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Research in Population and Culture: An Evolutionary Framework¹

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Theories on the relationship between population and social evolution, such as those of Malthus, Marx, and Boserup, have disappointed analysts who wish to use such theories as a basis for inference in unrecorded history, largely because of the unrealistic specificity of the hypotheses and the lack of adequate data in categories that help to clarify the application of theory to human population history. We propose that it is time to reformulate such a theory, drawing upon a new level of sophistication in anthropological demography that permits more complexity in the theory itself about alternative outcomes of stressful points in population history and the testing of hypotheses in more realistic contexts of continentwide examinations of populations influencing each other by their expansions and contractions. We anticipate finding that population fission is a particularly interesting alternative response to population pressure when we are dealing with relatively sparse populations and that the place of the given population, in the core or on the periphery of population growth centers, is crucial in influencing the alternatives used.

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Four observations motivate us to make the following proposals on the place of population theories in anthropology. First, we see the fundamental demographic events of birth, marriage, migration, and death as imbued by all human societies with intense emotional and moral significance. These events are central to the drama of human life, and all cultures treat them as important and interesting, worthy of elaboration and ceremonial notice.

Second, we are encouraged by the increases in technical quality of the demographic studies in anthropology that have been appearing in the past few years (Howell 1986a). This higher-quality analysis and discussion, largely free from the ambiguities and errors that characterized demographic anthropology in the past, give us optimism about the expected yield from theoretical work in this area.

Third, we see the subject matter of demography as cutting across and unifying the subdisciplines of anthropology as few other topics do. A focus on demographic phenomena provides a common ground for observation, discussion, and intellectual integration across the subfields. The simplicity of demographic concepts, the possibility of unambiguous counting procedures, and the consequent ability to test hypotheses provide opportunities for new research and new collaboration between physical and biological anthropologists, archaeologists, and social and cultural anthropologists interested in a range of questions.

Finally, we see human demographic regimes as important causes and effects in a theory of the coevolution of the human species and the cultural equipment that it employs. Evolutionary theory has the potential to be central (and integrating) to anthropological theory, but the specifications of evolutionary theory have been deterministic and teleological, and its mechanisms for mankind have often been vague. A demographic perspective may dispel some of this vagueness by focusing upon recognizable events in real human groups. The role of culture in human evolution has presented many difficulties for theory. A demographically based formulation may integrate cultural with biological evolution (Bennett 1976; Durham 1978; Harris 1977, 1985; Wilson 1975).

We freely admit that none of the ideas here are entirely new, even when we cannot at the moment point to our predecessors. What we propose is not really a theory but only a starting formulation, a partial specification, and an incitement, to ourselves and to colleagues, to join in constructing a philosophically acceptable extension of evolutionary theory, one in which demographic events are the central mechanism and leading indicators of the coevolution of bodies, minds, and societies. We cannot emphasize too strongly, however, that in our view "evolution" is not a direction but a set of interacting mechanisms resulting in the production of variation and its selection.

Global Population History: A Statement of the Question

Our colleagues R. D. Lee and K. Wachter have in recent seminars and in forthcoming publications addressed themselves to problems of population homeostasis in premodern populations. At the base of these explorations is the question why human populations grew so slowly over the enormously long span of prehistory. Wachter (1985) spells out that the theory of branching processes in statistics tells us that, no matter how slow a rate of growth, the population(s) characterized by it will, given enough time, become either extinct or of infinite size. The population of *H. sapiens* has done neither during the period of observation, although it may have had time to do both. Critical to this question is the definition of the date and size of the "starting population." Under some plausible assumptions, Wachter shows, the inevitable outcomes of branching-process theory may not have had time to emerge. Lee (1984) has maintained that the answer lies in the negative feedback mechanisms of Malthusian homeostasis operating on the population demand side and that at some point a threshold is crossed at which Boserupian endogenous technological change operates on the resource supply side.

Our response is that these are answers to the wrong question. It seems to us misleading, at least at this stage of our theoretical sophistication, to talk about the total population of a species unless that species is localized

and a breeding population. From the time of its initial expansion, *H. sapiens* was neither. The stochastic processes leading to extinction or infinite expansion applied to local, not global populations. The zero or near zero net growth of the global population could have been an average of widely differing local population growth rates. If theories about response to resource constraints are to be applied within the framework of economic rationality and decision making, or even within the framework of random variation and selection, they must apply to populations *sensu stricto*, not to aggregates of populations.

How to combine theoretical principles that operate at the local level with concepts of global population and resources is an important problem, and when in human history we have to begin to think about local/global differences is another. How can we turn these ideas and the basic notions of evolution itself to an understanding of the coevolution of human demographic regimes, the species, and its cultures?

Elements of the Theory

The main thrust of our argument is to place the development of human demography and coevolved culture squarely within the framework of biological evolutionary theory—evolutionary theory as modern biologists know it, not as some others remember or imagine it. The two main points of that theory, simply conceived, are (1) that variations—in genes, in individuals, and in cultural elements, large and small—appear as "errors" in the process of transmission of elements of information from one generation to the next and (2) that some of these "errors" have more reproductive success than others in particular environments. Their initial appearance is independent of their potential reproductive success, so that this evolutionary scheme in its simplest form depends only on random variation and selection, in the classic style of the Huxley synthesis.

We expand on these basic notions in minor ways. A first modification is obvious: beyond the occurrence of random variation, sentient goal-oriented animals can and do give cultural development a twist in which the occurrence of "copying errors" on any level can be intentional and designed to be adaptive. (Whether they are adaptive, and for how long and in what environments, is moot.) That is, in humans the invariability of the statistical independence of variation and its selection is lost in principle at least in part. Second, we propose that one response to reproductive success in the presence of ecological constraints is group fission and that fission can act as an intensifier of evolutionary development, multiplying the opportunities for change. Third, we propose that fission makes some kinds of cultural developments, especially communicative ones, particularly adaptive and that these developments have substantially changed the environment of demographic and social processes.

We need to provide a few definitions and some clarification of our assumptions. A *population* is a group

of self-replicating organisms of a single species, such that most of the breeding is done within the population. Another useful definition stresses the multigenerational nature of a population: it consists of the common descendants of a group of ancestors, and within it future descendants are produced and raised. The minimum size of a population will be set by the ability of its members to provide mates for one another's offspring. The maximum size will be set by the ability of its members to mix freely with one another, so that an individual is about as likely to select a mate from one segment of the population as from another. A population can be internally differentiated, but its members necessarily share a geographic base, characteristic responses to problems, language and culture, etc. Groups that violate these definitional criteria are special cases that may not be well described by our model. We do not assume that populations are tightly bounded or sealed-off units: the island isolate population is a rare, even if an important, phenomenon in population history (Benoist 1973). We expect that in every generation some members of the population will go outside, and others will come in, bringing their genes as well as their culture, their experience, their language, and their social ties with them. The degree of transfer between populations per generation is an empirical question complicating our need for an operational definition of population. Adams and Kasakoff (1975) suggest that 80% of breeding choices made within a group is a useful and realistic (even if arbitrary) criterion. One implication of these definitions is that a population is frequently not coterminous either with the local living group (the population may be much larger) or with the society (the population may be much smaller). The concept of "effective population size" (Kimura and Crow 1963), difficult as it is to operationalize, is helpful in this context (see also Barth 1969, Hammel, Wachter, and McDaniel 1983, Wobst 1974).

The assertion that a population "confronts" scarcity and "responds" to it by making choices or selecting alternatives is difficult. What does it mean to say that a unit as potentially amorphous and unorganized as a breeding population makes decisions or has alternatives? The units involved in perceiving and deciding are likely to be subunits of a population—individuals, families, leaders, and, at some levels of population development, perhaps the government of a nation. Nevertheless we continue for reasons of theoretical simplicity to focus on the level of the population, even when the spokesmen for and the obvious decision-makers in populations may not do so.

We do not claim that populations "make history just as they wish," to paraphrase Marx. We can assume that actors will be rational, at least on the average and in the long run, in the expectation that people prefer full bellies to empty ones, leisure to hard work, survival to death. But we also recognize that rational actors may not perceive these issues, may have other agendas than demographic ones, and may well not care about the unit of the breeding population. We recognize that populations can only move from where they are at the moment, that

alternatives unavailable to a population for any reason, including lack of organization, are not alternatives for them, and that the actions and decisions of other populations will frequently be major factors in the immediate situation of a particular population.

