THE FOOD CRISIS IN PREHISTORY

Overpopulation and the Origins of Agriculture

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3 THE ARCHAEOLOGICAL MEASUREMENT OF POPULATION GROWTH AND POPULATION PRESSURE

Before proceeding to analyze the archaeological evidence from various regions of the world, it is necessary to spend some time discussing the methods archaeologists employ to measure prehistoric populations. I have already suggested that some of our traditional reconstructions of prehistoric events should be reconsidered. It is my contention also that standard archaeological techniques for reconstructing prehistoric populations may contain sources of error and significant biases. I think that these limitations in our existing methodology may account in large part for the tendency to underestimate the extent of population growth in the Pleistocene and the importance of population pressure as an incentive for economic change during that period.

The first, and probably the most serious, problem is the fact that at present there is essentially no methodology in archaeology designed to identify, far less to measure, the elusive concept that we refer to as "population pressure." Almost all of our established procedures deal with the measurement of relative or absolute population size or population density (size relative to geographic area); none deals directly with the relationship between population size and resource utilization patterns. The recent literature (see, for example, P. E. L. Smith 1972a; Harner 1970; Hayden 1975; Cohen 1975b) offers a number of hints as to types of evidence which might be used to measure prehistoric population pressure, but to my knowledge no systematic methodology for dealing with this concept has ever been devised.

The problem is further complicated by the fact that our standard methods for reconstructing prehistoric population size and density are themselves fairly crude. They provide only a very rough, and often

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unreliable, indication of the direction and magnitude of demographic change. Moreover, for reasons which will be discussed, these methods are particularly weak in measuring the size and density of nonsedentary populations, and thus are especially insensitive as indicators of demographic trends during the period prior to the development of agriculture. In order to defend the population pressure model, therefore, it will be necessary first to discuss some of the problems inherent in the standard archaeological methods of population measurement, and, second, to propose a number of additional types of evidence which can serve as indicators of population growth and, more particularly, of population pressure among nonagricultural groups.

Consider for a moment the standard techniques used by archaeologists to estimate the size and density of prehistoric populations. Our techniques are all built around the same basic model (see Cook 1972). First, a count is made of some parameter which is assumed to be representative of population at a particular point in time. Any number of different parameters can be used. Counts typically are made of the total number of houses or rooms in a village; the total number (or total acreage) of sites from a particular horizon or time period; the total number of shellfish or animal bones contained in single layers of a certain site; the total number of grindstones found in a village; the total number of acres under irrigation at a certain time; and so forth. At this point the method has variants; the counts may be used in one of two ways. First, they may be converted directly to absolute population estimates for the period in question by establishing the ratio of actual persons to the units counted. If five people are known or estimated to have occupied a particular type of dwelling, then population is estimated as the number of houses multiplied by five; if a cultivated acre of land supports an average of two people, then population is estimated as the number of cultivated acres multiplied by two; conversely, if one person consumes 50 shellfish in a day, then the number of shellfish divided by 50 determines the number of man-days of shellfish consumption.

The alternative method is to use the parameters to establish the relative size of the total population in two different periods. Thus, if there are twice as many houses occupied in one period as in another, twice as much acreage cultivated, twice as many grindstones used, or twice as many shellfish consumed, population is assumed to have doubled between the two periods. This latter method permits us to establish the relative size of population even if no absolute estimate of population is

available for any period. Often, however, at least one of the periods in question provides evidence that permits us to estimate absolute population size. The absolute population figures for the remaining periods can then be calculated proportionately.

This method of population reconstruction (in either of its variant forms) makes three central assumptions. First, it assumes that the relevant parameter is fully preserved and recovered (if absolute estimates of population are to be made), or that preservation and recovery of the parameter, if not total, is at least constant from period to period (if only relative population size is to be estimated). Second, it assumes that the ratio of units of the parameter to actual persons is known or at least that these ratios are constant. Third, it assumes that the various units of the parameter counted are strictly contemporaneous, or at least that they all fall within a narrowly defined time period of known length. Ideally we wish to know exactly how many rooms were occupied or acres cultivated at a particular point in time or exactly how many shellfish were consumed in a single day. Usually we settle for knowing the relative number of rooms occupied, acres cultivated, or shellfish eaten within two broadly defined periods of roughly comparable length.

This basic methodology for estimating prehistoric population, with its two variants, enjoys an extremely wide vogue. It is, essentially, the only method we have for dealing with populations for which no census data are available. The method nonetheless has a series of significant flaws. The first important problem is that each of the three basic assumptions involves some inaccuracy, and as a result the method always includes some minor degree of error. The archaeological record is only a fractional sample of past human behavior and its residue. The archaeologist certainly cannot count on total recovery, nor can he presume with any assurance that the degree of sampling error is the same for all the periods studied. There will always be some differential preservation or recovery from various periods. In addition, any assumption that there is a fixed ratio between a unit of the parameter and a unit of total population will obviously involve some error. Families vary in size, for example, so that there will never be a perfect correlation between the number of houses occupied and the number of persons in the population. Moreover, average family size may well change from period to period without any obvious change in house size, so that the populations of two different periods need not be strictly proportional to the house or room count for the two periods. Finally, absolute contempo-

raneity between units counted can almost never be reliably established, nor is absolute dating sufficiently accurate to permit us to define with any precision the interval of time the units span. We can never determine with any assurance exactly how many houses in a village were occupied at any one point in time. And, if we are dealing with a more general measure such as the relative number of rooms or houses occupied during two prehistoric periods, we cannot control precisely for such variables as the length of the two periods being compared, the length of time each room or house was occupied during each period, and so forth.

