Poland). The grinding of seeds was perhaps uncommon and not part of the routine food-acquisition activities.

The entire culture and economy of the Final Palaeolithic, as we know them so far in Europe (perhaps with the exception of the preceramic on the Greek peninsula), represent a natural final phase of the approximately 1,000,000-year-old tradition of the hunters and gatherers of the Old Stone Age. We do not yet know of any attempts to domesticate herbivorous animals (indications of the domestication of the dog appear in the Magdalenian in the territory of the GDR [Musil 1980]), nor are there any signs of farming. No basic changes occurred between the Late and Final Palaeolithic, and the people continued to live as hunters and gatherers (cf. Rozoy 1978).

Only in the Near East were there populations gradually adopting new forms of productive economy (Natufian, Zarzian, Geometric Kebaran, etc.), and only they deserve the name Mesolithic. While they were founding the first settlements of herders and farmers, some 10,000 years ago, the plains of northern Europe were still roamed by herds of reindeer, and central and western Europe were witnessing the onset of the forest-steppe fauna and the technology of the Late Palaeolithic. The historic breakthrough occurred in Europe as late as the 6th millennium B.C., in the Atlantic period, with the onset of Neolithic farming and herding. The earliest Neolithic expanded

very quickly throughout central Europe, apparently because local populations adapted to the new economic and social forms. The population of the Final Palaeolithic seemingly played no active part in the discoveries affecting the productive economy, but it was able to adapt quickly and thus to contribute to the success of the "Neolithic Revolution," a process that took several thousand years in the Near East (Clark 1980).

The assessment of the European Palaeolithic according to ethnographic models based on the study of contemporary hunting societies mostly living under extreme ecological conditions that never existed in the temperate zone is rather disputable. It is almost certain that throughout the Palaeolithic people were forced to live in a certain balance with the natural sources of nutrition on which their existence depended. A dramatic growth of population can be documented only after the onset of the Neolithic, when, for example, in the loess area of Moravia there was on the average one Linear Pottery settlement for every present-day village. The network of Palaeolithic settlements, although relatively dense, is nothing like this.

by J. J. WYMER
17 Duke St., Bildeston, Ipswich, Suffolk, England. 31 III 81

This is a thoughtful, stimulating essay, and I would agree that one of the main differences between Upper Palaeolithic and Mesolithic economies may have been, in some cases, a considerable increase in the proportion of *r*-selected species as foodstuffs. However, I do not think that this should be used as a means of distinguishing between them. It is not a definition of the Mesolithic. The distinction between Upper Palaeolithic and Mesolithic is mainly an archaeological convenience, a temporal distinction as far as Europe is concerned, with any non-food-producing economy with a leptolithic stone industry occurring after the final retreat of glacial ice defined as Mesolithic. In Palaeo-Indian societies the distinction is blurred and the term Mesolithic has other connotations. Ignoring this, the main theme of the gradual dependence on *r*-selected species is a sound one. It is a natural development from the economy and society of the advanced hunting communities of the Upper Palaeolithic, which finds its archaeological expression in highly specialised stone industries based on blade production, use and modification of natural shelters, the building of artificial shelters, strong indications of the conservation of *K*-selected species, permanent or semipermanent places of settlement as well as nomadism, and a graphic art which betokens complex social orders. The gradual change to a greater dependence on *r*-selected species probably happened on many different occasions at different places, during and after the Late Pleistocene period. The first manifestations of it are in some of the flake-blade or Mousterian industries of the Last Interglacial or its equivalent. Hayden rightly emphasises the greater emphasis on this type of economy in Mesolithic and Palaeo-Indian societies, but it was nothing new. However, it was a critical factor in the development of food-producing societies when other circumstances were favourable. The other circumstances were both environmental and social. Food-producing societies could not have evolved from a Lower Palaeolithic (say, Acheulian) hunting economy, but not just because of their almost total (or was it?) dependence on large animals (i.e., *K*-selected species).