Finally, what do we mean by *scarcity*, given our desire to formulate a framework that will work equally well for protohominid bands and the "world system" of modern market capitalism (Hayden 1975)? In most cases, we expect that absolute declines in food or other material resources signal scarcity, but the level of need per person is subjective and variable. There is perhaps no level of plenty that would satisfy an elite focused upon consumption and perhaps hardly any level of scarcity that would fail to satisfy a Zen community. We assume that a population needs to meet some standard of per capita distribution of goods and services to its members. The needs are met by the application of land, labor, capital, and organization to the environmental resources available to the population. We assume that there is a constant process of assessment and evaluation of the relation of population needs to resources. When the resources, compared with the absolute standards of the people or with the standard of the recent past, for example, are in balance with the needs of the population, we anticipate the continuation of the population without pressure for change in any of the elements of the equation. (We do not assert that social, cultural, or technological change will only occur when there is population pressure, but merely that population pressure in this sense will always produce a need for evaluation of change.) Population pressure need not be produced by population growth, although that is one frequent path to pressure. Others are reductions in productivity of the technological base, a loss of land or other elements of production, an invading army, or just a run of bad weather or bad luck. When there is a surplus of resources relative to population (which can be caused by a decline in population as well as by an increase in resource availability), the surplus may be "spent" in the form of less work, more leisure, more pleasure, or investment in the elements of production for future consumption. Alternatively, the population may respond to surplus by an increase in size or by some combination of increase in numbers (as Malthus suggested long ago) and increase in the size of individuals.

Responses to Population Pressure

Population history consists of the results of responses to pressure on resources through a relatively small number of choices available to populations. Such a history is modified by stochastic variation, especially in small populations. Through these responses and fluctuations, populations are created, maintained, and transformed into larger or smaller units, and organizations and ideas and technological systems are created and transformed. The direction of change observed in *surviving* populations has tended to be toward the larger and more com-

plex, but we do not assert a unidirectional system. Small societies are at greater risk of disappearance for competitive and stochastic reasons and are less likely to be part of a set of survivors; thus those tending toward smallness are less likely to appear to us. While combinations of responses are possible, in the most general terms we see four alternative responses to population pressure as explaining most of population history. In discussion of all of these points we must recognize complementary responses when population pressure is negative, although we go into little detail.

1. *Fission*. The population can expand geographically (or attempt to expand). The form that this takes will vary depending upon whether the population has unoccupied land available and with how large a membership its social organization can cope. When there is vacant land and social organization is relatively inelastic, we expect expansion by fission. When the population is surrounded by other populations and there is no unoccupied land available, we look for forms of conflict with neighbors, and we expect to examine carefully the differing positions of those who start territorial conflict as opposed to those who are trespassed upon and different positions for those who win and those who lose. When population pressure is negative we may often expect fusion of social groups in response.

2. *Limitation*. The population can restrict fertility, increase mortality, or otherwise modify itself to adjust population size to the available resources. This may involve deliberate changes or quite unconscious adjustments and second-order effects and may require technological or cultural change or be strictly biological. Clear density-dependent population restriction does not work invariably and automatically for human populations, but this is not to say that it plays no role in population regulation (Howell 1980). Malthus (1976 [1798]) called these responses to population pressure "the preventive checks." When population pressure is negative and resources exceed demand, population may grow as a consequence of inherent excess fertility capability, or special measures may be taken to enhance fecundability, depress mortality, increase exposure to the risk of pregnancy, etc.

3. *Intensification*. The population can increase density and adapt itself to increased population size by intensification of food production (or whatever the scarce resources may be). This response usually involves technological change and is familiar as Boserup's (1965) view of population adjustment. If population pressure is negative, the population can fall back on less intensive modes of production. Modifications of the practices of distribution of the product of economic activity may also be made; they may affect the resource balance of segments of a population.

4. *Decline*. The population can restrict or reverse population growth by increases in misery, starvation, sickness, and war. Malthus called these factors "the positive checks." We would also include here decreases in consumption, although it might be difficult to distinguish these empirically from shifts in consumption, which

might be classified under Alternative 2, or changes in distribution, which might be included under 3.

These outcomes should be considered not as competing theories of population adjustment but rather as alternative responses used or experienced by populations under varying conditions, with considerably different consequences for the future of the population in question (and perhaps of others, to the extent to which populations are an element in the reality confronted by other populations) within an evolutionary theory. (This view of complementarity, at least of Malthusian and Boserupian approaches, has also been propounded by Lee [1984] and Simon [1978], among others.) We see the task of demographic anthropology as specifying the conditions of complementarity and testing the hypotheses derived from them.

Initial Theoretical Considerations

The names of three major theorists emerge in any discussion of the dynamics of population: Malthus, Boserup, and Marx. We take these three theories in their simplest form as making these basic points:

All three are characterized by some notion of the short-term finite nature of resources for exploitation by human populations, a notion of a carrying capacity of the environment more or less complexly determined by the qualities of that environment and by the perceptions, habits, technology, and social structure of the exploiting groups.

All three agree that humans, like other animal populations, are characterized by an inherent tendency to increase toward that carrying capacity as defined in the short term, in the absence of any customs or responses that would diminish that increase.

In Malthus's (1976 [1798]) view, an approach too near the resource boundary would trigger misery in varying combinations of the Four Horsemen of the Apocalypse. These positive checks on population could be avoided by three mechanisms: adoption of methods of fertility control, prudential delay in marriage (either of these holding down the increase in population), and exogenous technological change that increased the resource base. He regarded the first mechanism as a vice, the second as morally appropriate, and the third as providing only temporary surcease. This is, with apologies to the sequence of historical events, a Darwinian view, in which the appearance of technological solutions is not (necessarily) brought about or encouraged by the conditions of existence but *exogenous* in the economist's terms or random in those of the evolutionary biologist. Thus, although technological change may relieve population pressure, its emergence is made no more likely by the prior existence of such pressure.

Boserup's (1965) view differs in proposing that technological change emerges *endogenously* and *in response* to population pressure on resources, in principle infinitely extending the cycle of challenge and response.

Marx's (1967 [1867]) view, particularly in criticism of

Malthus, was that Malthus misidentified the source of the immiseration of the working class. It was not, for Marx, the mere tendency of populations to increase toward or beyond the carrying capacity of their environment but rather the maldistribution of the products of their labor.

Particularly in the light of the evolution of Malthus's thought as illustrated in the successive refinements of his *Essay*, we see no reason these views should be regarded as necessarily incompatible. Political polemics and rhetorical strategies aside, there is no barrier to thinking of the resource base as defined by the environment not only as culturally perceived and exploited but also as socially distributed. Similarly, although history as we know its evidence has shown Malthus to be wrong and Boserup right, in the sense that technological change has in some sense continued to overcome resource scarcity, any definition of a finite universe would necessarily show the classical Malthus right, in principle, in the very long run. There are of course empirical questions to be answered on the degree to which the positive and preventive checks have operated; our point is only that within reasonably known history the inevitability of the positive checks and the rarity of technological rescue seem less certain than Malthus painted them. The critical difference between the classic Malthusian and Boserupian views is whether the appearance of cultural variation is independent of its adaptive value—in this context whether technological (or, more broadly, cultural) change is exogenous. Even this difference is not a necessary one in any broader theory of population and resources, for there is nothing in Malthus insisting that technological change be exogenous; his argument requires only that it be, in the long run, futile. Complementarily, there is nothing in Boserup that insists that technological amelioration is permanent; on the contrary, the cycle of challenge and response extends into the long run. How long that run will be, and what cultural considerations will intervene in tolerance of increased densities, are unknown to us.

The prospect of broad theoretical unanimity should not blind us to the existence of problematic areas. The first of these is the notion of carrying capacity or resource boundary, net of any technological innovation. A population lives in and simultaneously exploits a habitat, which consists of a territory and the resources, especially food, contained in it. The perception of what is edible is not constant and immutable. Neither is the perception of what is habitable, or marriageable, or exploitable in any other sense. Hierarchies of food preference are observable in animals and more elaborately developed among human beings. Thus, part of the response to approaching the resource boundary is to eat something different. This makes for a flexible resource boundary in two senses. First, the eaters may move down the hierarchy of food preferences. Second, they may alter the hierarchy of preferences. The first tactic allows populations to subsist at the cost of reducing culturally appropriate satisfaction. The second device allows subsistence and recovers satisfaction. Although at some point, in the

absence of some new technology or biological change (such as developing the ability to digest cellulose), the resource boundary of a given environment is fixed, the ability of human populations to avoid such boundaries is impressive. We call attention to the need for culturally appropriate and not merely external definitions of immiseration.

