



A Demographer's View of Prehistoric Demography [and Comments and Replies]

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A Demographer's View of Prehistoric Demography¹

by William Petersen

INCREASINGLY DURING the past decade or two, archeologists, prehistorians, anthropologists, and practitioners of related disciplines have grappled with the problems involved in deducing population statistics from various types of merely indicative data. The results have seldom been satisfactory even to the authors themselves. The comment in a recent archeology text (Hole and Heizer 1969:306) seems to be typical of the current mood:

Probably few kinds of archeological interpretation have more systematically built-in sources of potential error than have estimates of population, yet such figures are commonly given and used for making further inferences. It is safe to say that, because our concerns in archeology turn more and more toward reconstructing social systems, we shall have to devise methods of obtaining better demographic data.

The way to proceed, I suggest, may be to make fuller use of some of the techniques that demographers have devised to analyze populations in other contexts.

¹ The content of this paper was first presented as several lectures in an experimental interdisciplinary course, cross-listed in anthropology and geology-mineralogy, organized by Ernest G. Ehlers at Ohio State University. Its purpose was to present techniques derived from several disciplines that are useful in classifying and interpreting archeological data. In preparing the article, I received welcome guidance from colleagues at Ohio State University: Emilio Casetti in the Geography Department and William M. Sumner, Peter W. Post, Frank E. Poirier, Erika Bourguignon, Lynn Ager, James E. Collington, and James Ryder in the Anthropology Department.

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The present article, submitted in final form 27 iv 74, was sent for comment to 50 scholars. The responses are printed after the text and are followed by a reply by the author.

That most of those who write on prehistoric demography show little or no acquaintance with the writings of professional demographers is based, one would suppose, on a fundamental misunderstanding: the notion that demography is concerned almost entirely with the analysis of the hard data gathered through modern censuses and vital statistics. Several misconceptions are included in this appraisal. In the first place, no nation has ever made a complete and accurate count of its inhabitants and their characteristics, so that even in the best cases what is eventually published invariably includes emendations by the central statistical office. The demographic data of most of the world, moreover, are full of holes and often quite unreliable. One of the most important practical applications of formal demography, thus, has become "demographic estimation for developing societies," the title of a first-rate text (Carrier and Hobcraft 1971).

Even more pertinent in this context is the momentous change over the past several decades in historical demography. A historian's analysis of population data does not differ essentially from any of his other efforts: given a range of estimates derived from competing sources, he uses his detailed knowledge of the whole social context to judge which is most likely to be correct. That a scrupulous *Quellenkritik*, as Ranke termed it, is necessary also when history is crossed with ethnography is exemplified by the records of Central Polynesian populations examined by Schmitt (1972); one after another, the experts have shown themselves unable to copy figures correctly, to date them precisely, and/or to ascribe them to the right territory. But even if the estimates of Captain Cook and the other Pacific explorers had been copied exactly from the original reports, they would have given no more than the slightest hint of the actuality. Apart from the fact that he usually has a greater and more varied mass of data to work with, the historian writing on any period but the most modern is hardly better off than the archeologist in trying to derive population estimates. The historian does have written sources as well as bones and sherds, but to reconstruct the link between any of them and past numbers is not easy. With respect to demographic analysis, the most fundamental break came not with the advent of writing, but with the systematic accumulation of population statistics gathered for their own sake rather than as the more or less incidental by-products of taxation, conscription, or other activities involving masses of people.

Population differs from other topics of historical or ethnographic analysis in that it is to some degree a self-contained process, invariant irrespective of the cultural context. Since any person aged 25, if he survives one year, will

be 26 years old, a series of reported ages can always be checked for internal consistency (Zelnik 1961, 1964; Coale and Zelnik 1963). Since the population of any area is equal to that population at an earlier data plus the intervening natural growth (difference between births and deaths) and the net migration (difference between immigration and emigration), if some of these elements are known the others can be derived or estimated. This equation,

$$P_1 = P_0 + (B - D) + (I - E),$$

simple as it is, has been one of the most useful in practical demography. Since in any society infants and the elderly are more likely to die within a year than adolescents and young adults, and since childbearing is physiologically limited to females in the same favored age range, there is a necessary relation among mortality, fertility, and the age structure. As A. J. Lotka pointed out as early as 1907, these three elements of any population are associated by the following equation (cf. Coale and Demeny 1966:9-10):

$$c(a) = be^{-ra} p(a)$$

where $c(a)$ is the proportion of the population at age a , b is the birth rate, e is the base of natural logarithms, r is the annual rate of increase, and $p(a)$ is the proportion surviving from birth to age a . In other words, if two of the three factors are known with a given degree of certainty, the range of the third can be stipulated.

Mathematics of even this modest difficulty is beyond the ken of most traditional historians, and the revolution in the field occurred when men well trained in such techniques applied them to historical data. The pioneer, it seems fair to say, was Louis Henry, whose analyses led to the authorship of a manual of historical demography (Henry 1970) as well as participation in the recently founded Société de Démographie Historique and its own *Annales*. The French school has been only somewhat more productive than its counterparts in Britain (e.g., Hollingsworth 1969, Wrigley 1966), the Netherlands (cf. Petersen 1960), Germany (e.g., Mönkemeyer 1966, Maier 1972), Italy (e.g., Santini 1972), and Hungary. In each of these countries, the work of historians writing about population in the framework of a *Quellenkritik* is being supplemented by analyses based on the physiological patterns of demographic factors. No one supposes that the historical expertise is dispensable, but few any longer doubt the superiority of the joint product.

One of the few comparable analyses of ethnographic data is the monograph of Weiss (1973). However welcome efforts are to bridge the gap between demography and related disciplines, one must note that Weiss's contribution shows the typical faults of a pioneer. In a paper addressed to an audience generally poorly versed in mathematics, he uses an unnecessarily cumbersome notation. More important, he displays an ignorance of fundamentals (or, at best, a carelessness in presenting them) that contrasts sharply with his technical pretentiousness.² Weiss's work differs

² A few examples must suffice. The crude death rate, conventionally defined as the number of deaths during a year per thousand persons in the population, he defines as the "number of deaths per person per year" (indeed, where one places the decimal point makes no difference, except in order to avoid this kind of physiological nonsense). He offers a unique definition of the age-specific birth rate. In his definition of the "average family size" (usually called the *completed family size*), he uses "menarche" to designate menopause. "The theory of stable populations," he writes, "requires that a population is infinite in size, has no net immigration or outmigration, and has fixed rates of fertility and mortality at each age." Here he makes a basic error (the population is of course not infinite), emends in a confusing way the usual

from the norm in archeology and anthropology mainly in that it makes some genuine effort to assimilate the elements of demography.

In spite of the growing interest in population, in other words, very little of the demographic analysis in these disciplines has reached the level of professional competence that is now almost routine in historical studies. Of course, it may be that the data base, particularly in archeology, will prove to be too slight to permit one to use these techniques, and I do not presume to prejudge the point. The purpose of this paper is only to pose the question: would not prehistoric demography be improved by a systematic cooperation between those now working in the subdiscipline and demographers, who to date have done little or no work in it?

EXTENSIONS FROM ARCHEOLOGICAL DATA

The paucity of archeological data impedes much more than demographic analysis. Of itself, the base of hard facts is generally too slight to substantiate any but the most elementary conclusions, typically of little theoretical import. Much of archeological lore, thus, depends on the extrapolations suggested by a number of models, either explicit or implicit. Two of these models, ethnographic analogy and the population-resources balance, are implicit in almost every assertion concerning population trends, and it makes sense to comment on them before discussing their applications to demography per se.

ETHNOGRAPHIC ANALOGY

The succession of culture stages the 19th-century theorists postulated, with living peoples interpreted as "fossils" of types that had elsewhere disappeared, has few defenders today. In a modern work it is rare to find such a statement as the following (J. Desmond Clark 1962:1):

Black Africa is rich in ethnographic survivals in micro-environments that emphasize the very gradual and conservative nature of cultural progress in the subcontinent prior to the coming of Western civilization. It is both permissible and illuminating, therefore, to make critical comparisons between later prehistoric cultural assemblages in similar environments and the way of life and material culture of existing, or recently existing, groups living at a similar cultural level.

If this is taken as the more permissive extreme of a continuum, the opposite end can be represented by a stricture against the very concept of "survival" (J. Grahame D. Clark 1965:171-72):

Modern savages have a history precisely as long as that of the most civilized peoples, even if it does not happen to have been written down. . . . Existing peoples can only be used as sources

stipulation that there is no migration in either direction (rather than no *net* migration, which would have to include a specification concerning the ages, fertility, and mortality of the migrants), and shows an unfamiliarity with standard terminology (*immigration* and *emigration* refer to international movements and *in-migration* and *out-migration* to movements within a country's borders, but a combination of the two refers to nothing). In another paper (Weiss 1972), he offers a presumably new measure of "human population growth regulation" (he means *population growth*, for not all elements of fertility and mortality are subject to conscious, effective control). The net reproduction rate (which he misdefines) measures the increase in the number of females over one generation on the assumption that age-specific birth and death rates remain constant. His innovation, which he terms the "index of growth regulation," is the same measure on an annual basis, or what demographers call the *true* or *intrinsic rate of natural increase*. It was first used by Lotka about a half-century ago.

for reconstructing the lives of prehistoric peoples with extreme caution and within well defined limits, since one is otherwise in danger of assuming what one is after all trying to discover.

In his delimitation of the range of permissible interpretation, Hawkes (1954:161) holds that it is "relatively easy" to infer from archeological artifacts the techniques that were used and, thus, "fairly easy" to infer the subsistence-economics of the human groups concerned. Grahame Clark (1965:181-82), on the other hand, warns against the facile association of an arrowhead, say, with the weapon of a contemporary primitive: "one has only to observe the immense range of variation in the mounting of closely similar forms and, on the other hand, in the types of head used for the same purpose to appreciate the dangers of this procedure."

In short, there is a wide range of authoritative counsel, and the canon, "Seek analogies in cultures which manipulate similar environments in similar ways" (Ascher 1961:319), is useful merely in eliminating the clearly inappropriate. In such an early classic as Hooton's monograph on the Pecos Pueblo (1930:chap. 11), for example, the mortality of the reconstructed population was compared with that of France in 1866-77 and India in 1901-10, because the age structures seemed to correspond. Even at that date, a demographer could have pointed out the crucial differences between the populations of a European people, the portion of an Asian one represented in the statistics, and an Indian tribe.