Hayden weakens his argument by stressing that this change in economy is a distinction between the Upper Palaeolithic and the Mesolithic. I do not believe it. Nor do I believe many of the other statements he puts forward to support it. There was a change (pestles, ground stone or flaked axes), but not a *major* change, in technology. The toolkit is still essentially the Upper

Palaeolithic leptolithic one. There were permanent or semi-permanent Late Pleistocene settlements (e.g., Combe Grenal, Kostienki, Haua Fteah, and hundreds of sites in the Périgord of France). Marine resources were exploited (e.g., at Klasies River Mouth). Such things apart, it is a very reasonable model, and if we cease to equate the dependence on *r*-selected species with just the Mesolithic period it is improved. The variety and adaptability of Mesolithic economies are clearly described in the short but profound essay by Clark (1981).

by DAVID R. YESNER
Department of Geography-Anthropology, University of Southern Maine, Gorham, Maine 04038, U.S.A. 17 IV 81

Hayden is to be commended highly for reviewing some of the major trends of Pleistocene and Holocene prehistory, emphasizing the role of technological change. In doing so, he presents a new theoretical approach comparable in scope to earlier statements by Binford (1968) and Cohen (1977). His chief contribution is to emphasize the theoretical importance of resource *reliability*—specifically, an increased reliance over time on more "reliable" resources. Although there are several problems with this concept, approaches which take into account resource reliability as well as the energy and nutrient value of prehistoric diets have been long overdue.

The central mechanism of Hayden's cultural evolutionary scheme is a "resource-stress model" involving a notion of continuous feedback between nutritional stress, induced by "cyclical fluctuations in natural resources," and "the cost of maintaining population controls" such as infanticide and other mechanisms which result in "reproductive waste." Elsewhere, I (Yesner 1977, 1980) and others (e.g., Denham 1974) have pointed to the lack of empirical evidence for the adoption of such artificial population-regulation techniques among any but the most marginal hunting-and-gathering groups. Even the ad hoc use of such techniques in situations of resource shortage that Hayden seems to imply would, in all but the most unproductive, unpredictable environments, result in wastage that would far outweigh any benefits. In a sense, this is akin to arguments about food taboos such as the "sacred cow"—i.e., it is not so much reverence for cows that is adaptive as the *prevention* of the disastrous results that would occur if people were permitted to eat their cows every time they had a bad harvest.

The outcome of this dialectic, according to Hayden, is the "maximization of resource reliability," a phenomenon which is made possible through "technological innovations," including "technological complexity and specialization." There is no disagreement as to the increase in technological sophistication that has occurred throughout the course of human evolution, but what is the ultimate driving force behind it? Hayden addresses this issue obliquely in his discussion of major "stages" of cultural evolution. For example, in Stage 2 (equivalent to the Middle Pleistocene), he tells us that "once small and scavenged animals began to be used, continued diversification of the resource base by including the effective and regular exploitation of larger animals would have been *perceived as the desirable next step*" (emphasis mine). Similarly, in Stage 3 (equivalent to the Upper Palaeolithic), we learn that "since there were still many species of large-bodied animals which were not regularly used because of the difficulty or danger involved in their exploitation, *continued motivation to diversify the resource base* during times of resource stress might be expected to have resulted in attempts to exploit these other animals" (emphasis mine). To me, these are mentalistic explanations of culture change, and I would have to place myself among those who, as Hayden says, view such interpretations as "reversions to nonexplanation." This is a piecemeal model which degenerates into description and interpretation of discrete "stages" rather than searching for underlying general paradigms of human cultural evolution. Neither environmental change nor population growth is given an important place within it.

Furthermore, the model does not deal with some specific facts. In particular, it rejects the well-established pattern of long-term growth of the human population during the Pleistocene, reflected in (1) the geographical spread of humanity from tropical origins to worldwide distribution, (2) the demonstrated increase over time in the number and size of archaeological sites, culminating in a tenfold increase in population during the Upper Palaeolithic alone (Butzer 1971), and (3) an increase in average life expectancy from ca. 15 to ca. 30 years, probably primarily through a reduction in infant mortality. This "bottle model," as Hayden terms it, of long-term population growth does *not* imply the nonexistence of population pressure, only that *emigration* was available as a mechanism to relieve such pressure.

Hayden feels that no attempt has been made to address the ultimate question of "why or specifically how human population growth is different from that of other animals." A major advance in this regard has recently been made by Lovejoy (1981), who notes that one unique feature of humans in comparison with other hominoids is their *greater* capacity for population growth as a result of closer birth spacing (among human hunter-gatherers). *Potential* human fertility is seen by Lovejoy to be quite high, making humans more *r*-selected than, for example, other apes. This has the clear selective advantage of *allowing rapid, opportunistic radiation into a wide variety of ecological niches and habitats* as the need arises. This has clearly been the great evolutionary advantage of the hominid line. In order to deal with such high potential fertility, humans have had to develop a wide variety of biocultural population-control mechanisms. And, as Hayden argues, some population growth must have resulted from opportunistic responses to greater food availability yielded by increasingly sophisticated technology.