Another difficulty lies in Malthus's oft-quoted assertion that population increases geometrically (exponentially), while the food supply increases only arithmetically (linearly). This notion is questionable in any context but particularly before primary dependence on the control of fixed areas of land typical of most known horticultural and agricultural societies. Food supplies are populations of edibles that wax and wane just as geometrically as do their consumers. It is the territory supporting the edibles that does not increase geometrically. If territorial limits are flexible, a population simply has to move enough to maintain some acceptable level of exploitation of renewable resources, whether these are acorns, deer, or milpas. By contrast, if the area available is fixed, productivity can be increased only through some kind of technological change and probably only at the expense of diminishing returns to labor. This observation leads to the view that the problem posed by Malthus did not become important until population densities became so great that mobility was restricted.

Our argument rests in part upon the recognition that Malthus, Boserup, and Marx conceptualized the relation of population to resources as a brief encounter, with a beginning, a middle, and an end. Our extension of their argument attempts to take into account the continuous nature of the confrontation of population and resources, in which members of populations, generation after generation, experience and perceive surpluses and scarcities in resources relative to their numbers. We assume that biological and social responses to scarcities are transmitted from one generation to the next, so that responses which restrict population growth in one generation will likely do the same for subsequent generations.

We stress that biological and cultural evolution provide alternative and sometimes complementary responses to population pressure. While it seems to be true that responses have been increasingly social and cultural over time, there is no point at which biological evolutionary responses end in the human record, and we should be cautious about assuming that cultural, social, and technological responses cannot appear even at very early points in the hominid record. For instance, the dates at which the origin of language and the control of fire produced important consequences for population are not known with any degree of certainty.

Howell (1985b) has considered some of the elements of the culture of early hominids that are likely to have had considerable survival value and hence were particularly likely to be retained once established for whatever reason. These elements include cooking, provisioning of members of the group, especially children, beyond infancy, and nursing of the sick, injured, and disabled of any age. She has argued that an automatic consequence

of the development of these cultural traits would be an increase in the frequency of survival of some adults past the reproductive ages. These cultural traits would transform a primate group from one resembling living groups of gorillas and chimpanzees to something we would recognize as human, but it is not at all clear that it was these and not some other cultural traits that transformed the human species.

Which responses to scarcity are elicited and which rewarded and/or "punished" by the environment can have important consequences for the population. The population itself may be transformed into a larger one, at a new exploitative equilibrium, or may be restricted to its old size and exploitative techniques. Cultural conservatism, or integrity if you will, makes cultural systems slow to respond to perceived scarcity. Nevertheless we take the view that in the long run population size and the cultural corpus within a given environment are usefully seen as having some long-term homeostatic equilibrium point. Failure to achieve the particular point appropriate to a given environment and given cultural corpus should continue pressures in its direction or result in its modification through either biological or cultural change.

The Fission Alternative

The fission alternative is particularly interesting because we regard generational replication as deeply conservative and insulated against change. Although cultural change is not the goal of the process of adjustment to scarcity, it is often a consequence, and one of the most interesting from the point of view of the long-term observer. We argue that cultural change is more likely in many ways in a small population than in a large one, more likely in a population that is new in its niche than in one that is well established, and more likely to come from a larger group of replicates than from a smaller group. Hence group fission provides more trials, more recombinations of cultures and population structure, and more combinations of cultures and niches, all of which will increase the probability of cultural change. None of this is to deny countervailing forces that facilitate change in larger and denser social systems.

A local breeding population somewhere along our line of ascent (for instance, see Chagnon 1980) might, under the vagaries of existence, decline in size, in which case its members might disperse to join other similar groups, or its remnant might be absorbed in some other group, or it might become extinct. None of these outcomes is particularly interesting in an evolutionary sense, although absorption dampens the process of differentiation and speciation, while extinction accelerates the development of lacunae that separate species. If such a population were instead to increase in size and press against its resource boundary, what might its responses be?

It might exhaust its resources, then decline in size and take one of the paths just noted. Alternatively, it might develop some innovation that would permit it to exist at the new, higher population density or revert to the previ-

ous density. These innovations are of two kinds: one is the other side of the Malthusian problem, namely, some mechanism for the reduction of fertility, whether prudential or vicious (that is, a demand-side solution). The second is some mechanism for increase in resources (that is, a supply-side solution). These latter are all more or less Boserupian in principle, but they can include anything from walking a little further each day or conquering one's neighbors to inventing new tools and techniques. But the most interesting outcome would be fission. If there is not enough to eat, some people go off to find more, and they may or may not come back. All animals range more widely in the face of diminishing food supplies, and people tend to do so in familial groups that can form the core of new populations.

What are the consequences of fission? The first is that the daughter groups are smaller than the parent group. As such they are more subject to the vagaries of chance; their demographic rates become unstable simply by virtue of their smaller size, so that they are more likely to begin precipitous declines or sharp accelerations in size. If groups decline in size, they can of course become extinct, thereby creating gaps in the network of communication that increase physical and cultural differentiation among those that survive. They can also rejoin the parent groups or combine with some other group. Any such fusion is facilitated by the ability to communicate, especially if the separation has lasted more than, say, a generation or two. The development of cultural symbols, language in particular, facilitates either recognition and reincorporation or diplomacy and fusion. Symbols are thus hedges against extinction (and speciation). Language is important for more than intragroup interaction, and the groups that developed it had a better chance of not disappearing as victims of mere random fluctuation in their demographic rates. Kinship systems, permitting differential claims on the resources of others, are a special form of symbolic system facilitating these processes. Social anthropology in its functionalist form has a lot to say about this and has had since McLennan (1865).

A second consequence of fission is that smaller size combined with geographical separation makes it harder to find mates, which condition may contribute to the reduction of growth or, alternatively, increase the probability of cultural change in rules of mate selection or the development of kinship nomenclatural systems ensuring at least one incumbent in each named kinship role (Hammel 1960).

A third consequence of fission is that daughter societies will not be perfect copies of the parent society. Not for a very long time (if ever) has every adult in a human group known exactly as much as every other. On fission, some knowledge may be lost. It may of course be regained by reinvention, but this may be as unlikely as a repeat mutation recovering a lost biological characteristic. (Since people began to think systematically about problem-solving algorithms, the probability of repetition of identical solutions has been enhanced.) Lost knowledge may be regained by borrowing or fusion with other

groups, but these processes will involve some of the same mechanisms for diplomacy. Most interesting, new solutions for old problems may emerge. Some of these may work worse than earlier solutions, but some may work better. In fact, some may overcome the resource limitations that earlier forced fission. One can think of a model in which economies of scale in large populations produce new ideas in segments of that population, ideas that can be implemented only with difficulty in the face of traditional conservatism and interest-group conflict but are then liberated for experimentation in small daughter groups.

The upshot of this is that fission does two important things. It increases the survival value of fundamental human characteristics such as intelligence and language, because these characteristics permit amelioration of isolation and hedge against downside demographic risk. Further, it permits daughter groups a new chance at breaking the resource barrier with a somewhat different combination of biological and cultural resources. Reproductive success that pushes societies against the Malthusian boundary is productive in an evolutionary sense not only because someone occasionally has a bright idea but because a possibly common response, fission, multiplies the number of semi-independent non-identical trials for success.

Core-Periphery Population Development

Some important differentiations in this hypothetical process must have taken place even at the beginning. Imagine, for convenience, an ecologically homogeneous plain of vast extent, occupied at its center by an imperfectly but reasonably well-adapted species which expands against its local resource boundary. It continually divides, like a colony on a petri dish, and daughter groups surround it like rings on a pond. Suppose that daughter groups on the periphery experience no changes in technique. Indeed, all that the people on the periphery have to do is walk centrifugally and breed. At the core, it is different because there is no place to walk. The environments in which these otherwise identical or randomly differing groups live on this ecologically homogeneous surface become different in a density-dependent way. There will always have been a difference in ease of movement from the periphery toward the core as opposed to outward from the core and through the periphery if there is any density gradient along a radius from the core.