Ethnographic analogy has ordinarily been understood narrowly, as the inference from certain specific similarities in artifacts to similarities in the material culture. It is also useful to apply a generic analogy—to assume that what Murdock (1945) called "the common denominator of cultures" applies also to prehistoric peoples. The link between a particular type of pottery and a particular religion must indeed be tenuous; but if in the entire ethnographic record there is no people without a belief in the supernatural, one should not succumb to a materialist explanation of all prehistoric phenomena. Cook (1946), for instance, offers a detailed and entirely plausible argument that from the late 14th to the early 16th century human sacrifice and war may have accounted for as much as 20% of the mortality in Central Mexico. He is unwilling, however, to accept the religious significance of propitiation of the gods or of the waging of war in part to replenish a waning supply of victims. His straightforward account of these ideational motives is muddled by the supposition that because "the margin of subsistence was becoming somewhat precarious," there was a "social urge" to check population growth. Is it not a remarkably straitjacketed view of history to hold that mass slaughter ostensibly in support of an ideology in fact must mask an unfavorable balance between population and resources?

In a demographic analysis, stretching the plausibilities of ethnographic analogy does not carry one very far, for typically the record of contemporary primitives lacks solid statistics and analyses about population. The painstaking compilation by Krzywicki (1934) is a collection of data mostly too poor to be useful, and in the four decades since it appeared, in spite of the excellence of particular studies, the overall picture has improved but little. We know more about kin structure than about family size, more about beliefs in the afterworld than about causes of death or the expectation of life, more about the structure of roles than that of age categories. Thus, when some of the best of today's anthropologists try to establish demographic generalities (e.g., Naroll 1962), they typically draw their presumably universal conclusions from a very small number of cultures (but see Naroll and Sipes 1973).

THE BALANCE BETWEEN POPULATION AND RESOURCES

A narrowly materialist interpretation of prehistoric societies is encouraged, of course, by the fact that—except in places where archeology overlaps with history—material artifacts typically constitute the sole basis for every reconstruction of their institutions. This bias was strongly reinforced by the influence of Gordon Childe, whose talent as an archeologist was magnified by his adeptness in finding memorable phrases ("the neolithic revolution," "the urban revolution") in his highly readable popularizations (e.g., Childe 1955). In Childe's work the effect of economic factors on any society was dictated by a rather simple-minded Marxism: even at the height of the cult of personality, rather few Western scholars would have cited Stalin to validate the observation that "the natural resources of its habitat" are among "the forces of production" that determine a society's structure (Childe 1951:35). As Braidwood and Howe (1960:chap. 1) put it, Childe's "tendency toward a materialistic philosophy of history rather forced his interpretations into the realms of what Redfield referred to as the technical order, to the exclusion of changes in the moral order." Originally, these authors report, their thinking was "strongly influenced" by Childe, but once in the field, as they tried actually to test his hypotheses, they established a more and more independent conceptual base. They rejected such terms as *mesolithic* and *neolithic*, which group together chronology, technical innovations, and cultural interpretations. Rather than combining all of these a priori and thus imprecisely, Braidwood and Howe analyzed them separately to the degree that this was possible.

Binford (1968) extrapolates from this exchange to an even more forthright denial of the axioms on which Childe's theorizing was based (cf. Sabloff, Beale, and Kurland 1973). It is not true, in Binford's view, that post-Pleistocene man was continuously trying to increase his food supply or, thus, that he had time to elaborate his culture only when he was freed from this preoccupation with the quest for subsistence (pp. 326-28):

While hunting-gathering populations may vary in density between different habitats in direct proportion to the relative size of standing food crop, nevertheless within any given habitat the population is homeostatically regulated *below* the level of depletion of the local food supply. . . . Not only [do] hunter-gatherers have adequate supplies of food but they enjoy quantities of leisure time, much more in fact than do modern industrial or farm workers, or even professors of archeology.

As before, one is inclined to doubt less the legitimacy of ethnographic analogy than the accuracy of worldwide generalizations. Is the link between hunger and work that Richards (1948) so plausibly traced among the Bantu merely to be dismissed? Is it not more likely that primitive peoples, from our point of view a residual category defined principally by their lack of a number of attributes, should vary greatly in this as in other respects?

The application of Childe's preconceptions to demographic questions is ordinarily based on the Darwinian model.³ By this thesis any population tends to increase up to the maximum that the available subsistence permits; thus, mortality, in particular death from starvation, is the main or even sole control of the growth of numbers. At least according to such a zoologist as Wynne-Edwards (1962, 1970), Darwin's model of natural selection does not com-

³ This is often called the "Malthusian" model, but quite inappropriately, for in the subsequent editions of his *Essay on the Principle of Population* Malthus radically amended the simplistic model of the first edition (cf. Petersen 1971).

pletely hold even for all other animals. When the population of some species of fish and birds, some other classes of vertebrates, and some insects and crustaceans reaches what might be termed an optimum level, the fertility temporarily declines, either by a physiological adjustment or by a change in social behavior.

The implication of the Darwinian model for humans is that one can estimate past populations from the "carrying capacity" of the land. Any people's habitat, as exploited with the techniques available in the culture, sets the maximum to which the population can grow. Using this maximum as the actual figure, however, must usually result in a considerable exaggeration. And it is hardly more reasonable to assert that all peoples populate the areas they inhabit up to a fixed proportion of the theoretical carrying capacity, even if this is taken to be something under 100% (cf. Sherratt 1972). It is not my impression that the ethnographic record suggests a constant ratio, or even a narrow range, for all peoples at a given level of technology. If one nevertheless uses the Darwinian model, moreover, the existence of an economic surplus becomes a problem. That at least in some areas man was sufficiently relieved from perpetual want to permit a relaxed search for improved ways of life is ordinarily taken to be a precondition of cultural evolution, but how was this possible under Darwinian conditions? Usually the conundrum is solved by assuming the development of a hierarchical structure, of which the top layers were well fed from the excess produced by the lower ones (e.g., Herskovits 1952:412-13; Dalton 1960; Orans 1966). The solution helps little with respect to the small bands of hunter-gatherers, who are assumed—by ethnographic analogy and common sense—to have had a relatively unstructured society. In short, crucial first beginnings of cultural advance must reasonably have been facilitated by a relative ease. Over the longer run, the life of man in "a state of nature" may well have been "solitary, poor, nasty, brutish, and short"; but this normal state was presumably interrupted by favored periods of perhaps several generations, a mere instant in archeological time. As possibly one such instance, Martin (1973) has hypothesized that, after the first men reached the Americas by way of the Bering bridge, they found a cornucopia of inexperienced prey and within a millennium decimated the native fauna.

As applied to modern populations, the Darwinian model used to be challenged principally by Marxists and by Catholics, and in the international conferences following the Second World War these two camps often formed a strange alliance against the "neo-Malthusians" (or, typically, most of the professional demographers of Western countries). More recently, scholars not associated with any of these ideologies have argued that population pressure, far from being a significant impediment to economic development and cultural modernization, is often (or even regularly) an important impetus to change (e.g., Boserup 1965, Kuznets 1967). When applied to the development of primitive peoples, the new rationale generated the same differences as those pertaining to economies struggling to reach the take-off point (e.g., Sheffer 1971, Spooner 1972, Alland 1972, Merrill 1972, Carneiro 1972). As before, I would suggest that the evidence is too thin in either context to warrant universal generalizations. The number of demographic analyses of developing economies as thorough as Coale and Hoover's (1958) of India is still pitifully small, in part because the data base is ordinarily too slight. Since the archeological record is sparser, it is even more presumptuous to extrapolate the somewhat dubious conclusions of particular case studies to universal postulates. Within the rather broad confines set by a preurban culture and a preindustrial economy, the variation in time and

in space must have been too great to be easily encompassed within any of the typical generalizations. When Zubrow (1971) writes, for instance, that "as an empirical generalization, [the "Malthusian" model] was valid for most of the pre-industrial world prior to 1760," he manages to compress several basic errors into a short sentence. He is manifestly ignorant of all of the recent work in historical demography, in particular that of Hajnal (1965) on marriage patterns. More fundamentally, he uses the preliminary statement of the principle of population that Malthus presented in the first, anonymous edition of his *Essay*, ignoring all of the quite basic alterations that followed it.

In sum, the interpretation of archeological data can go only a very short distance without the aid of ethnographic analogy, and the best guard against inappropriate parallels is the fully conscious acceptance of appropriate ones. Such general strictures, however, obviously count for little. Grahame Clark, who counsels "extreme caution" in the abstract, in the concrete notes that contemporary Eskimo women prepare skins and that the presence of skin-working tools on the Mesolithic site at Star Carr, therefore, "argues for the presence of women" (Clark et al. 1954:11). But this is a picayune point compared with what I have called the generic ethnographic analogy. None of the classic studies of primitive peoples—Malinowski on the Trobrianders, Kluckhohn on the Navajo, Firth on the Tikopia, or whatever—gives the slightest support to the notion that primitives' behavior patterns are encompassed by their material belongings, irrespective of their values and beliefs. If they are to be realistic, archeologists must transcend their artifacts and postulate, even if without details, the existence of a full spiritual life in the peoples they study.

DEMOGRAPHIC ESTIMATES

If the models that have been used to generate demographic estimates are often deficient, this certainly does not mean that the rationale behind them is completely invalid. And if sometimes it has been too facetiously assumed that all prehistoric and primitive cultures must have essentially similar demographic characteristics, this exaggeration does not challenge more modest generalizations concerning all such populations. Assumptions must be made, but perhaps less exuberantly than has been the wont.

WHAT IS A POPULATION?

In most discussions of modern populations, the analyst passes over the most basic question: what entity is being measured? The persons living in a particular juridically bounded area, typically a national state or one of its subdivisions, ordinarily constitute a "population," though this fact does not specify the concept. For different purposes one may want to include in "the population of the United States," for instance, all actual residents, including one-day visitors; or only permanent residents, with "permanent" variously defined; or the civilian population only; or also the military population, either including or excluding overseas units. In other contexts the ambiguity can be greater. One can measure the cultural differences between the populations of northwestern and southern Europe, say, but the result will depend very much on where one draws the boundary between the two. Estimates of how many Jews the Nazis killed, as another example, vary according to whether one defines a Jew "racially," following the Nuremberg decree, or more narrowly.

Archeologists can seldom delimit a population by a legal definition. As they often use the word, implicitly it means

the same as in zoology, a breeding group; but archeological data on mating and fertility are of course poorer than those concerning any other demographic process. Lee (1972a) seems to be suggesting something of this kind when he proposes that "instead of postulating a mean group size of 25, or 50, or 100 for prehistoric populations, it may be analytically more useful to think in temporal terms of the amount of *time* members spent in groups of various sizes" (cf. Birdsell 1973). The span represented in a single dig can constitute centuries, during which much—or nothing—may have happened to the population(s) that once lived there. The puzzle is typically solved by classifying whatever data the archeologist (or paleontologist or anthropologist) has accumulated and then associating a population with each of the classes (cf. Hill and Evans 1972).