Another of Hayden's objections to the long-term population-growth model is to be found in his statements that "it is much more difficult and complex to maintain a constant but imperceptibly low rate of growth than it is to maintain either zero or rapid population growth" and that "mankind would have been uncomfortably close to potential extinction, with a maximum annual rate of possible growth of only 0–0.0001%." No one, however, is seriously suggesting that such a low growth rate was always maintained, even on a short-term basis. Clearly, a rapid human population growth potential, counteracted by emigration as well as nutritional (and probably disease) stress, would have resulted in continuous episodes of high *local* population growth and decline throughout the Pleistocene. Hayden himself refers to "documented cases in which very nomadic hunting-gathering groups have increased their populations rapidly" and discusses the "net potential for substantial population growth among hunter-gatherers," particularly in connection with the early peopling of the New World. In sum, reduced child spacing among human hunter-gatherers should be seen primarily as a population-growth-promoting device; the fact that no further erosion of child spacing is observed among hunter-gatherers may be attributable to the necessity for maintaining *some* sort of mechanism to prevent what otherwise might be rapid resource depletion.

Hayden notes that, according to his model, "research measuring rates of resource overexploitation using human skeletal remains should show little trend toward increasing stress over time," which, in fact, appears to be the case throughout most of the Pleistocene. However, this result would be expected from *either* his model *or* the long-term population-growth one. Because emigration was an important mechanism of Pleistocene population control, one would not expect physiological stress to show up in human populations until niche saturation began to occur, i.e., in the Late Pleistocene. Following a population-growth model, one would expect first more intensive use of marginal habitats and less "valuable" (in terms of energy and nutrient cost/benefit ratios) resources as long as these were available; true physiological stress—of the type likely to be visible in human skeletal remains—should appear only when and where "settlement" solutions were no longer possible. Such areas may well have been the very areas in which initial food production occurred, as has been recently demonstrated by Cohen (1980) for areas as far apart as Egypt and the midwestern United States. The situation is analogous to Harris's (1974) arguments concerning why the Yanomamö show no physiological indications of stress even though they apparently suffer from population pressure on resources; although village fission may take place among the Yanomamö *before* any onset of physiological stress, this by no means disproves that population pressure underlies it. This is the key to understanding the fact, referred to by Hayden, that sedentary (e.g., Northwest Coast) hunter-gatherers have "little difficulty obtaining food": such a situation by no means implies that these groups are not affected by population pressure, only that they have developed efficient redistributive mechanisms for dealing with it. Clearly, as Hayden says, population pressure is difficult to operationalize, but this does not make it any less real. The two best indicators of such pressure are the increased use of marginal resources and habitats and, if unchecked, clinical manifestations of physiological strain; these operate on different levels of intensity.

It seems difficult to deny that human population growth played some role in cultural evolution (cf. Hayden's comments about ranked societies among western European hunter-gatherers during the Upper Palaeolithic). Population growth alone, however, does not explain all of the changes that took place in human societies during the Late Pleistocene; the impact of environmental change must also be considered. For example, environmental change almost certainly contributed to the extinction of the large Ice Age beasts, a fact of which Hayden makes little mention in spite of its potential contribution to resolving his dilemma about the worldwide contemporaneity of Early Holocene trends in cultural evolution (the "Mesolithic/Archaic"). He does acknowledge a role for Late Pleistocene climatic oscillation in increasing the frequency of technological innovations.

Environmental factors should also properly be raised in attempting to understand the increasing use of foods such as nuts, grasses, anadromous fish, and shellfish at the end of the Palaeolithic. Changes in the forest environment following the extinction of the large game, forcing people to turn more toward newly available vegetable foods, represent (to my mind) a more parsimonious explanation of Early Holocene events leading to the advent of food production than either population pressure alone or a teleological struggle for more reliable food resources (the "cultural developments occur when culture is ready" argument). Population pressure will not work alone, because none of these resources is marginal and many are "reliable" in Hayden's terms. On the other hand, a "reliability" argument will not work either. Hayden's model rests on the notion that *r*-selected resources are more difficult to exploit and therefore were not utilized until the Upper Palaeolithic simply because the technology had not been developed for exploiting them, but this notion cannot be applied equally to vegetable foods, which were *always* a focus of human collecting strategies. Part of the problem here involves Hayden's insistence that the so-called principle of least effort "leads us to expect that people will opt for sedentism." In fact, this cannot be demonstrated to be the case. If so, why weren't nuts, fruits, and grasses exploited more intensively during earlier phases of the Palaeolithic, allowing more sedentary populations? Lack of appropriate technology cannot be the answer; environmental factors must have been important here.