Variability becomes important toward the center in two ways: first, it allows failures to emerge and go out of business, making room for other groups. Second, it allows better solutions to emerge by the processes already mentioned and these to spread to other groups through communicative mechanisms. Under these circumstances the rate of culture growth will be higher at the center (or, if there is a density gradient, just closer to it) than on (or toward) the periphery simply because some variations have better adaptive consequences than

others and because selection pressure is greater at (or toward) the core. This is a density effect but a different kind from some others that may also play a role. For example, even if the number of intelligent persons per capita in a population does not increase, the number of these per unit area is greater in denser populations. Thus, increased density may increase the rate of invention. Further, the chance that a new idea will take hold is enhanced by density, since the chances of transmission to others are increased. There are, of course, countervailing density-dependent resistances to change. For example, it is reasonable to speculate that increases in density accompanied by increase in hierarchy and interest-group formation may increase cultural conservatism, time lost in diplomacy, and a variety of other kinds of makework. Parkinson (1975) and Murphy (Block 1977) need to be included as sources of major theoretical enlightenment along with Malthus, Marx, Boserup, and Weber (1947).

There are a number of speculative consequences of gradual expansion and higher densities at the cores of areas beyond the idea of higher variability and innovative cycling at the core. The first of these is the development, through the encounter with new ecological conditions as the periphery expands, of new cores and thus the basis for regional systems. The second is the establishment of differential access to the same resources within a given area, through hierarchicalization or other differentiation of the social structure (such as ethnic or caste group specialization). The third, related to the last, is conquest. Under any of these, trade or exchange, equal or unequal, may operate. In all of these circumstances the resource boundary limit remains the same but is overcome for a larger population through some kind of differentiated redistributive scheme. Of course, stratification need not lead to increased efficiency. It can lead to an increase in the standard of living for the elite, at the expense of the mass, with no net change in overall per capita consumption (total resources per capita). At the same time, consolidation of political control can raise the productivity of labor and decrease the wage, for a net increase in product, as a result of simple exploitation. Differentiated social systems create new kinds of niches within which pressures for different kinds of demographic behavior may operate. The conditions are laid down under which human reproduction, in the pursuit of perceived self-interest, leads to higher or lower fertility in different subgroups of the same population or at least the same society. Other consequences are differential mortality, nuptiality, and migration.

One of the most important consequences of the process of group fission, as here conceptualized, has been the emergence of elaborate symbolic codes for identification and communication. These have more or less overcome isolation, less earlier and more later. One direct consequence of increased communication was an increased probability of epidemic disease. Increases in density following on successful technological innovations also facilitated the spread of epidemic disease, as did the maintenance of and close proximity to dense popula-

tions of animal hosts acting as reservoirs for some diseases. Cattle carrying tuberculosis and plague-carrying fleas living on rats eating stored grain are examples. Mortality patterns since the development of increased densities and enhanced communication may be very different from those that prevailed during most of human history. This makes the use of modern life tables very problematic outside modern contexts (Lovejoy et al. 1985).

But the most important effect of increased communicative ability has been the emergence of mass culture, a process that has been going on for longer than we complain about and perhaps from time immemorial. Currently, starving children in Ethiopia are featured "live" on the evening news, and rock groups organize systematic intervention into the mortality process of a geographically and culturally distant population. Any increase in communicative efficiency damps the isolation whose very nature gave rise to the intensifying effect of multiple tries at ecological solutions. Indeed, we now approach the situation in which questions about the global human population make good sense. Disruptions in one part of the earth may impinge immediately on all of it, today, whereas only a short time ago they would never do so immediately and might only rarely do so in the long run. Economies of scale in the development and distribution of innovations that overcome resource barriers are now surely more important than the independent invention of solutions in local groups. The multiple tiny world systems of the earliest human beings that were ultimately eclipsed by the more ambitious partial world systems of early empires are now approaching coalescence into a larger one, so that it is appropriate that demographic phenomena are a topic for concern in the forum of the United Nations. Many factors, from the Green Revolution to organizations like Oxfam, decrease the importance of locality. The mechanisms of evolution, driven by demographic process, have transcended themselves as processes of competition between allopatric segments of the species, insofar as the species becomes communicatively homogeneous. While Malthus and Boserup could have been right in the long or short run for local societies, they are now more likely right in the long or short run for all of mankind together. Marx's conclusion that the fundamental basis of modern demographic differentiation is social and based in distribution of resources is brought closer to being the whole truth in just the same measure that the world is one.

Research Possibilities for Theory Building

PRIMATE POPULATION STUDIES

One of the attributes of Old World primates, and thus probably of our ancestors, is that females mature sexually and otherwise earlier than males. Generally, and given premodern mortality schedules, more of an average female's life-span is reproductive than of an average male's. This difference is increased in any social system that delays the access of physically mature males to reproductive females, and some such systems have devel-

oped among many primate species. The social delay in male maturation has no necessary effect on the reproductive potential of a group. The time difference in physiological maturation between male and female would not have had any necessary effect either if it had occurred through slower development of males. If it had occurred through accelerated development of females, it would have added reproductive potential, given no change in life expectancy or timing of menopause (Hammel 1976, Lancaster 1978). Is this kind of change at the bottom of the dryopithecine expansion? An adaptation in which females mature early in times of food plenty and more slowly during periods of scarcity would be a particularly convenient arrangement, as is suggested by Frisch and McArthur (1974), and the resolution of that question by empirical research will provide important insights into prehistory (Scott and Johnson 1985). Solid empirical studies of primate populations are still scarce but are an accepted goal of the field (Burton and Sawchuk 1981, Cutler 1976, Dolhinow 1984, Hamburg and McCown 1979, Hrdy 1977, Sade et al. 1985).

PALEODEMOGRAPHY

The empirical bases for answering many of the questions we want to ask about prehistory will necessarily come from paleodemography, a field which is going through extensive revision (see Acsádi and Nemeskéri 1974, Bocquet-Appel and Masset 1982, Hassan 1981, Howell 1976, Petersen 1975, Van Gerven and Armelagos 1983). Many of the issues in this currently lively field are methodological, scholars finding sufficient fault with methods used in the past that the usefulness of many earlier findings has been questioned. With a new generation of scholars asking more specific questions and treating their available data bases with more rigor, a new generation of studies has emerged (Buikstra and Konigsberg 1985).

Results from empirical studies based upon the analysis of archeological and skeletal data will have to await resolution of pressing methodological questions (Weiss 1973). New techniques for aging and sexing skeletons (Lovejoy et al. 1985), new methods of data collection and data reduction that facilitate an increase in completeness and reliability of the empirical base (Lovejoy 1971), and new investigations of the distribution of causes of deaths in isolated populations all promise to contribute to the value of paleodemography for the kind of population reconstruction we are considering here. Indeed, it is the increasing promise of valuable data from paleodemography that encourages us to believe that the kind of ambitious model proposed here will eventually find empirical clarification (Mann 1968). Special topics in paleodemography, such as the effects of status, infections, and diet on the skeletal population (Buikstra and Mielke 1985), are of particular interest, even if eventual changes in the methods of aging and sexing skeletal materials require reanalysis of some of the raw data. Consequences of cultural traits for paleopopulations have been almost entirely ignored (but see Hammel, Wachter, and McDaniel 1983, Isaac 1978).

HUNTER-GATHERER POPULATIONS

During the past decade the field of demographic studies of hunter-gatherer societies has increased from almost nonexistent to the point that a few hunter-gatherer societies have been well described, a few have been well studied and partially described, and rich materials are anticipated to be available during the next decade. One of the results of these studies has been an appreciation of the complexity of the population structure of even the smallest and simplest groups. Observations which have emerged as central include the extent to which hunter-gatherer peoples restrict their population growth and the extent to which the restriction is conscious and deliberate. The low fertility adaptation observed among the !Kung (Howell 1979) seems to be common among hunter-gatherers (Hastings 1978), but there is much to learn about its frequency and the mechanisms by which it is established and maintained. We need to know whether the mortality patterns of hunter-gatherers tend to be distinctive from those of peasant populations, perhaps because of differences in population density and exposure to infectious diseases.

The increasing body of studies also serves to warn us (Schrire 1984) not to oversimplify the definition of hunter-gatherer societies or to exaggerate the differences between them and food producers (Testart 1984). Many hunter-gatherer societies have tried food production to some extent at times in the past. Many regularly trade collected or hunted products with food producers, so that their occupational specialty resembles wage work or cash cropping in some respects, and few if any have been completely isolated either from other populations or from non-hunter-gatherers. The niches occupied by hunter-gatherers vary widely, from the Arctic to the tropics, in both obvious and subtle ways (Cashdan 1983).

Studies of hunter-gatherer groups, contemporary and historical, that include the dynamics of the group over a period of time as long as a generation are needed. To a standard demographic reconstitution we need to add quantitative assessment of diet, energetics, decision making, contact with outsiders, and so on.