According to what Chang (1967:71) calls a reasonable estimate, 80 or 90% of an archeologist's time and energy is spent in classifying his material in order to delineate the "relations within a culturally meaningful system." That it is very easy to construct a nonsensical typology he illustrates with several that a stranger might fabricate out of a collection of American coins and bills. In a similar "plea for statistical caution," Brothwell (1970) offers a comparative morphology of gravestones in British cemeteries of the 19th and 20th centuries, from which—without our extraneous knowledge of so recent a period—many false conclusions could be drawn. The essential point is that there is no fixed association between the "style" of gravestones (or pottery, or residential structures, or whatever) and a particular population. The first can change over time or space with no change in the other; and that the first remains the same over time or space does not mean a necessary constancy in the second.

The most elaborate development of such a classificatory system was the anthropometry once used to divide the human species into races. As with monetary units, the division between any two classes depended in part on which physical characteristic was used, for in a population of widespread miscegenation the various indices of race are not highly correlated. More important, the indices are not, as hypothesized, wholly determined by heredity but can also be markedly affected by the habitat. As Boas (1911) demonstrated, the new environment of America significantly changed even the shape of the skull, once a major criterion of racial differentiation. For a while it was believed that serological traits are neutral with respect to human survival, but for at least some of them this is certainly not the case. In a reaction against 19th-century skulduggery, some anthropologists would now like to expunge the word *race* (and one sometimes feels even the concept) from professional writings, but this reflects a taste for puristic precision that virtually no classification in the real world will satisfy. That there are no unambiguous classes, because the division between them is often imprecise, is true not only of races but of species, as well as of all the cultural classifications that social scientists use routinely: between developed and developing, rural and urban, employed and unemployed, and so on.

Following Boyd (1950:207), I define a race as "a population which differs significantly from other human populations in regard to the frequency of one or more of the genes it possesses. It is an arbitrary matter which, and how many, gene loci we choose to consider as a significant 'constellation.'" In other words, how many races exist and what the boundaries are between them are not immutable facts; each analyst can legitimately focus on the constellations relevant to the particular questions he is putting to the data. Since to some degree racial characteristics are hereditary, they are in principle a better basis for the classification of breeding groups than cultural artifacts, but

neither affords the comfortable certainty that some would like.

Modern populations are more often classified by language than by race, and the difficulties in the two operations are analogous. In both instances the analysis depends on which "constellation" is selected out of a clustered continuum as the classificatory unit. As Sapir (1921:163-64) put it,

All languages that are known to be genetically related . . . may be considered as constituting a "linguistic stock." . . . At any point in the progress of our researches an unexpected ray of light may reveal the "stock" as but a "dialect" of a larger group. The terms dialect, language, branch, stock—it goes without saying—are purely relative, . . . convertible as our perspective widens or contracts.

Nor does it ordinarily help to combine the two imperfect instruments of racial and linguistic classification in the hope of improving on either one. Occasional peoples such as Basques or Lapps constitute enclaves on both racial and linguistic maps, but they are exceptional. As Max Müller once wrote, "An ethnologist who speaks of Aryan race, Aryan blood, Aryan eyes and hair, is as great a sinner as a linguist who speaks of a dolichocephalic dictionary or a brachycephalic grammar." In an analysis of the interaction between racial and linguistic criteria for classifying the populations of Bougainville, Howells (1966) finds that only 11 of the 18 physically distinctive groups correspond to linguistic divisions. Spatial distance is insignificantly correlated with the degree of biological differentiation, but biological and linguistic "distances" are fairly well correlated. Howells concludes (p. 535), "The most definite evidence leads us back, somewhat frustrated, to the conclusion which was the assumption of the early anthropologists: differences are due to pre-existing differences," at least mainly and overall.

POPULATION SIZE

In both historic and prehistoric demography, it would seem that one of the simplest operations is to relate the inhabited area to the number of persons that once lived on it. Such estimates, however, involve several recurrent difficulties, none of which can be overcome completely. An archeological excavation typically includes only a portion of the site, and to generalize from the known segment to the whole must ordinarily result in some slippage. Is the population density deduced from the completely excavated dwellings specific to them? Were all of the houses inhabited at the same time, or do they represent a number of successive generations living in different houses on adjacent plots? Thompson (1971), for example, points out that among the Maya it was the custom to bury a dead person in his hut and then abandon it and, on another scale, to move from one site to another as good soil became exhausted (but cf. Haviland 1972). In Central Mexico, similarly, "sites may represent those segments of a living community which have been abandoned during normal community operations. All contemporary communities studied in detail in the Teotihuacán Valley have some structures unoccupied at any given time" (Charlton 1972). The dwellings inhabited seriatim would presumably have been constructed over perhaps a few decades, too short a period to be distinguished by almost any dating technique.

Trying to generalize on a world scale involves more problems, for population density is likely to differ with the climate, the usual mode of construction, the number and type of nonresidential buildings, and the like. Even

in so uniform a culture as the contemporary United States, the density of metropolitan populations varies considerably between such cities as Denver, made up largely of one- and two-family houses, and Chicago, where most persons live in apartment houses. Among medieval European cities, the estimated number of persons per hectare varied from 58 in Bruges to 289 in Bourges (Russell 1958:tables 63-65). Should one assume that the ratio of persons to inhabited space has a smaller range among primitive peoples? From 18 ethnographic studies, Naroll (1962) estimated very roughly that each person in a primitive society requires 10 square meters of roofed living space (cf. LeBlanc 1971). From California Indian tribes, Cook and Heizer (1968) derived 12 square meters per person. Given the wide range of error implicit in the calculation, the two ratios, 10 and 12, can be interpreted as essentially identical, especially since the total populations derivative from either one are usually very small (cf. Phillips 1972). The data are limited to one type of population, and similar formulas are needed to apply this kind of analysis to hunter-gatherer bands and to agricultural, class-structured societies (Baker and Sanders 1972).

The population-area ratio of a settlement would seem to imply an average household size, but calculating the latter typically involves additional dubious assumptions. There is no way of determining how the size of a nuclear family, one of a number of interrelated demographic variables, relates to that of a household. As Allen and Richardson (1971) point out, the difficulties in the reconstruction of kinship from archeological data—which inherently are certainly great enough—begin with the confusion among ethnologists concerning contemporary residence patterns. The apparently clear distinctions made a generation ago among types of residence rules have proved to be inadequate, and in any case in many societies practice differs significantly from the presumed norm. Among 13 American Indian tribes, the floor area of the house assumed to contain a single nuclear family ranges between 119.5 and 322 square feet (Cook 1972:table 1), and if worldwide data were compiled the variation would presumably be greater. In a stratified society, moreover, the average number of persons in a household or the average amount of space they occupy would probably differ greatly from the mode. In calculating a total population from the area of an entire settlement, one can reasonably expect that some of these several types of variation will average out.

Rough guesses have been made about the size of a household from the capacity of cooking jars (Turner and Lofgren 1966) and about the size of populations from the skeletal remains (e.g., Howells 1960) or even the animal bones in garbage heaps (e.g., Phillips 1972). One wonders whether even the most ingenious argumentation could develop a plausible estimate from such data. As before, a comparison in the style of Brothwell's morphology of gravestones would suggest that an investigation of modern kitchens, modern cemeteries, or modern dumps would lead to no demographic conclusions worth recording.

NATURAL INCREASES

The growth of modern populations is ordinarily a secondary datum, derived either from a series of census enumerations or from a count of births and deaths. Especially when these primary data are deficient, an independent model of population growth can be a useful check. But if such a model is based on only an implicit rationale and is followed more or less blindly, the supposed tool can become a prison. One instance is the almost routine assumption, deduced from the Darwinian theory of how population relates to the food supply, that during the Neolithic the rate of

increase greatly accelerated. A typical statement of this dogma is that "the increased food supply resulting from combined agriculture and dairying must have produced an exceedingly rapid growth of population" (Linton 1955:237).

Few persons who have not worked regularly with population data (or, alternatively, with such a phenomenon as the effect of an interest rate on the principal) intuitively feel the power of geometric growth. Malthus spent so much time drilling his contemporaries in two very simple progressions because, in fact, they were hard to get across. In 1971 the population of the world was increasing by an estimated 2.0% per year and that of various countries of South America or Africa by 3.4% per year. This means that these populations would double in, respectively, 35 and 21 years. As dozens of writers have now shown, it is easy (particularly with access to a computer) to project such rates some decades into the future and thus to demonstrate, by the stupendous populations that would ensue, that the growth rate of the modern era is anomalous. It cannot have been so high very far in the past, and it cannot last much longer.

Carneiro and Hilde (1966) have applied the same technique to the population growth in the Near East during the Neolithic (cf. Nag 1973, Kurth 1974). They take 100,000 to be the initial population of the area at the beginning of the era, 8000 B.C. If the rate of annual increase had been 0.5% (only a quarter of the rate at which the world's population is currently growing), they calculate that the population would have become 46,200,000,000 by 4000 B.C. They go on to say (p. 179):

The choice of a rate of even 0.13 percent results in a total population for the Near East in 4000 B.C. of over 18,000,000, which still appears excessive. . . . The increase in population that occurred during the Neolithic period was *not* "exceedingly rapid." It was, in fact, only on the order of one-tenth of one percent per year. For a village of 100 this rate of increase is equivalent to a net gain of only one person over a 10-year period.

Of course, there were fluctuations over these several millennia. In especially favored areas or periods, the increase was undoubtedly considerably faster, but that means that in other places and times it was even less than 0.1%. Overall, the growth was so slow as to be imperceptible during anyone's lifetime, and during the whole of the era the fertility and mortality were more or less in balance.

MORTALITY

As Grahame Clark (1965:chap. 3) has expounded in fascinating detail, the survival of the body's soft parts is much more common than most archeological works suggest. Even so, the direct evidence on the mortality of ancient man depends mainly on skeletal remains, from which rather little can be deduced with reasonable certainty.

Bone pathology is not an important medical specialty altogether. As a visiting professor in a New York medical school, Henry L. Jaffe gives a single two-hour lecture on the subject, and that is "more than some other medical schools devote to skeletal pathology" (in Jarcho 1966:65-68). Any conclusion from only the bones of a person recently deceased is difficult, and during the centuries that ancient man's skeletons endured they usually underwent many changes that make a diagnosis yet more uncertain. "When we remember the many ways in which a pseudopathological appearance can be produced—or a genuine lesion obscured—it no longer seems extraordinary that paleopathologists occasionally make a wrong diagnosis. The wonder is that we ever make a right one" (Wells 1967). In some instances at least (e.g., Stewart 1966), the recent revival

of interest in paleopathology has meant less a development from such pioneers as Ales Hrdlička and E. A. Hooton than a rejection of their work and a fresh start from sounder beginnings.