Increased sedentism *does* appear to have occurred in Late Pleistocene and Early Holocene times, probably as a requirement for more intensive exploitation of fish, shellfish, sea mammals, and vegetable foods. I believe that this created more stress on resources through further human population growth, induced by even further reduction of birth spacing than was characteristic of earlier hunter-gatherer groups. Hayden doubts this. He argues that, if sedentism correlates with population growth, linguistic diversity should be inversely related to sedentism; since the empirical evidence is that linguistic diversity is *directly* related to sedentism in Australia, he considers a relationship of sedentism to population growth invalid. The trouble with this is that linguistic diversity is probably related more to antiquity of occupation of an area than to any demographic variable; for example, linguistic diversity is greater in southwestern Alaska than anywhere else in the Eskimo realm.

Hayden additionally points to the fact that "domestication did not first occur where the environments were rich enough to support sedentary, hunting-gathering-based ranked societies," but several examples, particularly Peru and Mexico, could be cited to the contrary. As I have indicated, the origins of food production and complex society probably have more to do with the nature of the terrestrial environments juxtaposed with rich, densely populated (e.g., coastal) environments than with the densely populated environments themselves (Yesner 1980).

What about the role of social organization? Hayden sides with Cowgill in supporting "economic-based competition, as opposed to population pressure, as a prime mover in cultural evolution." I do not perceive these as mutually exclusive; pressure on resources can often be an excuse or justification for the development of ranking systems and centralized political control.

Finally, then, we are left with Hayden's notion of resource reliability. This is, however, a somewhat confusing concept, because in some places he seems to say that it is the total *assemblage* of resources (i.e., resource diversity) that promotes reliability, while in other places he appears to refer to specific resources as more "reliable." Nowhere is this confusion more apparent than in his discussion of "Upper Pleistocene trends toward the hunting of more dangerous and harder to get animals": how can these be viewed *individually* as more reliable resources? Reliability here must consist of expanding the total range of resources exploited. On the other hand, he refers to more intensive exploitation of *r*-selected resources as increasing resource reliability, principally because of the "relative difficulty in overexploiting them." While this may be true of some *r*-selected species, for shellfish it is not true because their greater reproductive rate is compensated for by the fact that they are sessile and hence easily subjected to overexploitation. Again, environmental arguments must be marshalled to explain the relatively late exploitation of shellfish (see Yesner 1980).

While the concept of resource reliability represents a significant contribution to cultural-ecological thinking, it does not seem to serve as a general organizing paradigm for human cultural evolution. Long-term population growth, coupled with environmental change, remain the most important explanatory factors, although the notion of resource reliability must be integrated into the equation.

Reply

by BRIAN HAYDEN
Burnaby, B.C., Canada. 12 v 81

There are three basic themes expressed in the comments: (1) alternative explanations of Pleistocene and Holocene technological changes, (2) questions concerning interpretation of changes in subsistence, and (3) questions concerning the portrayal of hunter-gatherers. Each of these areas is of substantial interest in itself and might well form the basis for a more detailed article.

Alternative models. A number of the standard interpretations that I had considered otiose are argued in the comments, sometimes in nearly their classic forms. These alternative explanations include the following:

1. Climatic explanations (Butzer, Hassan, Yesner). I have already made my arguments on this topic but might add, in reply to Hassan, that while the oscillations of the Würm may have been dramatic, we simply don't know how severe oscillations were in previous glacial periods because the geological record is more fragmentary for those periods. I see no reason that the Würm oscillations should have been unusual in this respect. It is also worth iterating that the Terminal Pleistocene changes Butzer and Yesner see as creating new opportunities or forcing people to turn to *r*-selected resources were not unique to that period and that these resources and similar situations existed in many parts of the world. Hassan adds an innovative element: the possibility that humans became inventive enough to exploit these resources only around 30,000 years B.P.; this will be discussed below.

2. Population-pressure explanations, classic (Yesner) and modified (Cohen, Goodyear). While I agree that fission and migration permit growing populations in unfilled environments to avoid severe population pressure for a time, it is, as Hassan (1978) has shown, unrealistic to expect that such situations can be maintained for more than a few thousand years. Even with a modest growth rate, all the available space is soon occupied, and any further growth will result in severe stress. Yesner continues to espouse the views I have previously criticized (1980*b*) concerning unrestricted population growth which is yet somehow restricted. I find his reformulation of the model's test based on evidence of stress implausible, since fission and migration into the New World can hardly be expected to have alleviated population pressure in Europe or Africa at any period. Yesner appears to have skipped the parts of my article which deal with increases in population over the Pleistocene. I fundamentally agree with Cohen's restatement of the conditions under which populations will increase; I reject as unrealistic, however, his thesis that when the world had filled up with people humans turned to *r*-selected species for food and had a hard time of it in terms of both work and nutrition.