DEMOGRAPHY OF FOOD PRODUCERS

The "origin" of food production by agriculture and herding around 10,000 years ago is obviously a central event in any account of population history (Cavalli-Sforza 1983, Clark and Brandt 1984, Cohen 1977, Cohen and Armelagos 1984), but we continue to urge caution about overgeneralizing. The model of Ammerman and Cavalli-Sforza's (1984) continentwide framework for incorporation of data over centuries is a major source of our optimism that the time is ripe for developing a model of responses to population pressure (see also Sanders and Price 1968). Similar attempts to look at the distribution of population and cultural traits continentwide over generations and centuries may now be in order.

With the development of food-producing societies, we find the phenomenon of populations growing stably over substantial periods of time. During the few centuries

after the origin of cultivation as a regular basis of life, we certainly see increased size of social units, the development of much larger political units and much larger communities than previously (Spooner 1972). Indeed, this historical period produced urbanization, increases in the scale of trade, differentiation of urban and rural social structure, development of status groups, and differences in access to the means of production that had not been regularly seen before (Skinner 1977). It is not clear, however, whether the distribution of the size of breeding populations changed very much during this period (Netting 1981). With food production, societies became regularly much larger than breeding populations, and populations tended to lose their geographical integrity. Holding of property by kinship units and eventually by individuals complicated social structure and motives of action. The extension of contact between populations and even between societies by trade, travel, and regular communication similarly complicated the "locus" of culture.

DEMOGRAPHY OF NATIONAL-LEVEL INDUSTRIALIZED SOCIETIES

The transformation of demographic regimes known as the "demographic transition" started some 250 years ago and has been proceeding apace ever since. It remains unclear whether any of the societies involved have "completed" the transition in any real sense, but it is obvious that many societies are still changing rapidly (Teitelbaum 1975). The form of the transition has been a drastic reduction in mortality, from typical average life-spans of 30–50 years to spans of 60–80 years. In many cases this change has been well under way before fertility starts to decline through the adoption of contraception within marriage. Typically the decline in fertility is from some 6–8 children per couple to 2 or fewer children per couple (on the average).

The trend toward extreme differentiation within and between populations that has characterized population history for over 10,000 years tended to be reversed in some interesting respects during the industrial period. We note generally an increase in the permeability of population boundaries, perhaps as a consequence of the ready access of individuals to cultures other than the one they are born into through mass education, mass culture, and mass communication. In some ways, industrialized populations resemble hunter-gatherer populations more than peasant or agricultural populations in respect of individualism, control over self and family, and freedom to try cultural varieties if available.

THE RESEARCH AGENDA

To construct an operationalizable theory of the coevolution of human biology and culture through demographic functioning, the research community may wish to organize available research results in new ways. Following Ammerman and Cavalli-Sforza (1984) in their exemplary study of the spread of agriculture in Europe, we see a need to examine the results of studies from a range of

disciplines on an organizing canvas as large as a continent, with observations arranged by centuries, including not only a target population isolated from the rest of the world but all populations in the population of populations as they influence one another, provide reservoirs of variation and sources of change, and create the environment in which they and others live.

We expect that databanks organizing observations from a range of scholars in a range of specialist subfields will be necessary to construct and test the kind of theory we have in mind. We anticipate that simulation, both microsimulation and macrosimulation, will provide a framework within which observations can be used to estimate the parameters and check the implications of observations, permitting both reconsideration and occasional correction of the empirical base and the production of null hypotheses for the continent and time period used as base and predictive hypotheses for other places, other periods. We anticipate that such simulations, always simplifications of reality, may become dauntingly complicated when the range of questions considered here is admitted to them, just as investigators continually find that detailed study of particular societies becomes more complex when more is known about them. Nevertheless, the construction of such a theoretical framework would seem to be possible and timely on the basis of the state of the field at the present time.

Comments

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The integration of biological and cultural evolution is a challenge to the human intellect limited by the current stage of development of science. However, one has to begin to approach this problem, since the isolated understanding of cultural and biological evolutions is unsatisfactory. The selection of demography as capable of unifying the subdisciplines of anthropology and untangling the multiple knots of coevolution seems to result more from the prevailing influences of great thinkers (Malthus 1976, Marx 1967) than from its inherent power. Moreover, while the high technical quality of the data from demographic studies today assures the fidelity of the information, it neither contributes to a better understanding of the past nor guarantees that demography is the right vehicle for us to ride on the roads of evolution. Only the results of the possible research for theory building will settle the matter. Looking ahead, I would venture the opinion that the data compiled will add to normal science (Kuhn 1978) but will be far from revolutionary with regard to our knowledge of evolution.

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Aspects of the presentation of Hammel and Howell seem hauntingly familiar to me—in particular the projection of human responses to perceived population pressure and the notion that those responses to scarcity are instrumental in societal evolution (cf. Dumond 1972). Rather than pursue parallels, however, I limit this statement to two differing but articulating points. One identifies a seeming paradox within the presentation; the other suggests an alternative conceptualization of a change-generating mechanism that can be used in more situations than can the notion of population pressure and that may contribute toward resolution of the paradox.

1. Hammel and Howell open with a persuasive statement that the dynamic of population change lies in individual breeding units rather than in populations writ large and that the focus of analysis must be these smaller units of the whole. But their ensuing relates only to such units that grow or otherwise experience scarcity, leading one to conclude that only those contribute to evolution in any sense. If this be so, what is the advantage of focusing upon the smaller units rather than the larger, since on the whole and through time the latter have grown—thus showing the only characteristic that is important? It is only if the contribution to evolution of the smaller units cannot be predicted from that of the larger that the smaller need be studied. The argument must be, rather, that contributions to evolution are also made by populations that undergo fission or otherwise change without increase in density and before stimuli other than perceived scarcity.

2. The confrontation of scarcity by means of altered behavior is a subclass of phenomena that may be termed the exploitation of differential opportunity. While some such opportunities may be mitigating paths taken in the face of a deteriorating present situation (scarcity), others may be seized in circumstances in which there is no deterioration of existing conditions and no perception of scarcity.

For example, some fissioning, migratory movements—the drive of ancestral native peoples across northern Canada around 2000 B.C. and again around A.D. 1000, the expansion of Euro-Americans within North America in the 19th century—can be seen as generated not by scarcity but by the promise of new and richer resources to be gained with less effort in the out beyond. But those movements occurred only as demographics supplied population sufficient for geographic expansion (without increase in density) with an accustomed productive system, thus permitting unsettled areas to be treated as a familiar, if drastically enlarged, ecological niche.

It is also likely that recent examples of subsistence intensification and expansion are related as often to money and markets as to scarcity at home. The intensification of rice culture in the region of Thailand

treated by Hanks (1972) came about as a response not to internal pressure but to the presence of a rice market in Bangkok. And yet it occurred only through redistribution of the population and (evolutionary) change in organization, and the whole could be described by Hanks as a Boserup-like linkage of population and agricultural regime. This is "scarcity" in an economic sense but not in a personal, perceptual one.

In these cases the demographic conditions may be more permissive than coercive, but they are crucial even without the driving mechanism of increased population density. With this perspective the end sought by Hammel and Howell should still be served.

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Hammel and Howell propose extending evolutionary theory in ways such that "demographic events are the central mechanism and leading indicators of the coevolution of bodies, minds, and societies." I applaud and agree both with the thrust of their argument and with nearly all of the specifics. I wish to emphasize a point that remains largely implicit in their presentation: the behavioral and biological change for which such a theory must account entails change in system states (see Handwerker 1986b:24-27).

To date, anthropologists (like sociologists and economists) have largely been content to analyze particular system states, and perhaps to contrast different system states (e.g., traditional and modern societies or bands, tribes, chiefdoms, and states). We have focused on micro-level investigations and have produced explanations that take the following form: (1) If people make assumptions A_1, A_2, \dots, A_n (2) within the set of constraints C_1, C_2, \dots, C_m (3) they conform to behavioral pattern B. Quantum improvements in our understanding of demographic phenomena have not been forthcoming because micro-level analyses, whether relying on formal optimization models or informal descriptions of cultural context and decision making, cannot address the most important issues: (1) on what assumption or assumptions (consciously or unconsciously) do people generate behavior, and why? and (2) how and why do changes in constraint specifications come about? Because changes in system states come about only when there are changes in assumptions, constraint specifications, or both, the central task of the theory building to which Hammel and Howell direct our attention must be to answer these two questions.

With respect to the first question, the most common model assumption is that people are optimizing creatures. But what do we optimize, and why? Is cultural/behavioral diversity explained by the optimization of a wide range of variables or by the optimization of one or a small range of variables in the context of constraints that are unique to the life history of the individual or population? To construct a theory, rather than an analyt-

ical framework, requires that we explicitly identify variables the optimization of which can account for the differential reproductive success of one or another biological and/or behavioral characteristic.