Nor is it possible to round out the evidence very much from ethnographic analogy. Contact with higher cultures has ordinarily influenced the mortality of primitives so much, both negatively and positively, that one can hardly reconstruct the life chances of isolated hunter-gatherer bands. According to a summary of 16 ethnographic studies in various parts of the world, the proportion dying under 1 year of age ranged from 10 to 35%, that under 5 years from 30 to 70%, and that under 18 years from 26 to 35% (Polunin 1967). Some of the variation is undoubtedly due to the different methods used in the surveys, but there is also no reason to anticipate an overall uniformity among primitive peoples independent of their habitat, standards of sanitation, nutritional norms, patterns of violence, and therapeutic skills. Both the dominant causes of death and the expectations of life should differ considerably.

In some areas, for example, food shortages have contributed greatly to mortality and morbidity, but among native Hawaiians famines were relatively infrequent and mostly local, causing more out-migration than deaths. The one exception was the famine caused by the civil wars of 1780–96, not an instance that fits in with the Darwinian model (Schmitt 1970). As this example suggests, the deaths from organized violence are far from limited to those killed directly. Even the meaning given to “warfare” is not consistent in anthropology (e.g., Schneider 1950, Gibson 1974), and any estimate of the mortality “caused” (however one stipulates that term) by war must be very loose indeed. Yet we know that there is a worldwide range from the Zulus, whose whole livelihood derived from preying on other tribes, to the entirely pacific Indian tribes among those in California.

Estimates of ancient man’s longevity can hardly be very precise. Whether cooperation with a demographer could significantly improve them depends, in short, on whether the archeological (or even ethnographic) data are good enough to apply more advanced techniques usefully. According to McArthur (1966), “Given the generally inadequate data we have about primitive populations, it would be foolish to try to generalize about the course of population change in Polynesia after its discovery. And because of the smallness of even the aggregates, none of these populations is amenable to the sort of logic that might be applied to really large populations.” This conclusion would seem to apply a fortiori to archeological data on prehistoric man, which are typically much thinner and even less reliable.

The sex of skeletal remains is often problematic (Krogman 1962:chap. 5). In the extreme case of the scrappy bits of *Australopithecus* recovered from several sites in East Africa, what earlier investigators had taken to be the male and female of a single species Pilbeam (1972) interpreted as two manlike species, one “robust” and one “gracile,” coexisting in the same general area. Even in the best case, determining the sex of a skeleton demands so much skill that a layman must depend on experts’ testimony, which unfortunately is not entirely consistent. Working on Egyptian mummies, Wood Jones (cited in Genovés 1970a) found pelvises that he would have characterized definitely as male except that there were fetuses in the wombs. With the whole skeleton and his own method of determination, Genovés (1970a) believes that he can classify 99% of all remains correctly; but with partial skeletons or what he adjudges less accurate techniques, the probability of correct classification has in his opinion fallen as low as 20%. In a recent survey by Acsádi and Nemeskéri (1970:74–75), something like the same range in estimated accuracy is reported from

other authorities. These authors select 30 key characteristics of a skeleton, each of which can be graded on a five-point scale from “hypermasculine” (+2) to “hyperfeminine” (–2), giving a total range from +60 to –60 (pp. 87–89). The added precision from this scale would partly depend, of course, on whether the characteristics are independent of one another. But would not a male with a “feminine” pelvis also usually have a “feminine” skull?

In the last decade or so the principal development in the sexing and aging of skeletal remains seems to have been the shift from individuals to populations. The physical anthropologist’s professional interest in identification, as Giles (1970) puts it, is “focused on some *population*, for which he hopes he has, in his laboratory, a representative sample, and from which he can adduce such features as sex, age, stature, body form, disease as reflected in the bone, and the like” (cf. Howells 1970, 1973; Hunter and Garn 1972; Ditch and Rose 1972). The main issue with respect to sex and age, I suggest, is that this “hope” is hardly likely to be realized in a collection of bones from an archeological site. The bones of females and of infants and children, since they are typically thinner, are less likely to remain intact in equal proportion; and information from burial sites or epitaphs, as scholars as early as Beloch (1886) pointed out, is likely to be biased in the same ways. The problem, in other words, would seem to be to devise mathematical techniques based not on the postulate of a representative sample but on the assumption that the sample is systematically biased in ways and degrees still to be stipulated.

In any case, a completely correct dichotomy of a population into males and females (as is possible, of course, with living primitives) is not much use for demographic analysis unless it is accompanied by a reasonably accurate classification into age categories. One common criterion for estimating the age of skeletal remains is the degree of wear on teeth, but, as Vallois (1960) has pointed out (with strong support in Howells’s commentary), no common standard exists on how rapidly the teeth of either fossil men or existing populations have worn down. Vallois would use the condition of teeth to judge age only up to about 12 or 13 years, with the closing of cranial sutures serving as the principal index thereafter. According to Genovés (1970b), however, the obliteration of sutures in the skull “does not follow a well defined pattern”; and R. Singer (in Heizer and Cook 1960:212–13) gives one reason why this may be especially so of fossil remains: “If you put a drop of hydrochloric acid on a fairly well closed suture, you will find that after a short while the suture will open up. . . . Highly or fairly acid soils . . . will also open up sutures. . . . That so many of the fossil skulls so far have appeared to be of fairly young men [may be because] very often the soil is acid.” More certainly, deaths of infants, children, and possibly adolescents are also typically underestimated, partly because in some cultures infants and children are disposed of separately and partly because—as I have said—the lighter bones are more likely to have disintegrated.

When archeologists compare alternative methods for estimating age from skeletal remains, they seldom have a schedule of actual ages against which their several results can be checked. One must assign special weight to McKern and Stewart’s (1957:172–73) monograph reporting the analysis of American soldiers killed in Korea, all of known age. The report is discouraging: “Individual maturational features or events are highly variable in a chronological sense. . . . [However,] viewing an unknown individual as a total skeleton, rather than bone by bone, [one can]

estimate the age at death within narrower limits." In general, though the steps of maturation by any of several indices succeed one another in order, their correlation with age is slight.

The limitations noted here are well known, of course, to the physical anthropologists working on aging and sexing, and new techniques are continually being offered. If these prove indeed to be improvements—and on this I offer no opinion—one should note that most of the existent data comprise estimates based mainly on less reliable techniques. In the best case it will take another generation to reanalyze enough skeletal remains to lay a better basis for our impression of the population structure(s) of ancient man.

Vallois (1937), studying the remains of 20 Neanderthal, 102 Upper Paleolithic, and 65 Mesolithic individuals, found that the proportions dying at or under 20 years of age were 55, 34, and 37% respectively. If, as seems likely, the very high proportion of infant deaths was underrepresented among the surviving skeletons, the usual age at death would be lower than these figures suggest. Subsequent compilations by Vallois and others indicate the same general range. Very roughly, then, over the span of man's existence there have been two doublings of the expectation of life from birth, from under 20 years in the earliest prehistoric period to 35 or 40 years in preindustrial civilizations and 70 to 75 years in today's advanced societies.

FERTILITY

The measurement of fertility is far more difficult than it might seem. The significance of "family" is ambiguous even in our own culture (does it include grown children who have moved away, or not?); and with the variety of kin structures to be found the world over, simple queries can often be misunderstood. Many peoples, out of a sense of privacy or a taboo, are reluctant to discuss any family matters with a stranger. In a fertility survey in East Africa, for instance, "a man who refused to give any information to the first two investigators finally gave the names of two children to the third. . . . [He] later admitted to ten children but still concealed a second wife in another village" (Richards and Reining 1954:362). And if questions are fully and accurately answered, the data are not always interpreted properly even by professional social scientists. For example, a survey of three "generations" on Eddystone, one of the Solomon Islands, seemingly showed a calamitous decline in the number of children per marriage, from 2.16 to 1.28 to 0.65 (Rivers 1922:98). In fact, since there was no control for the age of the mother, the contrast was mainly between the completed fertility of the older women and the partial families of the younger. In general, the critical datum for any long-term analysis is not an annual rate, which can fluctuate widely, but the total number of children that the average couple bring into the world.

No direct data exist, of course, on the fertility of early man. From the suppositions that his mortality was, by our standards, extraordinarily high and that his population was not depleted, one can conclude that the fertility must have been close to the physiological maximum. Indeed, most demographers of a generation ago assumed that this condition more or less obtained until the modern era, when the development of effective contraceptives and the dissolution of traditional norms presumably led to the first substantial decline in family size. This reconstruction has been proved wrong, not only for most of preindustrial history but also for most present-day primitives. Reproduction up to the physiological maximum is approximated by the Hutterite sect in the United States (Eaton and Mayer 1954) and the population of the Cocos-Keeling Islands

(T. E. Smith 1960), but by few other societies at whatever stage of development.

In Tikopia, for example, deliberate control of population growth was effected by a number of traditional means: nonmarriage, more or less enjoined on young men without land; coitus interruptus, commonly used in both extramarital and marital sex; abortion, generally restricted to extramarital pregnancies; and infanticide, sanctioned at the discretion of the father irrespective of the child's sex. Exposure to the risk of pregnancy was typically not over the whole of a woman's fecund years: marriage was later than at puberty, and the remarriage of widows was rare. These conscious mechanisms were supplemented by the effects of such other traditional practices as interisland voyages and wars, by which many young males were lost (Firth 1957:163, 373-74). From an early survey of primitive cultures, Carr-Saunders (1922) concluded that *all* of them include customs whose primary function is to restrict the increase of population: abstention from marriage, delayed marriage, periodic abstention from intercourse, coitus interruptus, prolonged lactation, other types of contraception, abortion, and/or infanticide. Whatever fault one may find with this generalization, it is certainly far more valid than its contrary—that primitives ordinarily follow the Darwinian model.

If we speculate about the beginnings of population control, ethnographic analogy can take us only to a choice of models. In many species, the check to further growth through mortality is supplemented, as I have noted, by a physiological or instinctive behavioral adjustment of the fertility. Konrad Lorenz and others have extended this process to humans, but with little evidence to support their notion. The only study I was able to find was by three Yugoslav physicians, who report that peasants who migrated to the city and got stressful jobs in a crowded and noisy factory became sterile for an average of three years (Milojković, Šimić, and Džumhur 1972). In the human species the control of fertility is mainly cultural, only incidentally physiological. If only through infanticide, man has always had the capability of limiting the number of his offspring, and the question is when and under what conditions he would be motivated to exercise this control. There is no reason to suppose that the transition took place in a single "stage"; rather, if mortality varied from one prehistoric culture to another as much as I have suggested, one should expect a parallel variation in the development of fertility control from the occasional practice of individuals to the institutionalized norms of societies.