3. Social explanations (Lourandos, Persson). While competition and cooperation clearly can affect resource utilization and innovation, these aspects of social structure have to be considered adaptations to environmental (including technological) conditions; except for some possible short-term inconsistencies, social structure—especially in the Paleolithic, with its slow rate of change—should closely follow the dictates of the available resources and technology. Clearly cooperation and competition are part of the *mechanism* of change, but they do not provide a very powerful reason for change. In order to be satisfying, explanations based to any significant degree on social structure should also incorporate a cause for change in social structure. I believe that change in the nature of the resource base is the dominant reason for change in social structure during the Pleistocene. In this sense social and materialist/ecological explanations are not necessarily so much in conflict as complementary; they are explanations at different levels. Mental sets (e.g., Deetz 1977) are even more proximate explanations than social structure, and most archaeologists find them the least satisfying of all even though it is clear that ideas must change before patterned behavior. In deference to Lourandos, I must admit to overstating the lack of competition during the Pleistocene. There can be no doubt that a certain degree of competition did exist and was adaptive, as is demonstrated by the substantial evidence for intergroup killing among Pleistocene populations (Roper 1969) as well as among modern hunter-gatherers (Hayden 1972); however, cooperation between groups and especially within them was essential and, to my mind, dominated all aspects of hunter-gatherer societies in the Pleistocene. The degree of cooperation that Persson views as central to explaining Upper Paleolithic developments seems rather to have been part of the human behavioral repertoire at least since Acheulian times, judging from the elephant kills at Torralba-Ambrona.

4. Biological explanations (Goodyear, Hassan, Mohapatra). There can be no doubt that biological changes and limitations provide the ultimate base line for behavioral changes; I have said so myself in relation to Stage 1 and Stage 2 changes in subsistence and the reasons for the slowness of change during the Pleistocene (n. 4). However, I think that most archaeologists tend to overemphasize the differences between contemporary humans and their middle-to-late Lower Pleistocene ancestors. Archaeologists are continually surprised by finds showing how clever these earlier humans were: they had fire 1,000,000 years ago, built structures 1,750,000 years ago, and were engraving stone and shaping bone (Bordes 1972:61; Freeman, Howell, and Klein 1981), using red ochre (Marshack 1981, Wreschner 1980), and performing complex religious rituals (de Lumley 1969:216) in the Acheulian. We should cease being surprised at such "modern" behavior on the part of ***Homo erectus***. From my experience with Australian Aborigines, I have long since concluded that the behavioral and mental differences between contemporary hunter-gatherers in semiarid zones and Acheulian hunter-gatherers in similar environments are so subtle as to be insignificant. This is an extremely important point, and it is probably the major reason I have sought a materialist/ecological explanation rather than a biological explanation of the technological changes of the latter half of the Pleistocene. If others, such as Mohapatra, wish to argue that intelligence only became sufficient to allow technological advances with the advent of the Upper Paleolithic, I will let them attempt to demonstrate it; I do not find much merit in the argument. As François Bordes was fond of saying, Upper Paleolithic man was probably capable of doing nuclear physics. Why should we consider *H. erectus* incapable of inventing ground-stone tools, fishhooks, nets, or baskets? The only thing he lacked was a perception of the utility of trying to subsist on very small-bodied plants or animals (most of which were difficult to procure or process with simple technology) when other alternatives were available.

5. Natural-selection explanations (Dunnell). If explaining technological change in the Pleistocene in terms of social factors approached the problem on too proximate a level for my purposes, explaining it in terms of natural selection and evolution was too ultimate an approach. I was primarily interested in middle-range (formal) theory: given the characteristics of hunter-gatherers of the last 500,000 years or so, why did specific technological and resource exploitation changes take place? To relate these behavioral and technological developments to natural-selection processes was beyond the scope of my article, although I am in total sympathy with such a perspective. As with the emphasis on social structure, the difference is one of level of interpretation, and thus I do not view my approach as conflicting with Dunnell's. In fact, what Yesner misconstrues as a mentalist explanation on my part is an example of my sociobiological leanings. As long as mental perceptual or structural characteristics of humans can be related to more basic causes, explanations based on them are acceptable. I would suggest that the perception of larger "packages" of food as more desirable than numerous small ones is rooted in the structure of the human nervous system and probably one of the most consistent of cross-cultural phenomena. Given a choice between a large mammal and a multitude of *r*-selected specimens of equivalent or even greater food value, especially where not all the small specimens are visible at once, there is little doubt that the large mammal will capture the

individual's attention and that, in the absence of empirical testing, people will be unable to assess accurately the relative food values of the small-bodied resources. Certainly, Piagetian experiments suggest that this is true of individuals lacking formal schooling and training in quantitative techniques. I view this characteristic as stemming directly from the operation of Darwinian selection on hominids of medium build lacking most technology.