With respect to the second question, is change in constraint specification a random (historically unique) process, or is it nonrandom (perhaps even directional), determined by prior demographic, economic, or environmental states? To phrase the issue in these terms means that, in the theory to which Hammel and Howell direct our attention, and as is implicit in much of their discussion, demographic phenomena are unlikely to be the central mechanism of change. Almost certainly, that "mechanism" will be ecological interdependencies. These interdependencies will include but will not consist solely of demographic phenomena, and they will reflect not merely relationships among environmental, technological, and demographic parameters but also power differentials within or between populations (see, e.g., Handwerker 1987, n.d.).

But what of innovation and individually unique decision making, which contribute to the creation of phenomena that have never before existed? To what extent (if at all) do such phenomena determine macro-level changes? Can we develop explicit, formal models that identify macro-level determinants of micro-level changes the effect of which is to alter macro-level constraints?

Finally, of course, evaluating these and other possibilities requires that we clarify interdependencies between biological and cultural phenomena and between biological and cultural evolution. What, specifically, are the linkages between our genome and cultural behavior, and how do they operate? How, specifically, does selection operate, at what organizational levels, and with what effects? Answering these questions implies a major shift in research strategy. Not only, as Hammel and Howell suggest, might we organize research or information differently, but we need to develop explicit models of the manner in which phenomena at different organizational levels articulate over time.

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This article is an excellent synthesis and statement of prospects for anthropological demography. I do have some reservations, but the following criticisms should not be taken to deny my general admiration for and agreement with it.

First, Hammel and Howell state early on that specifications of evolutionary theory for humans have been "deterministic and teleological." But the incorporation of stochastic models into evolutionary theory is an old accomplishment and widely acknowledged by evolutionists as necessary to explain and understand gene distributions. Evolutionists may write deterministic equations, but they all understand how stochastic processes

modulate their results. Evolutionary theory, as I understand it, is hardly ever teleological, although I would argue that it should be when applied to an organism with foresight and intelligence.

Second, their use of "population pressure" is troubling, since their discussion seems to indicate that they would include anything that induces culture change. But, if population pressure is effectively synonymous with culture change, we are left with words chasing each other in circular definitions. They might write a hypothetical dynamic equation and identify the term or terms that encapsulate what they mean by pressure.

Third, the meaningful definition of a population seems to me to be a hopeless task, except in the case of isolated islands. The 80% rule is arbitrary, and if we pick an individual and draw an 80% circle around him, then we are faced with the uncomfortable arrangement that everyone belongs to a different population. An empirical consequence of this difficulty is that anthropological demographers sometimes can be more confident of the numerators of their calculated rates than of the denominators.

Finally, talking about populations doing things leads subtly to thinking that there are regularities about the behavior of populations. There are at least two problems with this. In sciences with laws reflecting large numbers of interactions at a lower level such as Boyle's law of gases, the particles are simple equivalent replicates of each other or nearly so. Humans are all different, they compete with each other, and they evolve. Our grounds for faith that such particles will behave in replicable ways in the aggregate are scant. Further, the implicit assumption in talk about population adaptation is that food is the prime scarce resource driving cultural evolution. In fact there is debate in anthropology and in evolutionary biology in general about the relative salience of food and sex as driving concerns. Insofar as Marx was talking about consequences of male competition, he is given short shrift in favor of Malthus and Boserup.

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The authors argue for the importance of the study of demography for anthropological theory. In my view, demographics should be treated in introductory textbooks on a par with more traditional subjects such as economy, domestic organization, or religion (Harris 1987:89-105). Despite the shared enthusiasm for anthropological demography, I cannot express much enthusiasm for the authors' proposal to make demographic studies the basis for integrating theories of biological and cultural evolution. No one would disagree that "demographic regimes" [sic] are "important causes and effects" in biological evolutionary processes. But as the authors, implicitly at least, acknowledge, patterns of demographic growth are caused by and affect other basic sociocultural processes involving, for starters, technological, eco-

nomical, and environmental variables. The crucial conceptual problem therefore is how to combine the study of demographic, technological, economic, and environmental variables into a coherent research strategy. The authors do not concern themselves in any general systematic sense with this problem, although they do discuss some aspects of the interrelationship of demographic processes with technological, economic, and environmental factors as well as some demographic interrelationships with political and symbolic-ideological systems. The most glaring symptom of the authors' paradigmatic ambiguity is their failure to deal with the concept of mode of production (in either the Marxist or the cultural materialist sense) and with the various efforts that have been made to demonstrate the causal priority of modes of production and modes of reproduction (the demo-techno-econo-environmental infrastructure) in relation to the structural and superstructural elements of sociocultural systems (Harris 1979, Ross and Harris 1987). Another expression of the same theoretical shortcoming is their choice of breeding populations as the basic aggregate of human actors upon which and through which the processes of biological and cultural evolution operate. While certain demographic and genetic problems are best approached through breeding population isolates, the integration of diverse anthropological interests cannot be achieved on such a basis. Sociocultural systems have seldom coincided with breeding populations. Throughout most of prehistory, human breeding populations extended beyond the boundaries of sociocultural systems, while with the development of the state, sociocultural systems came to embrace several different breeding populations. If the goal of nomothetic anthropology is to account for the divergent and convergent evolution of sociocultural systems, it is the sociocultural system—people plus infrastructure, structure, and superstructure—which must be the basic bounded aggregate and not breeding populations, whose basic demographic rates cannot be understood apart from the entire infrastructural conjunction and the latter's feedbacks with structural and superstructural factors. Incidentally, there is no evidence that the bounding of empirical sociocultural systems presents any greater methodological difficulties than the bounding of human breeding populations, especially if Adams and Kasakoff's 80% rate of inbreeding is to be the operational definition.

Turning to specific population-related issues, the authors provide some useful summaries and research agendas. It is regrettable, however, that their overview of anthropological demography does not reflect a growing realization that basic demographic rates (fertility, sex ratios, mortality, and population growth) and their ideological accompaniments throughout prehistory and into modern times can best be understood in terms of the variable costs and benefits of rearing children in sociocultural systems characterized by specific infrastructural and structural conditions (Ross and Harris 1987). This formulation largely supersedes much of the authors' discussion of the role of scarcity as an explanation of demographic rates, provides currencies by which

cultural selection can be distinguished from natural selection, and thereby links demographic theory with theories that account for the divergent and convergent trajectories of sociocultural evolution (Birdsall 1983; Caine 1977, 1983; Caldwell 1982, 1983; Coontz 1957; Handwerker 1986a; Harris 1984; McKee 1984; Mamdani 1973; Miller 1981, 1983; Nag and Kak 1984; Nag, White, and Peet 1978; Scheper-Hughes 1984; Scrimshaw 1983; Skinner 1985; Sussman 1972; Tilakaratne 1978; Vining 1985; White 1973, 1982; Wood 1982). The failure of the authors to consider this growing corpus of facts and theories suggests that we still have a long way to go before demographic anthropology gets its own act together and can present itself as a model for integrating our fractious profession.

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Hammel and Howell's paper is a valiant effort to deal with the biological and cultural aspects of demography, "to place the development of human demography and coevolved culture squarely within the framework of biological evolutionary theory." Our reaction is to hope that this article marks a recovery from the polarization stimulated by Wilson's *Sociobiology* (1975), a breach that has stifled discussion by members of junior cohorts who were not loyal to the sociobiologists' camp. Applause is due Hammel and Howell also for setting the record straight in regard to the population as the biologically evolving entity but not the decision-making unit. Finally, their treatment of the concept of "resource boundary" is instructive and should prove useful in demographic anthropology.

From the perspective of a biological anthropologist, the major difficulty in Hammel and Howell's proposal lies in their approach to culture change. Their formulation does not clearly recognize that cultural evolution becomes a metaphor when placed "within the framework of biological evolutionary theory." For example, the concept of "copying errors," while appropriate for characterizing mutation, misconstrues culture change. Humans seem to be incapable of the exact cultural replication implied by "copying," and cultural variation does not inherently signify "errors." In fact, variation is as fundamental to culture as "copying" is to genetics. Support for this statement is widely available, ranging from studies of language acquisition—the first sentence uttered is not a copy of someone else's sentence—to ethnographic observations. For example, in central Mexico, one of the authors of this comment (Millard) observed that each woman's tortillas were regarded as distinctive, and women also noted that their tortillas varied from day to day, but none were seen as incorrect. Thus, characterization as "copying errors" misconstrues cultural variation in fundamental ways. The other side of the simile of cultural variation as "copying errors" is the authors' concept of cultural stability, seen as analogous

to genetic replication without mutation. However, even the most stable cultural traditions represent outcomes of dynamic, context-dependent layers of cultural creation and transmission, unlike the programmatic replication of genes.