Moreover, there is no reason a priori to date this process late in the prehistoric era. Lee (1972*b*) has shown that among the !Kung Bushmen, hunter-gatherers in transition to a sedentary life, the burden of rearing children is becoming markedly less. When the band is on the move, the mother has to carry her young child or children. From estimates of the weight of a child of each age and the total distance covered per year, one can calculate the work load per mother, and thus the incentive to space births. In this tribe the first pregnancy comes three to five years after puberty, and extended lactation keeps the interval between subsequent births at about three to five years. "Data on the !Kung (who are in one sense on the threshold of the Neolithic) suggest that sedentarization alone may trigger population growth, since women may have children more frequently without any increase in work on their part and without reducing their ability to provide for each one" (Lee 1972*b*:342). The generalization one might derive from Lee's paper, that hardship stimulates the control of births and an enhanced well-being increases fertility, is hardly in accord, however, with the thesis that "the demand for oysters and champagne, not for basic bread and butter,

triggers off social conventions which hold human populations down" (Douglas 1966). It is also reasonable to suppose with Angel (1972) that the shift to sedentary life improves a tribe's diet and therefore its health, and thus raises both the proportion of females surviving to childbearing years and the average woman's fecundity.

DEPOPULATION

One method of estimating the past populations of primitive peoples is to postulate that there was a sizable or even calamitous decline following the first contacts with civilization and to extrapolate backward from the more recent, comparatively well authenticated counts. By way of ethnographic analogy, the technique can also affect our idea of the probable size of prehistoric populations. The thesis certainly has some validity. Infectious diseases, always more virulent in a fresh population, have often devastated primitive peoples, who almost by definition are more isolated and thus more likely to encounter them for the first time. In any power struggle with representatives of an advanced technology, primitives invariably lost out, with sometimes quite serious losses of population. The introduction of such new weapons as steel knives or repeating rifles also made their own wars more deadly. And the disruption effected by the infiltration of the sometimes deleterious elements of alien cultures could be cumulative. The social structure of a primitive people, though sometimes resilient in the face of reverses associated with their own tradition, often proved to be fragile in an encounter with a higher culture, for the social constraints of a nonliterate society typically rest, in Durkheim's terms, on mechanical rather than organic solidarity. Indeed, the causes of depopulation comprise so impressive a list that the problem would seem to be how it happened anywhere that some primitives survived.

The commentary that Pitt-Rivers (1927:19) offered on the process in the Pacific islands is still largely true today (but cf. McArthur 1968):

During the past fifty or sixty years the dying out of the native Pacific populations has frequently been the subject of official and unofficial inquiries, and it is remarkable that there is as little agreement on the subject now as when it was first investigated. . . . No satisfactory system or method has been established, and, largely in consequence of this, during the whole period few exact vital statistics are obtainable which might throw light upon the matter and establish the correctness or otherwise of diagnostic attempts.

Pitt-Rivers compiled an amusing table listing the "causes" of depopulation among South Sea Islanders. In the first column was, for example, the allegation that the abolition of headhunting, by depriving the natives of their chief interest in life, had brought about a despondency which eventually decreased fertility; in the second, the argument that headhunting continued and contributed to a high mortality. In the first, again, were listed various types of European foods or clothing condemned as unsuitable, and in the second various types of native food, clothing, housing, and so on that were denounced as unsanitary. Analysts have found it all too easy to ascribe depopulation to any prior condition, whether the persistence of elements of the native culture or the change in these by acculturation. *Post hoc, ergo propter hoc* has seldom been applied so freely.

Depopulation was far from universal, and where it occurred its severity varied greatly. For example, among three 17th-century peoples in the Caribbean area, the Indians of Hispaniola (Haiti) were nearly extinct within a single generation, the Omagua were reduced by half within 40 years, and the neighboring Cocama retained about

their original numbers to a recent count (Steward 1949). In the New Hebrides, the population of some islands—for instance, "Tanna, Malo, Paama, Merelava, and probably Tongoa" (Felix Speiser, in Rivers 1922:51)—increased. It would seem that Eskimo populations have generally grown since market products were added to their traditional subsistence economy and that, except for the coastal villages along the Bering and Beaufort Seas, this increase took place without a prior decline (Hughes 1965). How difficult it is to check these assertions can be illustrated from one example Hughes gives—the Angmagssalik Eskimos on the east coast of Greenland, who numbered 413 when Holm discovered them in 1884 and have apparently increased without interruption since then. The statistics that the Danish government collects are based not on tribal units but on places, of which the inhabitants are designated as either born in Greenland or not. If one assumes that those resident in the commune of Angmagssalik who had been born in Greenland are Angmagssalik Eskimos, then Hughes is correct in his assertion. At the end of 1971 the population, so defined, was 2,249 (Denmark 1973:table 3B). In general, however, many Eskimos have some white forebears, and in that sense—but in that sense only—the figure may be too high.

In the most recent period, one must emphasize, indigenous peoples are often increasing faster than the surrounding population—the Navaho in the United States, for instance, or the Maori in New Zealand (Borrie 1959, Pool 1967). Often, as their numbers grow, so does their pride in their heritage, and their nativist accounts of a more or less legendary past support the supposition that a large and prosperous people flourished before the Europeans came.

If past populations are estimated from the maximum carrying capacity of various habitats, the figures, as I have noted, are usually too high. Using this technique, Sapper (cited in Steward 1949:656) arrived at a range of 37 to 48.5 million natives of the Americas at the end of the 15th century. Moreover, the first actual figures for any area are likely to be set by explorers, missionaries (cf. Schmitt 1967), or administrators, all of whom can improve their reputation by upping the numbers they have, in their several ways, dealt with. So astute an anthropologist as Kroeber found it appropriate, therefore, to cut all estimates of the Indian population derived from Spanish sources; Dobyns, on the contrary, established their validity by pointing out that they often support each other. There is a natural inclination, moreover, to generalize from the most striking examples of population decline. Dobyns (1966), thus, estimated the "nadir," or lowest level of population, from modern estimates or censuses, then multiplied it by a measure of depopulation based on guesses about catastrophic losses. For the whole of the Central Andes, for instance, Dobyns relied on the depopulation in areas that had suffered an estimated loss of $\frac{15}{16}$ or even $\frac{24}{25}$, whereas according to a detailed analysis of just this region (C. T. Smith 1970) the depletion may have been from something over 400,000 to something under 100,000—certainly dramatic enough for most tastes.

Finally, in many instances the loss of population was statistical rather than physical. Many natives disappeared by the route of race mixture. "When is a Maori a 'Maori'?" (Pool 1961) is a question that, appropriately specified, must be posed as the prelude to virtually every analysis of the population of primitive peoples (cf. Petersen 1969). Primitives' most usual response to disaster perhaps was to move out of its path. With the poor statistics available to present-day compilers, such migrations are likely to be interpreted

as still more depopulation, for those escaping from oppression in one area were hardly likely to advertise their presence in another.

In the Middle West of the United States there is a vast number of mounds, 10,000 in the Ohio Valley alone, built centuries ago by a people that vanished completely before there were any whites on the continent. No one knows why, and the lack of fact stimulated a lush growth of speculation. The builders of the mounds became the Mound Builders, and from their paltry remains were deduced the qualities of a diligent and noble race. It would be a useful exercise for those intent on magnifying the population losses elsewhere to ponder on this example of romantic fallacy, as recounted in Silverberg's (1968) "archeology of a myth."

Might not the frequent exaggeration of the depopulation of primitives be based on a similar stance? The general unreliability of all estimates would not of itself preclude the figures in either direction. The bias, I would conjecture, is founded on two predisposing tendencies. The self-selection of persons who become ethnologists is strongly reinforced by their training, of which the usual product regards nonliterate peoples with full, even fulsome, sympathy. And anthropologists are hardly immune from the mood in all the social sciences that designates modern civilization, and especially its liberal capitalist version, as largely evil. Whenever these two types of cultures came into contact, however, the overall superiority of modern civilization was inescapable. Analysts who regret this imbalance in technical skill, political power, and moral force may fall back on representing the depopulation of primitives as a kind of *Götterdämmerung*. One senses a striving for the dramatic in the climbing estimates, for example, of the pre-Conquest population of the Americas, from 8.5 million in 1939 (A. L. Kroeber, cited by Steward 1949:656) to 100 million in 1962 (Woodrow Borah, cited with approval by Dobyns 1966:414) to 113 million (the higher estimate of Dobyns himself).

MIGRATION

In a world relatively unpopulated by humans, presumably each small band of gatherers or hunters would follow wherever the available subsistence led, moving only a few miles a day but eventually perhaps covering considerable distances. As Childe (1950:93) surmised, "Assuming quite short shifts of territory every 12 years or so, it would take only a few centuries for a modest initial population to spread from say the Drave to the Harz." For ancient man, migration was not an occasional aberration from a settled life, but the norm; as I have indicated, every other demographic characteristic, from the definition of a population to any estimate of depopulation, is partly based on (usually implicit) assumptions about it (cf. Kurth 1963).

The reconstitution of prehistoric migrations from any type of current data ordinarily consists of three steps: (1) classifying the data into a spatial pattern, (2) discounting the factors other than migration that might have changed the pattern, and (3) inferring migrations from the remaining systematic differences in the pattern. Embedded in the schema at point (2) is that hoary dispute concerning repeated invention versus diffusion from a single source, and in its current phase the argument is sometimes still as dogmatic as in the 19th century. Thus, Coon's (1962) thesis that the human species evolved through a convergence of discrete hominid stocks, though offered with an expertise that seemingly would have demanded respectful attention, has been rejected by most scholarly reviewers out of hand. In a major work of folklore methodology (Krohn 1971:58, 126-27), as a completely different instance, it is axiomatic that "the variants being compared all go

back to one parent form. . . . Even one single independent reoccurrence of a complicated form—for example, the Cinderella tale—as the result of the general similarity of human fantasy or pure chance is highly unlikely." Variations, moreover, generally "increase progressively in the direction of migration." The analysis of folklore so conceived consists in the search for the *Urform*, its geographical placement, and the tracing of routes of diffusion from that nucleus.