While I would reject some of the alternative explanations suggested by commentators outright, others (e.g., biological changes) can be usefully combined with the resource-stress model (as I have done for the early stages of human evolution) and still others (e.g., social and selectionist explanations) may be applicable at a more specific or a more general level.

Subsistence changes. Some commentators (Bowdler, Kamminga, Persson, Yesner, and to some extent Wymer) would seem to argue that there were essentially no major changes in the use of resources throughout the Pleistocene. Various of these cite finds of shellfish or fish in Middle Paleolithic sites and evidence of seed grinding as early as 15,000 B.P. I am quite comfortable with the latter, whether in Australia or North America. As Lourandos points out, this is when similar developments occur elsewhere in the world, and in terms of world prehistory it *is* a "late" development. As to claims for early shellfish use and the possible even earlier use of grass seed, I have said that "it had probably always been recognized that small-bodied foods were edible; however, since they were so small and difficult to obtain, they were perceived as having little food value. . . . they may have been used occasionally as low-return foods throughout the Pleistocene during times of extreme shortage." My scenario calls not for complete absence of these resources in early periods, but for their development from an infrequently, opportunistically used, low-return food to a major high-return staple (see fig. 3). The archaeological record is remarkably consistent with this view. To use some of the examples cited by the commentators: The shell deposits at Haua Fteah or Devil's Tower have not been measured, and it is entirely conjectural what "abundant" shell or "thick layers in the hearths" meant to the archaeologists of the time. Moreover, there may be problems at both these sites (see Volman 1978). At Lazaret only a few fish bones were recovered, certainly not enough to demonstrate regular or reliable procurement of fish. While the opportunistic use of fish and shellfish is represented in the South African sites during the Middle Stone Age, Kamminga and others should have read farther in the reports:

> the Klasies MSA levels contain far fewer remains of fish and flying birds than are found in comparable LSA [Late Stone Age] horizons. . . . MSA peoples exploited coastal resources less effectively than LSA people in the same habitat. . . . the remains of fish and of flying birds are confined almost entirely to the LSA levels. [Klein 1975:266]

> The implication is that fishing and fowling (for airborne birds) were probably beyond the technological capabilities of the [MSA] Klasies people. [Klein 1977:121]

> [the data] suggest that such [marine resource] utilization was less intensive during the Last Interglacial and earliest Last Glacial than in the Terminal Pleistocene and Present Interglacial. . . . implements such as gorges and line (or net) sinkers, which are reasonably interpreted as fishing and fowling gear, are so far known only in LSA contexts. . . . The most economical explanation for this contrast is that MSA peoples were technologically incapable of active fishing and fowling. [Volman 1978:913]

It is these and parallel changes elsewhere in the world, not the presence or absence of evidence for use of specific food types, that I have attempted to explain. It should be clear from the above that Middle Stone Age people did not actively seek out *r*-selected resources; they used them when opportunities for easy procurement arose and when food was in short supply. For earlier times, Isaac (1971:293) notes that fish bones and shellfish have been found only in very small quantities and "do not appear to have been important gathered foods in spite of the proximity of the sea." This is especially noteworthy in connection with Acheulian sites close to old shorelines around Casablanca. Klein (1977:121) makes a similar observation. Moreover, there is no evidence for the systematic use of freshwater fish or shellfish (aside possibly from Lake Mungo) anywhere in the world, and this lack of evidence cannot be attributed to marine transgressions. Although Kamminga argues that simple technology can be used to collect such resources, there simply are no sickles, seed grinders, net weights, mortars and pestles, edge-ground implements, or other tools to indicate *intensive* use of small-bodied species. In this regard, if Persson believes that grass seed can be easily collected and eaten, I invite him to make a full meal of any stand of grass without technological assistance. Even if he were to demonstrate that it is possible to consume some of the green seed/glumes/rachi, such meals would have been of very limited value prehistorically and available only for very limited times. It is exploiting ripe, dry grain that has the nutritional advantages, and for this technology is definitely required. Lake Mungo is at this point the only site in the world which might be considered an exception, and even here populations may have simply been taking opportunistic advantage of the temporary ponding of water from seasonal floods to capture fish, as was done in Upper Paleolithic Egypt (Wendorf and Schild 1980:268), rather than using technology. In fact, there is no evidence for any fishing technology, except for the spear, anywhere in Australia at this time.