A second instance further illustrates confusion from failing to recognize the metaphoric aspect of cultural evolution when placed in a biological evolutionary framework. The authors predict more rapid culture change in populations that are smaller and newer to their environments, a characterization seemingly drawing on Wright's work (e.g., 1978) on random genetic drift and selection. While many researchers note the place of culture change, we wonder whether anyone's sense of it is clear enough to substantiate cross-cultural comparisons. Existing measures of culture change are few, none as precise as measures of change in gene frequency. Certainly a phenomenon of nuclear winter, produced by state-level decisions, would bring about the most rapid culture change that the world has seen, if not extinction (i.e., biological evolutionary change).

The general approach advocated by Hammel and Howell, the use of demographic analysis in examining the coevolution of biology and culture, certainly merits further attention. The task should be approached carefully, since, as the instances above illustrate, theoretical borrowing from biology can occur in disregard of important differences between organic evolution and culture change. It is an inescapable thought that the metaphor of "cultural evolution" has singular power in our times because of the ascendancy of science in the latter half of the 20th century. As others have noted, biological evolution is a widespread commonsense model in social science (e.g., Giddens 1984). Approaches by population geneticists that show some awareness of the resulting problems range from Fisher (1930) and Haldane (1932) to more recent work including Williams and Williams (1974), Kaplan (1976), Reid (1976), and Cavalli-Sforza and his coworkers (Cavalli-Sforza and Feldman 1981). In demography, the work of Howell (1979) herself is a sterling analysis of population dynamics in small groups and can serve as a model for anthropological approaches. The issues presented by Hammel and Howell in population dynamics and culture change are intriguing and remain formidable challenges worthy of more attention at this early stage of development of demographic anthropology.

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In principle I am basically in agreement with the ideas expressed by Hammel and Howell; my comments, therefore, are going to be brief, their main purpose being to call attention to a large number of papers that are being published in this area on South American Indian populations.

The first point I want to make is that, as is mentioned by the authors, the hunter-gatherers/food producers dichotomy is an oversimplification. At least in my continent, hunting and gathering is always supplemented by some form of agriculture, and this should be kept in mind in any extrapolation from extant to prehistoric populations. Secondly, in my opinion the category "food producers" is too inclusive. The population structure of pastoralists probably differs in important ways from that of agriculturalists. The type of land exploitation is basically different in the two cases, and pastoralists may be more mobile, which would place them in an intermediate category between groups engaged mostly in hunting-gathering or in agriculture. Finally, another point of similarity between industrialized and hunter-gatherer populations is their fertility patterns. Hunter-gatherers controlled their family size in a much more rigorous way than agriculturalists, since numerous children would be a source of embarrassment in wanderings in the jungle but a valuable help in agriculture.

Population structure studies among South American Indians have been manifold, and a review of them is being presented elsewhere (Salzano and Callegari-Jacques 1986). Relatively extensive information is available about their basic demographic parameters (age and sex distribution, marriage patterns, mobility, fertility, and mortality), and this empirical information has been compared with the results expected under various models. Attempts at improving these demographic data have also been made, using such procedures as urine tests for pregnancy and physical examinations to check the reliability of reproductive histories or the application of serial censuses (Neel and Weiss 1975; Black et al. 1978; Gage, Dyke, and Riviere 1984a,b). Questions about the genetic effects of fission in general or the differences between random and lineal (involving genetically related individuals) fission have been considered by Thompson (1979) and Smouse, Vitzthum, and Neel (1981). Computer simulations have also been used to estimate the probability of survival of a neutral mutation (Li, Neel, and Rothman 1978) or the inbreeding coefficients for alleles present in the founding population of American Indians (Spielman, Neel, and Li 1977). Thus multidisciplinary studies of the type proposed by Hammel and Howell are already being conducted among Amerindians.

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When viewed as a paradigm to be tested by much more research, the paper by Hammel and Howell successfully challenges us with "a philosophically acceptable extension of evolutionary theory, one in which demographic events are the central mechanism," and "a framework that will work equally well for protohominid bands and the 'world system' of modern market capitalism." They

enumerate and elucidate the many assumptions about "local breeding population" used so often in studies over time and space. Their paper complements a proposal by Paul Baker to recognize human population biology as a viable transdiscipline; he observes that "the wedding of demographic and traditional population genetic theories offers new perspectives on evolutionary processes" (Baker 1982:212). Both papers synthesize methods, de-emphasize disciplinary boundaries, and suspect attractive theories which may have "exceeded the extant data base for testing" (Baker 1982:212). Both emphasize the study of local populations for comparative purposes and therefore recall the empirical value of inductive case studies moving slowly towards theory (though sometimes not fast enough for the theorists).

The reader may get the impression that population fission is the usual alternative to adaptive pressure. It is listed first in the "Responses to Population Pressure" and then discussed under its own heading, "The Fission Alternative." As the ideological artifacts of the concept of "linear progress" in Euro-American culture, growth and development dominate population thinking in anthropology. Expanding populations are easier to study in the field or in the archaeological record because, epistemologically, we know presence better than absence. But Hammel and Howell balance this notion, because they "do not assert a unidirectional system." They appreciate that population decline and extinction are possible, if not likely, events of some evolutionary consequence (cf. Gallaher and Padfield 1980). Their research agenda for core-periphery development should add the historical study of marginal communities on the edge of early industrial centers; there are instructive examples from Europe, especially in the United Kingdom (Cohen 1982, Pratts 1979, Stevenson 1984). If we are to understand the dynamic present in the sense of Hammel and Howell, we might strive to measure the carrying capacities of nation-states (Maserang 1976) and evaluate the demography of local habits and history within the emerging world community (Sheets and Kelly 1987).

Reply

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We are grateful to the commentators for their responses, which help us identify the areas where we were unclear or excessively terse and those where we simply disagree. In our response we expand on some of these. We point out, as in the original, that our presentation is far from being a theory but is only a beginning. We address the comments of each reviewer in turn and conclude with an attempt to clarify our perception of evolution.

Azevêdo: We hold out no hope of revolution in anthropology. Some gradual reform would be enough for

us. Azevêdo is betting that our approach will not be productive but does not convince us we should not try.

Dumond: We acknowledged that our arguments were synthetic and did our best to absorb and cite the relevant literature. Hence we are gratified that Dumond finds our argument "hauntingly familiar" and trust that the same could be said by the authors of many other papers not cited. The paper that he cites is close to our account in spirit but not in substance.

The query is why we should study small units. Our distinction is between populations as breeding populations and the global human population, not between large and small populations as such. Readers will recognize this as the same distinction suggested years ago by Steward (1953) between multilinear and unilinear or universal evolution and an insistence on the history of actors rather than the history of categories. We also state explicitly that population pressure is not the only root of change; it is simply one to which we wish to draw attention.

In respect of Dumond's second point, we would agree that perception of opportunity is a potential cause of culture change. Indeed, under our broad definition of "scarcity" it is exactly the perception of insufficiency that is the driving force. This point is not new in population theory, not to mention social theory, and was explicitly recognized by Malthus in the later editions of his *Essay*, as we note. If that perception is generated by a cultural propensity to maximize, the search for opportunity can become pervasive and satisfaction can be only ephemeral, as we again note. We would not agree that the expansion of Europeans to and within North America was unrelated to scarcity, at least scarcity in our terms. It was resource insufficiency that sent many Europeans to North America in the first place—sometimes insufficiency at a minimal subsistence level (the Irish), sometimes insufficiency with respect to desired wealth (some English, some Dutch, some Germans), and sometimes a political insufficiency in respect of established religion (various Protestant sects, although the action of economic forces in these movements ought not to be denied too soon).

We also question whether the perception of insufficiency may not have been a factor in the expansion of Amerindians across northern Canada or indeed into the New World. The choice for a hunter-gatherer group is whether local resources are good enough or whether the hunting is better or easier further on. Even if the desire is to generate the same amount of food for less work and more leisure, so that what is scarce locally is leisure, not food, increases in human population density or decreases in resource density can inspire migration. In general, we cannot agree that "economic scarcity" is not also personal and perceptual; if anything, we insist in our presentation that definitions of scarcity be culturally based and not externally imposed.