Perhaps the simplest example of a reconstructed past migration is that given in Jackson's (1953) masterful work on the relation between Brittonic (the language that the Celts had brought to Britain) and early English. He uses geographic place names, especially of rivers, to mark the westward movement of the Angles and the gradual retreat of the Celts. East of a line running north from the Isle of Wight, Brittonic names are rare and are confined almost entirely to large and medium-sized rivers. At the other extreme, Monmouthshire remained Brittonic until at least the Norman conquest, most of Cornwall until the 18th century, and much of Wales to the present day; and in this area the names of rivers are overwhelmingly Celtic. "From the evidence of place names it is clear that in point of fact the British population was nowhere completely exterminated, though it certainly survived more fully in some areas than in others" (Jackson 1953:234). This seems a very large conclusion from so slight a base. If we imitate Brothwell's exercise with modern gravestones and ask what relation the existence of Indian place names in the American Middle West has to an Indian population, or Spanish names in California to Spanish, or Polynesian names in Hawaii to native Hawaiian, we impose a stricter test than Jackson put to his own thesis.

Nor does an attempt to reconstruct prehistoric migrations from the present distribution of human races (e.g., Taylor 1928) lead to an unambiguous conclusion. As I have noted, the hypothesis that certain physical traits are wholly hereditary has been disproved with respect to some and is an unproved assumption with respect to others. To the degree that racial characteristics respond to environmental influences, it is impossible to deduce a prior contact from present similarities. To the degree that physical traits are inherited, the decisive factor is the separation of gene pools, and this can be effected either by migration or by such a social pattern as caste endogamy. Migration, while it generally results in sexual isolation, is not a necessary condition to it.

Whether we depend on the present distribution of races, of languages, or of artifacts, there is no reason to assume that their diffusion invariably used human migration as its vehicle. "It is plain that most culture changes from without have occurred through subtler and more gradual or piecemeal operations" (Kroeber 1948:473). And if a similarity among surviving artifacts does reasonably indicate a prior migration, one cannot necessarily determine which way the prehistoric bands went. After citing four archeologists who reconstructed a Neolithic migration from the Danube basin to what is now Macedonia, Childe (1950:50) suggested—"reluctantly"—that the movement had been in the opposite direction. An analogous state of knowledge would leave us in doubt whether Englishmen populated the United States or Americans England.

COMMENT

Some instructors in field methods, I am told, advise their fledgling ethnographers to start the study of any people with a census, and in the undoubtedly prejudiced view of a demographer this is excellent counsel. To have a count

of the two sexes, the several main age brackets, the families, the social and economic and cultural characteristics is to have a map to guide the analyst to whatever topic he desires, with many of the links and patterns already suggested. Yet until quite recently the study of population was alien to most anthropologists. A recent survey of the subdiscipline (Baker and Sanders 1972) does list many items, but most pertain to such adjacent fields as human genetics or physical anthropology. One senses that a breakthrough is coming, and perhaps it will be hastened by collaborative efforts (e.g., Romaniuk and Piché 1972); the population analysis of even so valuable a monograph as Firth's study of the Tikopia was much improved in a subsequent paper written jointly with a demographer (Borrie, Firth, and Spillius 1957). In the longer run, a larger proportion of working anthropologists and archeologists will themselves acquire the quite modest technical expertise on which demographic analysis is based. From this nucleus may develop a general awareness that data on age-sex structure are worth gathering and, thus, eventually a far better factual base for comparative studies.

Until some of this development takes place in anthropology, the direct benefits of ethnographic analogy to prehistoric demography will continue to be slight. Once that small step has been taken, the more fundamental problems in archeological research will become more manifest. Much of the work pertaining to the population of early man, it seems to me, is compromised by false premises—has gotten off to a wrong start by not asking the right questions.

There seems now to be a widespread consensus that the Darwinian model, which cast *Homo sapiens* as one more mammalian species responding more or less automatically to his environment, is inadequate. The conditions that set man's fertility, mortality, and migration patterns were always in part physiological and physical, but they were also cultural and social. To permit the difficulties in gathering nonmaterial data to reshape one's conceptual framework makes for poor science. In some respects, as Willey (1968) seems to agree even in his critical statement, the new emphasis on "settlement archeology" that K. C. Chang and others proposed would seem to be a step toward a better-rounded social analysis. From settlement patterns it may be possible to deduce first community patterns and then several of the social structures basic to demographic analysis—though indeed with the recognition that "limitations will always be with us" (Sears 1961). Demography makes use of mathematical models and is related to such physiological subdisciplines as genetics, but it is itself a social science.

The usual generalizations concerning the demographic characteristics of prehistoric man often strike one as too broad, extrapolations from a few nonrandom cases to a supposed typicality. Perhaps it will never be possible to do more, for some of the difficulties are inherent to archeology; but it may be feasible, given the will, to indicate not a figure but a range, with some stipulation of the preconditions that govern the probable level of fertility and mortality in various settings. To the eye of modern man, all primitives look alike; but certainly it is the goal of a professional observer of primitive society to transgress this view.

Prehistoric demography, finally, shares with many analyses of other types of population a disposition to see the growth of numbers mainly, or even only, as a dependent variable. Whatever the general worth of Boserup's counterthesis (1965), it at least led to the discussion typified in the work edited by Spooner (1972). In the context of the transition from hunter-gatherer bands to the first urban settlements, the increase in population stimulated the rise

of new institutions, more complex social structures. Demographic factors, in short, typically are both cause and effect, elements in a material-cultural complex; and to view them as only the consequence of Neolithic or urban revolutions is, like any other monistic theory, distortive.

Abstract

The direct data on the population of prehistoric man are typically too sparse to be used alone. However, such supports as the population-resources model can easily become distortive prisons rather than aids to analysis, and the most important general point from ethnographic analogy—that contemporary primitives differ widely in their demographic characteristics but in all cases these are affected by a belief in the supernatural—is seldom reflected in discussions of ancient man.

There is every reason to believe that the conscious control of procreation was practiced in prehistoric times, and one can surmise that the shift from ranging to a sedentary life resulted in a rise in fertility. Early death was prevalent, but one should note also that the age at death undoubtedly varied considerably according to a people's therapeutic skills, nutritional standards, proclivity toward violence, and the like. The balance between births and deaths, the growth or decline of a population, is no easier to estimate, paradoxically, than its two components. The numbers of persons inhabiting a site at successive dates do not constitute data that archeological evidence yields easily and automatically. Such factors as migration and race mixture, often impossible even to guess at, complicate the estimation of population growth and structure.

During the past two decades a new interest in the demography of prehistoric man has developed, and one can anticipate a considerable improvement in the techniques of analysis over the next generation.

Comments

by ROBERT J. BRAIDWOOD

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As regards the Near East, I would guess that had Petersen fully realized the flimsiness of most of the available pertinent evidence, he would have been even more pessimistic. Ideally, our Near Eastern evidence bearing on ancient demography will gradually become more complete and informative. This is bound to take a long time, however, not only because of financial, logistic, and political complications but also because of the development and application of more subtle methodologies.

It is instructive to select almost any of the now available Near Eastern site reports (for early historic as well as prehistoric time ranges) and to compute the proportion of areas exposed to that of over-all surface indication of original occupation. At Jarmo, for example, where our upper-level exposures were relatively large as Near Eastern excavations go, the proportion of exposure to probable total settlement area was ca. 7% (and it was reckoned that a third of the original site had been completely eroded away). For the deepest levels, however, the proportion was only ca. 0.8%. These figures, especially the latter, hardly suggest an adequate sampling of over-all settlement plans

or even of possible house-type variations.

Furthermore, Petersen is quite right in pointing out that we cannot yet be certain, even were a complete settlement exposed, that every structure was indeed occupied at any one given moment of time. I have seen many quite flourishing contemporary villages with up to a quarter of their mud-walled structures already vacant and slumping into ruin. Even more, we have as yet no foolproof means of assessing which of the mud wall-butts hitherto excavated were in fact the foundations of walls of rooms within roofed domestic structures and which of enclosures open to the sky (courtyards, sheep/goat folds, etc.?).

Hence, I myself remain very unimpressed with applications of the 10 to 12 square meters per person rule, as regards sites in the Near East. There has been increasing need for such a sobering article as is Petersen's on prehistoric demography.

by HENRY F. DOBYNS

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Ohio State anthropologists and geographers are to be commended for involving a demographer in seeking techniques to improve interpretation of archeological data. Some danger nonetheless inheres in asking demographers to deal seriously with population data from nonindustrial societies. Some comments on my (1966) discussion of Native American population evidenced that danger. Petersen's pontifical perambulation repeatedly realizes it.

Petersen misstates one "method" for estimating past populations of primitive peoples as *postulating* sizeable decline after initial contact with civilization. Actually my technique, which he cites, estimated precontact population magnitude by reconstructing, as well as data allow, the scale of depopulation, following fundamental work by Cook and Borah (1960). They (1971:73-118) have since developed a more sophisticated approximation technique. Moreover, Petersen overlooks conscientious reconstructions of catastrophic Native American population decline in Hispaniola (Cook and Borah 1971:376-410) and Colombia (Cook and Borah 1971:411-29; Colmenares 1969; Friede 1963, 1965, 1967), war as a depopulant among New England Indians (Cook 1973), careful assessment of depopulation by the first New World pandemic (Crosby 1967; 1972:35-63), and monumental quantification of diseases and famine as factors depopulating Cholula (Malvido 1973). If "climbing estimates" of precontact Native American population are "dramatic," it is not because Borah or Dobyns strove for drama, but because Kroeber was less astute than Petersen thinks.

Because Petersen resorts to imputing bias based upon value positions he ascribes to ethnographers as a class, it seems appropriate to offer specific refutation. Petersen sees anthropologists as sharing a social-science mood that condemns as evil modern civilization, especially its "liberal capitalistic version." While I do not share a Charles Wilson view that what is good for General Motors is good for the United States, I do invest in common stocks. I admire achievements of companies and corporations which are truly capitalistic and not so tied to government as to be functionally socialistic. I have even employed the *laissez faire* language of the U.S. Constitution to provide an evaluation framework for overseas guided cultural change programs (Holmberg, Dobyns, and Vázquez 1960, 1961).

Petersen stereotypes ethnologists as regarding nonliterate peoples "with full, even fulsome, sympathy." Hard-won standards of modern research demand that anthropologists dwell for long periods among those whom they study. They inevitably develop close personal friendships with individu-

als. That does not mean anthropologists like *all* persons in a society studied, or even find the society congenial. I find six sociocultural systems in which I have conducted research definitely differentially likeable (Dobyns 1971, 1972, 1973; Dobyns, Doughty, and Lasswell 1971; Dobyns and Euler 1970; Euler and Dobyns 1971).

by WOLFRAM EBERHARD

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Petersen's article should have a sobering effect on archeologists and historians; it should also warn those who accept reports on the decimation of "natives" in the Americas and the South Seas that the reliability of some recent estimates is shaky.