Given both the lack of technological indicators for the intensive exploitation of *r*-selected resources and the rare and relatively insignificant occurrences of these types of food themselves, it is appropriate to conclude that, for the Paleolithic as a whole, *r*-selected resources were infrequently used emergency resources, chance beach finds, or unusually rich concentrations of fish or shellfish that could be obtained with ease. Therefore we can expect to continue finding fish, shellfish, and other small animals from time to time throughout the Paleolithic, but rarely if ever as staples. Some small animals may also have been hunted in the Middle Paleolithic for their pelts (Klein 1975:265). With regard to fish, in spite of occasional use throughout the Paleolithic, it is not until the Natufian and Mesolithic of Palestine (Perrot 1968:382; Moore 1979), the Magdalenian of Europe, and about 12,500 B.P. in northeastern Asia (Chard 1974:33) and South Africa (Klein 1977:121) that there is evidence of their being used intensively as a staple. I have admitted that I can see no good reason some species of shellfish should not have been exploited from the earliest of times; I would not consider my argument weakened if it were to be demonstrated that this was the case.

Both Bowdler and Kamminga express surprise at my characterization of the Australian sequence. While there may have been some early opportunistic use of fish and shellfish, there is no doubt that the Lake Mungo population relied to a much greater extent than Bowdler has indicated on moderate-sized land mammals ("There were in addition many broken bone fragments which belonged to animals much bigger than the ones listed" [Bowler et al. 1970:53]). I don't doubt that the earliest inhabitants of Australia used littoral resources, but it is difficult for me to view this as a technologically sophisticated or intensive adaptation. In all probability it was basically opportunistic. For example, the really systematic use of shellfish documented at the Weipa shell middens simply does not appear at earlier sites in Australia; nor does seed grinding, the use of airborne birds, or sedentism. My reference to the use of lizards was admittedly more conjectural. On the other hand, ground-edge axes do occur in a context consistent with technological elaborations related to increasing resource diversity and/or reliability (see Hayden 1977)—Kamminga has misinterpreted my arguments in this matter; I have *never* claimed that ground-stone tools are more efficient than flaked-stone tools—and I think that their occurrence at around 20,000 B.P. is acceptable within the broad outlines of the model I have presented. The Australian developments may not be exactly synchronous with developments elsewhere in the world, but the *relative* sequence is very similar and ends with the construction of canals for food production, for which there is no known analogy in early Australia (Lourandos 1980*b*). It is gratifying that Lourandos also perceives substantial changes in the use of resources in Australia along the general lines I have suggested.

Thus the archaeological data consistently support the overall subsistence changes I have outlined, and most regional syntheses of Paleolithic subsistence independently show similar sequences. The exploitation of *individual* small-bodied resources is simply not very profitable. People cannot see them all amassed as a single food unit, and they cannot easily evaluate the total food potential of these species without considerable experimentation. Such resources cannot be consistently exploited profitably except en masse, and for this a very considerable amount of motivation, experimentation, and technology is generally necessary.

Portrayal of hunter-gatherer life-styles and technologies. Butzer suggests that resource stress is a yearly occurrence; I would argue that such yearly stress is a very mild form—the type everyone expects to pass through briefly and easily. Taking the opposite view, Goodyear maintains that hunter-gatherers *never* experience severe stress or starvation. The ethnographic record is clear that episodes of severe deprivation and even starvation did occur, though infrequently, under traditional foraging conditions. In addition to the evidence cited in Hayden (1981:table 22), Long (1971:268) observed one group of Central Desert Aborigines in a very bad state of malnutrition, while Duguid (1963:58, 138–39) records several instances of near starvation in the Petermann Ranges and in Western Australia. In addition, while Lee has portrayed the !Kung as never experiencing severe shortages, this may be because of their recent more intensive contacts with herders. Earlier observers reported of Bushmen that "during bad seasons . . . they are . . . reduced to great straits, emaciated to a dreadful degree, so that it is a wonder they survive. Their bodies are fearfully shrunken, so that they look and are literally bags of bones" (S. Dornan, cited in Bodenheimer 1951:138).

Goodyear finds the assertion that man:land ratios remained unchanged from the Oldowan to the Neolithic incredible. It should have been clear that I was referring to the ratio of resources to people and the frequency of resource stress as a function of this ratio, not to area of land used per person.