Handwerker: We recognize the cogency of Handwerker's comments on changes in system states and agree that we did not carry the theoretical formulation to a formal recognition of those relationships. We make the

wry observation that the complexity of the theory necessary to approach these problems is a function of the complexity of the social organization of the society in the context of which it is applied, and the amount and quality of the data necessary to test the theory are currently also a direct function of that complexity. This means that we have the worst data for the simplest theory. Because we were thinking about a theory that could relate demographic to evolutionary approaches, we quite clearly swung toward the hunter-gatherer pole, and we wonder, in spite of our enthusiasm, whether we can ever find a set of data sufficiently good to test a theory that is sufficiently simple to allow agreement about its principles. We just hope so and are prepared to try.

Harpending: We agree that statements about the evolution of *H. sapiens* and of culture, especially the latter, take scant advantage of modern concepts of evolution. This is our complaint (although politely disguised). It is not modern evolutionary theory we criticize but most anthropologists' and especially social-cultural anthropologists' use of it in arguing either for or against evolutionary approaches (see also our response to Millard and McBride).

We define pressure as an unfavorable change in the ratio of perceived supply to perceived demand. This is not the same as culture change, although an exogenous shift upward in perception of demand, for example, redefinition of culturally acceptable consumption, would generate pressure in our sense by shifting the demand/supply ratio.

Of course the 80% endogamy boundary is arbitrary, and we say so. One really needs density equations for the continuous case or good historical or ethnographic evidence of discontinuities to permit empirical definition of boundaries. The situation is no different from trying to detect the boundary between species as speciation is occurring in time and space. In general the approach to measurement of endogamy and thus of social boundaries is no different from the measurement of resources or interactions of any kind and requires a probabilistic approach, typified by the demographers' notion of rates related to a population at risk of an event. The technical complexity of measuring endogamy is illustrated by Romney (1971).

We disagree with Harpending on the implications of his example from physical science and with Millard and McBride on the making of children and of tortillas. If we could observe molecules of a gas with sufficient resolution, we might discover that they were not perfectly identical either but just sufficiently identical for some purposes. We know that the behaviors of individual molecules are not identical and cannot be predicted easily—that is exactly the rationale for statistical mechanics. We cannot predict with much certainty where an individual molecule will be in a cloud of gas or which ones Maxwell's demon will let in or out, but we can talk about the average of these events and positions. Similarly, we can often predict with impressive accuracy the number of persons who will marry or be born or die at some imminent date, even though we could predict who

they would be only with the greatest of difficulty. Further, although we could predict the average production of children and tortillas, we would no more expect the individual producers of children to be identical in their reproductive behavior than we would expect them to be identical in their productive behavior. Just as one tortilla is not exactly like the next, so also one child is not exactly like the next, and culture is not different from biology in this respect.

Finally, we did not restrict our definition of scarcity to food but explicitly included what was marriageable as an example of a resource.

Harris: In general we could not agree more about the importance of the entire range of linkages from natural-resource infrastructure to the moral economy and thought we had said that. We are simply suggesting a demographic focus as a useful avenue of approach. Unlike many other phenomena of anthropological concern, problems in the area of demography often have definable and measurable outcome variables.

A social and cultural theory about demographic behavior faces the same necessity to incorporate a wide and complex range of causal forces as any theory about human behavior. The integration of anthropology that we envision is an integration not of disputing theoretical factions (for example, Marxists, materialists, symbolists, and the rest of the ragbag), as Harris describes it, but of areas of substantive concern, namely, ethnology, archaeology, biology, as Kroeber might have described it. Again we acknowledge that this intent swung us not so subtly toward the hunter-gatherer pole and the simplest kinds of theory, and the poorest bodies of data, even if these bodies of data are improving. Notice (and see also Handwerker's and Harpending's comments and our response) that scarcity in our terms must be defined by the moral economy. We include here cultural perception of the costs and value of children, of intergenerational transfers and especially old-age security and accession to resources, and other systems of evaluation of costs and benefits as approached in the fertility theories of Becker, Easterlin, Caldwell, Handwerker, and others.

Clearly we disagree with Harris about the best units, concepts, and research strategies, and we look forward to his forthcoming book with Ross to see how he spells out his preferred alternatives. We could not, on the other hand, agree more with his conclusion that we still have a long way to go, but a journey of a thousand miles . . .

Millard and McBride: Millard and McBride encumber the notion of error with excessive semantic freight and approach the analogy between cultural and biological evolution at a level which is too substantive and insufficiently formal and processual.

An "error" in our sense is defined formally as a failure to achieve perfect replication. We do not claim that variation is not important in cultural evolution, nor did we seek to judge whether it was more important in cultural than in biological evolution. Our claim is only that variation is the source of raw material for evolution and that failure to replicate is the mechanism that produces the variation. That formal equivalence is enough for us at

the moment. Insisting on the complexity of cultural phenomena seems unnecessary, and assuming the simplicity of biological phenomena may be a mistake.

We dispute Millard and McBride's claim that the first sentence uttered by a child is not a copy of someone else's sentence, in that the first sentence uttered by a child is in important senses exactly a copy of someone else's sentence, else it would not be intelligible. It is precisely the achievement of grammaticality as culturally defined that permits the first sentence to be recognized as a sentence. We are also beginning to discover that there are biological underpinnings to all language and thus to all sentences, consequently to all first sentences. It is just a mistake to think of identity in terms that are too simple and direct (see also our comments in response to Harpending, above).

Similarly, we take exception to the absolute distinction between stable and variant. We made no such distinction, nor do we think our discussion implies the necessity of it. Even if at some observational level some elements of a complex whole (culture or genome) can be said to have been perfectly copied, it is unlikely that all elements of any complex whole are ever perfectly copied. ("Unlikely" does not mean impossible but only less likely than most other outcomes.) The proportion of imperfect to perfect copies, thus the error rate in copying in our terms, is a measure of variation and thus of one factor in the potential for evolutionary change.

Regarding the relationship between group size and speed of change, we point to both the accelerating and the decelerating effects of small and of large size. We speculate that small sizes intensify random drift effects and do so with feedback from stickiness in the marriage market and that the absence of multiple and well-developed interest groups might impede innovation less. We also draw attention contrariwise to the importance of higher population densities in multiplying the number of potential innovators within a single society and in increasing the speed of transmission of innovations. Millard and McBride's introduction of nuclear winter into this discussion confounds the problem of the production and transmission of variation with that of the consequences of adoption of some variants. We hold no brief for runaway political arrogance as a useful survival tool.

Salzano: We are grateful for the additional information on South American populations, some of which had escaped our notice.

Sheets: Again we are grateful for references of which we were unaware. In respect of expanding the scope of our concern, it is particularly important to note that we can only study those social or cultural units which have survived population processes to some point in time. This means that any synchronic or cross-sectional study must deal with a censored data set; the nonsurvivors are missing, and any comparisons are defective. It is only with longitudinal data that we can begin to understand the consequences of different responses to the same or similar events. Hence our theory building is explicitly longitudinal and geographically based, rather than cross-sectional and typological, although we acknowledge that

some excellent work has been done in cross-sectional and typological (or isolated) case studies.

Evolution: In conclusion, we should comment on some perceptions of evolution as applied to culture change that we are trying to avoid. The difficulty with the model of evolutionary theory applied to cultural phenomena is that it is mostly wrong. It is usually Spence-rian and not even Darwinian but pre-Darwinian. We might in fact whimsically regard it as a vestigial survival. Where the concept of evolution is at least Darwinian, it is mechanistic, nonstochastic, monocausal, and insufficiently linked to mechanisms of variation. Indeed, Darwin did not have the information necessary to speak to the origins of variation, only to its selection and the origin of species. Even now, the mechanisms of the origin of biological variation grow daily more complex as knowledge increases, and the issues of endogeneity raised crudely by Lamarck continue to be difficult, especially at the molecular level. We distinguish between endogeneity and exogeneity but do not insist on one or the other as fundamental principles.

We make no case for directionality but only for the identification of mechanisms that can be tested for their effects. In particular we draw no explicit analogies between the substance of biology and the substance of social science but only point to formal mechanisms that should be operative in any system of interacting forces, whatever their empirical realization (provided that they are indeed systems). Neither do we insist on closed lists of operative mechanisms or the relative strength of their effects. We call attention to the complexity of the ecology of population dynamics, including therein a spectrum of constraints and accelerators from the raw natural resource base through cultural perception of the environment, the political structure, and the moral economy, but we do not insist on primacies, only on the necessity of paying heed.

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