I would like to raise the question to what degree religious beliefs may influence fertility. In a study of Chinese beliefs, I found that there are numerous restrictions on intercourse: intercourse in certain places or at certain times may displease certain deities; intercourse after a heavy dinner, after midnight, during a storm or an earthquake, etc., is regarded as harmful. If all the restrictions are counted, there remain hardly more than 100 days per year during which intercourse is approved. As these 100 days may not often coincide with the fertile period of the woman's cycle, customs of this kind could restrict fertility. Even modern handbooks on sex in China warn the man against having intercourse too often; the ideal number for a specific age is much lower than that given in Western books (Eberhard 1967: 204-5). A comparison of birth data obtained in Pakistan indicated that during the hot months of summer, many fewer children were conceived than during autumn and winter. It seemed that summer temperatures reduced sexual activity (unpublished field notes, 1965-68).

When Petersen says that "marriage was later than at puberty," he means, at least for India and Pakistan, that marriage in the physical sense of cohabitation was later than puberty, while legal marriage often took place earlier. In addition, I found that often the young wife conceived only after one or two years of actual marriage, perhaps as a consequence of poor physical condition.

In the discussion of depopulation, I might mention that it seems that the whole non-Chinese population of large areas of Central and South China has disappeared. Today, we do not explain this simply as genocide, but rather assume that more and more non-Chinese young women were married by Chinese men, who could, by doing so, save most of the large bridal gift they had to make when they married a Chinese woman. The non-Chinese men, then, had more and more difficulty in finding wives, and as they often could not make a living without a wife to work with as a team, would remain unmarried or migrate to the Chinese cities where, again, they would disappear as a "minority" and be accounted for, if at all, as "Chinese."

by ROBERT E. KENNEDY, JR.

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Petersen has made a significant contribution to the study of prehistoric demography through his insights into areas of research in anthropology and archeology where, from the demographic perspective, inappropriate assumptions are being made or the wrong questions are being asked. His discussion may serve a dual purpose: to stimulate anthropologists and archeologists to reconsider some of their basic suppositions on population matters and to help demographers better understand the major issues related to population with which anthropologists and archeologists

are concerned. Speaking as a demographer, I believe Petersen has carried out his task with a combination of detail and comprehensiveness which few other demographers could have achieved.

Petersen poses the question whether prehistoric demography would be improved by systematic cooperation between researchers in the area and demographers. My response is "yes, but. . ." The recurrent problem of a lack of data appropriate for demographic analysis might have been avoided, at least in some cases, if a demographer had taken part in initial decisions regarding procedures to be used to collect and organize data. But as a practical matter, it may be more feasible for anthropologists and archeologists to learn elementary demographic methods themselves than to find a demographic colleague who is available for collaboration when needed. I agree with Petersen that demographic analysis can begin with a rather modest level of technical expertise, and that such basic demographic sophistication can multiply the analytic power of any population data which may happen to be available.

by GOTTFRIED KURTH

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Petersen's paper should be interesting and stimulating not only for prehistoric demography, but also for paleoanthropology and population genetics—i.e., the approximative interpretation of evolutive processes during hominisation. Many anthropologists tend to oversimplify to the point of a kind of absolutisation the possible effect of a single factor or a few factors biologically regulating population processes. We should never forget that we can offer only working hypotheses and a scientific time-picture (*Jeweilsbild*) for any interpretation of past or present processes.

In the course of my training in physical anthropology and prehistory, including long experience in the excavation of settlements and cemeteries, I often confronted two basic questions: What was the size of the permanent population reflected by this cemetery? To what degree are the burials excavated representative of this population? The problem of reconstructing relatively correct age pyramids and reproduction rates for populations without written records or statistically correct calculations stimulated my interest in paleodemography. We started with research on villages and families and reached the 17th century for some villages and the 14th century for families. Surprisingly, the average number of offspring per reproductive family was much lower than usually believed. More important for population increase in the long run was not increase in the mean offspring per couple, but decrease in the relatively high average age at marriage and increase in the average frequency of marriage. I believe that earlier populations reached a kind of self-regulation of population mostly by regulating these factors, whether intentionally or unintentionally. I'm sure that *Homo sapiens sapiens* since the Upper Paleolithic has had enough insight to achieve a "floating balance" (*Fliessgleichgewicht*) between food supply and population numbers through social, economic, and/or ritual control of when an adult gets the permission of his group to marry and to whom. Additionally, observations of so-called primitive peoples demonstrate that groups even regulate the desirable interval between births—e.g., through coitus taboos—and have sufficient knowledge to avoid fertilization and to produce abortion.

Important in any case, also in the long run, was the mean duration of a generation. My definition of the effective duration of a generation is half the sum of the ages of man and wife at marriage plus the time until the

birth of the first child that survives until reproduction. This biologically correct calculation is independent of the time of menarche or first ejaculation and considers the (different) socially determined regulations as well as the possible losses from high death rate of infants or juveniles before self-reproduction. For any calculation of the possible tempo of evolutive processes, the length of time before the next generation offers new gene combinations for selection (generally limited, by other factors, in number) is important for the effective realization of new gene combinations.

Cultural remains never offer a basis for calculating even approximately the contemporary permanent population numbers. Unfortunately, numerable human remains also seldom offer an exact basis for the reconstruction of the permanent population. In most cases our human remains offer only a section of the contemporary population of the area. The only representative series may be those with a conical age distribution according to age at death. In this connection, we should use for comparison the age pyramids and death distributions only of preindustrial rural populations, which by 1800 had at last achieved a life expectancy for the newborn of 20 years. From this it seems relatively unimportant whether we can determine more or less exactly the age at death of human skeletal remains or not. We know that, in any case, the percentage of individuals over 50 years of age for such populations was on the average very small.

The value as evidence of human skeletal remains may be limited by, e.g., different burial customs for newborns, children, and "socially recognized" individuals and the greater probability of preservation of the larger and thicker bones of adults. If the death curve from 0 to 20 years coincides with the probable life expectancy, but we have more adults, we should regard it as one explanation that many adults may have been unmarried. Our paleodemographic research demonstrates that in most cases about one quarter of the adults over 20 remained unmarried until death. If we have a series with a probably "normal" death distribution, including about 100 individuals, with an average life expectancy of about 20 years for the newborn, and the cultural remains indicate a time span of about 100 years for the use of the cemetery, the permanent population may be calculated as about 20 persons, i.e., only a few families. In fact, the approximative calculation of cemetery use even with relatively large series of human remains indicates only a limited number for the permanent population, to the extent that the remains represent, as indicated by burial customs, not just a section of that population (see Röhrer-Ertl 1975 for discussion of this problem).

In any case, we have to consider that the regulation of population processes and numbers depended upon a complex interaction of demographically relevant factors, and that the tempo of increase of local, regional, and world population was probably for the most part, including the so-called Neolithic and urban revolution, remarkably slow. The time permitted for reproduction was limited by an average adult death apex of about 37–38 years, and the generally high death rate until the 20th year skimmed off up to 60% of all newborns before reproduction. Thus the possible remaining surplus of the birth rate remained in the long run relatively small.

In this connection, a few words on "migration": Characteristic of hominids, and especially *H. sapiens sapiens*, is a great capacity for learning. Thus a migration of cultures or languages never automatically indicates a comparable migration of individuals. We must therefore carefully

distinguish between the historically comprehensible effect of a "migration" and the real number of migrants. There are many indications that the long-term stability and biological continuity of population sequences is relatively great. This accords very well with our other paleodemographic observations and working hypotheses. For further discussion, see Kurth (1965, 1972, 1975), Kurth and Weber-Olderup (1972), Maier (1972), and Mönkemeyer (1966).

by CHRISTOPHER MEIKLEJOHN

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Petersen's article is timely and should be read by anyone contemplating work in the area of prehistoric demography. As one already involved in such work (Meiklejohn 1974a, b), however, I am surprised at how little of it is new to me. It reads more like a description of the type of work currently being undertaken than a plea for new approaches. This is especially true in the area of hunter-gatherer studies (for example, Williams 1974).

Many of the criticisms raised by Petersen refer to older approaches. This is highlighted by many of the references used. Few of us today would consider Vallois in the mainstream of work on mortality. His papers are classics. To a certain degree Petersen can be accused of shooting down straw dogs.

In other cases it is possible to query generalizations. Can style and function never be related? Flannery (1972) has argued cogently for correlations between house structure, population site density, and mobility. In the interpretation of disease, Neel (1971) has provided models for certain population types in certain environments. Does the shift to sedentary life improve diet and health? I think that this remains to be proven.

Finally, I would like to query what I consider slightly misleading allusions to accuracy in studies of skeletal age and sex. The reported figures by Petersen seem to refer to older work. With a reasonably well preserved sample, estimates of age and sex will be at the upper end of Petersen's range of accuracy. This is especially true if newer methods, such as those proposed by Acsádi and Nemeskéri (1970), are used.

by YOSHIO ONUKI

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One of the major concerns in prehistoric or archeological studies is culture process, and not a few daring studies have been appearing in recent years. Since culture process is closely related to the development of the social system, archeological studies of culture process cannot but be involved in demography. Petersen's paper is a very timely warning. His caution as to ethnographic analogy and his criticism of the idea of population-resources balance deserve attention. I fully agree with Petersen when he says that archeological data are too poor for population estimates. Thus it seems that, at the present stage of knowledge and methodology, we cannot estimate population size for any category of prehistoric time. I wish Petersen could have suggested what archeologists should do or what demographers can do with archeologists.

Archeologists perhaps do not need such exact population figures as demographers expect from the accurate modern census. There must be other ways to consider population as a factor in culture process; one example may be Sanders and Price's (1968) *Mesoamerica: The Evolution of a Civilization*. I should like to know Petersen's opinion of this book, especially the part in which population is treated as a factor in culture evolution.

by KENNETH M. WEISS

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This paper is a typical review of one field by a member of another. Many good, fundamental points are raised, but a great deal of needless confusion comes from the author's fixation upon the basic traditions and concepts of his own field along with an inevitably spotty knowledge of the literature of the other. If we use those traditional concepts to our own ends, or in ways deviating from his conventions, we are accused of error. This is regrettable, for it only causes wasteful argument. Unfortunately, I am subjected by Petersen to a condescending attack of this kind.

My monograph (1973) is accused of having cumbersome notation; this is perhaps true, although the notation is consistent, correct, and hence perfectly valid (typos and, yes, occasional carelessness excluded). I am accused of "technical pretentiousness"; since no constructive comments are given, this must be due to the "ignorance of fundamentals" Peterson "documents" in n. 2. My definitions for crude death rate, age-specific birth rate, and net reproduction are all clear, mathematically correct, and either *standard definitions found in many demography works* (if Keyfitz, Coale, and Lotka, for example, are demographers) or *equivalent alternative expressions*. The characterization of the theoretical population as infinite is but one way of conceptualizing a continuous mathematical model (i.e., what is the meaning of a continuous age distribution, $c(a)$, in a finite population?). My treatment of migration is mathematically proper and more relevant to anthropology than the standard sociology-based version (our populations have no "country's borders," but do practice reciprocal exchange). In my use of the term "growth regulation" I choose to include non-conscious factors and explain this in both sources Petersen cites. There is no substance to any of these criticisms.