Goodyear and Cohen view the technology involved in exploiting *r*-selected resources as energy-costly. I find it inconceivable that, when a group could collect enough grain in a week to feed itself for a year, processing would have required so much additional time as to increase the average subsistence work load significantly above two hours per day for the year as a whole. The same might be argued for the massive procurement and processing of fish, as on the Northwest Coast. Perhaps these commentators are aware of some quantified data unknown to me; perhaps they have neglected the processing costs of other types of food (e.g., butchering time, shelling time for nuts, cooking time, etc.). In any event, a recent review (Hayden 1981) has disclosed no good data on the topic and indicated no major increase in work levels associated with subsistence among hunter-gatherers using *r*-selected resources as opposed to *K*-selected resources. I would also insist that Lee's remarks on the !Kung aversion to travelling refer not only to foraging distance from sites during aggregation phases, but to *total* travel. It seems self-evident to me that *all else being equal,* hunter-gatherers will adopt strategies involving the least amount of movement. I readily concede that there may be other factors militating for sedentism.

Yesner's claim of a lack of empirical evidence for the adoption of artificial population-regulation techniques is evidently based on a very selective reading of the ethnographies. In addition to the documentation I have published (Hayden 1972), revised data on the !Kung place infanticide at six per hundred births (Ripley 1980:353n), and social means of controlling overpopulation, including infanticide, are reported among many mammals (Ripley 1980, Cohen, Malpass, and Klein 1980). Additional cases for hunter-gatherers are provided by Dickeman (1975) and Van Arsdale (1978).

In sum, I maintain that my characterizations of hunter-gatherers are well founded.

Miscellaneous comments. Goodyear considers resource stress not broadly enough defined and lacking in predictive value. I would counter by saying that it is *the only* operational concept available to archaeologists and, as far as I can see, is well tailored to the questions and problems posed here and should be useful as a heuristic and predictive device. While Bowdler finds "reliability" useful for partitioning the types of food consumed by hunter-gatherers, her use of the concept is essentially static and cannot address the question of change. Criticisms of my linguistic test of population growth stemming from sedentism (Kamminga, Yesner) do not address the central concept: that unchecked population growth as represented by Lee (1972) and others must result in large, linguistically homogeneous areas over one or more millennia. I said nothing about the causes of linguistically diverse areas. My use of linguistic-migration theory is consonant with other recent applications in Australia (Gruhn 1980:805). Arnhem Land hunter-gatherers are clearly more sedentary than most other groups in Australia, no matter what definition of tribe is used, but present no evidence of population expansion.

Valoch's discussion of Late Pleistocene and Holocene reliance on big game seems intended as an example of failure to use *r*-selected resources. I would add that Plains Indians, Subarctic Indians, and perhaps the post-Pleistocene groups cited by Wymer never got over their fixation on large game either. My point was that if an environment is not rich in *r*-selected resources, post-Pleistocene adaptations in that area will not emphasize them and in many respects will more closely resemble Pleistocene adaptations.

Dunnell and Cohen are the only commentators to pick up on the fact that *r*-selected resources can fluctuate dramatically from year to year and in this sense are not quite as "reliable" as I suggest. While such fluctuations do characterize many *r*-selected resources, in comparison with the fluctuations of *K*-selected species they are very brief (one or two years) and can usually be compensated for by other resources (providing the options are diverse enough). In fact, alternative resources can even be overexploited during such periods, since the bounce-back time for the *r*-selected resources will be short and conservation of alternative ones will not be critical. In contrast, larger species take much longer to reestablish their populations (Steward 1938:35). Antelope took 12 years or longer to reestablish their populations under traditional circumstances, while for the Subarctic it has been argued that following overexploitation large mammal populations were depressed for hundreds of years, leading to starvation among hunter-gatherers in the area (Bishop 1978). It is also possible that in areas of rich *r*-selected resources sufficient food would be available to get by even in the worst years because of the incredible potential abundance of these resources. It is inconceivable that no fish would be caught on the Northwest Coast or that no grass would produce seed in the Near East.

The most important issue in this article is that there are major differences between *K*- and *r*-selected species when used as human resources and that these differences have considerable potential for explaining many of the characteristics of societies using primarily one or the other. I may not have identified all the critical relationships correctly, but I am convinced that a fundamental relationship is present which archaeologists cannot afford to overlook in their quest to explain cultural similarities and differences. To this end, I would like to thank Hardesty for his elaboration of one of these related concepts; I would also like to thank all the reviewers for contributing to the general ferment on the topic, for surely why cultures change is one of the most important questions that archaeologists can address.

[The comments of Druss and Matson reached the author too late to be considered in his reply.—EDITOR.]

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