Petersen is totally wrong in two accusations: (1) My "average family size" is correct as defined ("menarche" included) and of specific use to anthropologists; I use "completed family size" as well, correctly defined in the same work. Being unfamiliar with my term, he assumes I am ignorantly misusing one of his. (2) My index of growth regulation is *not* merely Lotka's 50-year-old intrinsic growth rate, as Petersen asserts, but a comparison of an observed stable rate with rates which are *intrinsic* to certain other hypothetical vital-rate schedules.

If Petersen does not like an anthropologist's work, let him criticize it in a constructive way, thus allowing a reply in kind. In fact, he offers few positive points or suggestions to lead us poor sheep from the forest. He is correct to point out that we overanalyze data, use methods which are too crude, and overlook some relevant work of real, licensed demographers. But has he any productive ideas for our kind of data? Or must prehistoric demographers merely wait patiently until our populations develop writing and grow large enough so that we can turn their analysis over to the census bureau, who will do it right?

by PAUL F. WILKINSON

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Petersen's paper exhibits some of the shortcomings that he attributes to Weiss's excursions from anthropology into demography and that perhaps inevitably characterize interdisciplinary studies.

The paper's strengths are its clear exposition of some elementary aspects of demography, with which prehistorians and ethnographers may be unfamiliar, its identifica-

tion of the shortcomings of archaeological data for demographic investigations, and the caution that it urges in using ethnographic analogies.

Its principal failing is, I think, that it does not show convincingly "the way to proceed." Given the inadequacy of archaeological data and the hazards of extrapolating from ethnographic studies, I cannot see how acquiring "the quite modest technical expertise on which demographic analysis is based" would materially assist palaeodemographers, although ethnographic studies would certainly benefit from a more rigorous approach. Petersen may be correct that demographic studies of early man have "gotten off to a wrong start by not asking the right questions," but I did not feel much surer at the end of his paper what the right questions were or how they ought to be approached.

One danger of interdisciplinary studies is that "alien" experts—in this case the demographer—show only a partial awareness of the present state of the discipline into which they venture. For example, to exhort prehistorians to acknowledge "the existence of a full spiritual life in the peoples they study" seems to me a superfluous call to a discipline that has been so obsessed with man's "spirituality" that it has all but forgotten that he is an animal. Nor can I agree that "ethnographic analogy has ordinarily been understood narrowly, as the inference from certain specific similarities in artifacts to *similarities in the material culture*" (emphasis added). The discussion of ethnographic analogies underestimates the scrutiny to which they have been subjected, most recently by Binford (1967, 1968b, 1972). To argue that "the best guard against inappropriate parallels is the fully conscious acceptance of appropriate ones" merely begs the question of judging appropriateness. Finally, the unsubstantiated assertion that "for ancient man, migration was not an occasional aberration from a settled life, but the norm" invites disagreement on account of its failure to define "migration" (are we concerned, for example, with recurrent seasonal movements within a prescribed area to permit the exploitation of unevenly distributed resources, or with unidirectional, purposive, large-scale movements?) and because it shows no awareness that this is a lively, current issue among prehistorians (see, for example, J. G. D. Clark 1966 and Adams 1968 on the special case of "invasions").

Two minor points: (1) J. Desmond Clark's (1962:1) comments are taken out of context and slightly misquoted, which, I think, distorts his viewpoint. (2) Debate will undoubtedly continue over the degree to which Childe's views were "dictated by a rather simple-minded Marxism," but it is worth noting that, in the book cited by Petersen, Childe (1963:39) criticized the Russian classification of social evolution because it "assumes in advance precisely what archaeological facts have to prove."

In summary, Petersen's criticisms are useful and worthwhile as far as they go, but I feel he underestimates the progress that archaeology has made, and I regret that he did not devote more time to suggesting more appropriate techniques and hypotheses to replace those that he demolishes so ably.

Reply

by WILLIAM PETERSEN

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As one with no pretense to professional competence in anthropology, archeology, or related disciplines, I offered

a critical review of one of their subfields with a certain trepidation. I am gratified that it has excited rather little adverse rebuttal and at least some approbation. The two most general criticisms of my paper in a sense cancel each other: that I failed to include in my review the improved techniques allegedly being used in some recent work, and that I did not round out the exposition with specific recommendations for improvements.

I was not under the impression that, among the books and papers I read, there was a steady improvement over time. My opinion of the several monographs by Cook and Borah, for example, is not as high as Dobyns's. Indeed, on the contrary, they typify the all too common indifference to demographic expertise: the authors use nonprofessional techniques to generate from dubious data conclusions that they seemingly find attractive just because of the presumed disasters. The implications of Cook's earlier paper (1946) on the population losses from pre-Columbian wars and human sacrifices were not so much balanced against this later view as abandoned. Why is it that sinologists, as Eberhard points out, generally hold that China's earlier non-Chinese populations disappeared mainly into a racial mixture, while analysts of such a country as Mexico, of which almost the whole non-Indian population is mestizo, usually take no account of this factor?

As another example, I know of no overall work on the population of classical antiquity that matches the breadth and insight of Beloch's, which was published in 1886, and I am less willing to dismiss such men as Vallois or Kroeber than my critics seem to be. Much of what is now being published, in this field as in every other one, will not survive, but the works that we now term classics have already been culled. That is not to deny, of course, that some recently published papers (including a few of those I cited) represent genuine methodological advances—though these have sometimes not penetrated even to others working on the same specific topics, not to say to the whole of the several disciplines. It would seem to be excessively sanguine to take such papers, even if one assumes that their promise will be realized, as a true measure of the current state of the art.

What is to be done? several commentators ask. I suggest again, as does Kennedy, that those who want to analyze population trends take the trouble to learn at least the elements of demography. Weiss is inclined to be defensive about every detail of his published record, and rather than go through the same litany of errors, let me suggest a parallel. Suppose that I as a demographer became interested in analyzing the relation between kin structure and fertility and, in pursuing this new interest, dipped into the large body of writings on social anthropology and eventually published a monograph in which *household* and *family* were confused, as well as *exogamy* and *hypergamy*, and in which the already complex notation that has become conventional for illustrating a kin structure was introduced to my fellow demographers (totally ignorant of the field, in the main) with some new and pointless complications of my own. I do not think such a procedure would be useful, and I hope I would react sensibly to the suggestion that I was not in control of a discipline that, after all, was not yet my own.

I had not read Sanders and Price (1968), and now that I have read it, following Onuki's recommendation, I agree that it is an interesting and competent work. Sanders, they write (p. 84), finds the estimates of population losses made by Borah and Cook "much too high and is preparing a full critique of their methodology and the underlying assumptions of their studies"; I shall look forward to reading

it. The comments that Sanders and Price make on the relation between population and social structure strike me as reasonable, and I find it especially commendable that they carefully construct typologies rather than generalizing to the whole of a culture "stage." Kurth's interesting comments, on the contrary, go from a valid point to what I find to be too broad a conclusion. The paper by Hajnal (1965) that I cited is a striking confirmation of Kurth's statement that in *Europe* the regulation of numbers was largely by shifts in the proportion married and in the age at marriage. I do not believe, however, that one can generalize from this one continent to the whole of mankind. In classical India and China, as prime examples, the "floating balance" was achieved far less by controls of marriage patterns than by what Malthus termed positive checks. I would hold, however, that Kurth is correct in one important sense: every culture of which we have the requisite knowledge includes norms or practices whose effect (though not necessarily purpose) is antinatalist.

As Eberhard points out, this is true even of traditional China, which American sociologists have typically denoted as the prototypical "familistic" society. The policy implications of his account are staggering: one can anticipate that modernization will erode the kinds of superstitions he describes and thus, in this instance, increase by three times the number of opportunities for conception. Efforts to establish family-planning programs in such countries are usually based on the contrary assumption, that modernization is wholly and unambiguously antinatalist in its effects. Not surprisingly, most of the programs have failed.

Childe did indeed make the comment on Russian archeologists that Wilkinson quotes, but I find its import quite different. Of the 20th century's two monstrous tyrants, Stalin went far beyond Hitler in his personal control of the whole range of learning, from astronomy to zoology. Except in a work on the history of ethnology, an anthropologist would have to be in a Stalinist straitjacket still to discuss seriously the theories of Morgan, not to say Engels. Judging from Childe's quotations from their works, some of the Russians seemed to be groping for a safe iota of independent judgment, and Childe compounded his deference to Stalin's scholarship by voicing disagreement with this mild dissent. My main point, however, was not Childe's politics, but rather that all too many who do not share his ideology have accepted the archeological framework partly derivative from it.

I am sorry that Dobyns chose to interpret my comments personally. The point I was making was quite general. Most in the United States who teach the social sciences, as well as many in all faculties of the most prestigious universities, are well to the left of the normal range of American politics (Lipset 1972, Lipset and Dobson 1972). In anthropology, I believe, this prevalent tendency was aggravated by a nonsensical interpretation of cultural relativism. In order adequately to study a band of naked savages, one must try to see the world with their eyes, accepting as a working postulate that this culture and all other cultures are on a par; in other words, one starts, as in much other scientific investigation, with a premise of as-if, in Vaihinger's phrase. But to accept the postulate as "a guide to the evaluation of value systems, especially ethics, politics, and esthetics, and [thus] an attitude toward practical problems of sociocultural reform and change" (Bidney 1968), strikes me as bad science, if only because it is almost certain to be dishonest. In his main works, Malinowski promulgated cultural egalitarianism as vigorously as anyone could and then showed, in two posthumous works, how completely alien this stance was to his actual thinking. The first of these books was an impassioned diatribe against Nazism (1944), which I would hold—just

because it was a vile and dangerous enemy—it was especially necessary to analyze coolly, trying to understand it "as if" one were a Nazi in order to see what attracted Germans to the cause. The second work, a diary that Malinowski kept in the Trobriands (1967), was published after his death by his widow. While in his public works Malinowski always put the hateful word "savage" in quotation marks, in this journal he was jotting down his "general aversion for niggers," his wish that he could beat up informants "without starting a row," his feeling that he would like to "exterminate the brutes." I wonder how different Malinowski was from all the other cultural egalitarians, except of course in the unfortunate publication of his private views. I wish that Dobyns, since he chose to comment on it at all, had discussed the issue in this broader context.

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