These sources of error are widely recognized and appreciated. Despite the popularity of this general method, most archaeologists do not attempt to define prehistoric population with great precision, nor would they argue that their reconstructed curves of population trends represent anything more than a very approximate indication of the shape of population growth and decline.

The second major problem with the methodology is far more significant. Aside from these constant minor sources of error, there are conditions, not always readily perceived, which can render results so completely misleading that even the general shape of the reconstructed population curve is wrong. If, for example, there is a major change in the conditions of preservation in an area, comparisons of the numbers of units of any parameter for two different periods may be meaningless. For example, as will be discussed in more detail in subsequent chapters, Butzer (1971:536) argues that the apparent decline in population in post-Pleistocene Europe, based on the observed decline in the number of preserved archaeological sites, is probably nothing more than a change in the conditions of site preservation and has no demographic significance at all. Similarly, if a major shift occurred in the cultural patterns governing the utilization of any of the items counted—a shift that affected either their preservation and eventual recovery or their numerical relationship to people—the reconstructed population curve could be very misleading. For example, the movement of a population from an area of good preservation and easy recovery to one of poor preservation and difficult recovery might very well give the appearance of a major population decline when in fact none had occurred. Likewise, if a culture changed from providing one grindstone for each nuclear family to providing one for each female over the age of twelve, the number of grindstones might increase considerably, giving a false impression of significant population growth.

The third major flaw in this methodology, and the one most relevant from my point of view, is that it is essentially incapable of dealing with nonsedentary populations. All the potential sources of error which are relatively minor when one is dealing with permanent villages become magnified enormously when one deals with temporary campsites. In the first place, temporary campsites tend to contain relatively little imperishable material. They are frequently not preserved, and they are easily overlooked by an archaeological survey. As a result, the archaeological sample of temporary campsites is likely to be much smaller and subject to far greater sampling error than is a sample of sedentary villages. In addition, with temporary campsites it is much more difficult to establish contemporaneity or measure the duration of occupation than it is with villages. Whereas two permanent villages of the same general timespan can often be determined to be either contemporary occupations of separate populations or sequential occupations by a single population, temporary campsites utilized during one or more seasons of one or more years may represent almost any number of different populations. Similarly, establishing a ratio between persons and artifacts is more difficult in temporary campsites than in permanent habitations. Do the outlines of three houses in a temporary campsite represent three families living together or the reuse of the site by a single family on three occasions? Unless the outlines actually overlap one another, there may be no way to tell. Do six grindstones in two campsites represent six families, or three families who left their grindstones behind when they changed their location?

The result of this confusion is that this methodology, which is essentially designed for sedentary sites, can be applied to nomadic groups only with great care—if, in fact, it can be applied at all.

The most serious problem occurs, however, when this same basic methodology is used to compare pre-agricultural and post-agricultural populations in any region. In this case, the results are very badly biased. Agriculture correlates roughly with sedentism in most regions of the world, and it is obvious that sedentism, whether or not it results in actual population growth, will result in marked increases in the number of sites recorded by archaeologists. Permanent sites are simply much more likely to be preserved and to be found by archaeologists than are

temporary campsites. We are left with the impression of a population explosion accompanying the development of agriculture, but this may result from nothing more than the preferential preservation and discovery of agricultural villages. Even if population growth does accelerate when populations become sedentary and begin to farm (and I am willing to concede that such is probably the case), it seems probable that this acceleration will be overemphasized in the archaeological record by the disparity in site preservation. The problem is thus a compound one: on the one hand, a methodology poorly adapted to the study of mobile groups tends to minimize our appreciation of group dynamics in preagricultural societies; on the other hand, we witness an abrupt (but possibly artificial) increase in the number of sites correlated roughly with the beginning of agriculture. The combination clearly leads to a strong bias toward the Malthusian assumption that population tends to remain stable until liberated by agriculture, after which revolutionary growth results.

Some of this theoretical bias may be eliminated if we consider a number of additional types of indicators of population growth. Although these are not quantitative indicators in the sense discussed above, they are, I believe, equally valid as measures of population change in a region (and particularly of population pressure on that region) as is site or artifact density. In fact, since these new methods do not rely primarily on *quantitative* data except in the broadest sense, it is probable that they are less subject to problems of sampling error than are the strictly quantitative types of estimates already discussed. These methods do have their problems, however, and must be used with care. Like all archaeological data, the indicators used by these methods are subject to some sampling error and can be considered reliable only if results can be replicated in many separate instances. In addition, many of the indicators which will be disucssed are subject to systematic biases in preservation. In some cases differential preservation can be expected to mimic the appearance of valid historical trends. Considerable care will have to be applied to distinguish patterns resulting from preservation from those which are of real significance.

The additional indicators to which I refer are changes in settlement patterns, food refuse, and food-related artifacts. If the archaeological record demonstrates certain changes in subsistence strategy, and if the changes demonstrated can reasonably and convincingly be assumed to

result from population pressure (or imbalance between a population and its resource gathering strategy), then the evidence of economic change itself may be taken as an index of population pressure.

Changes in the subsistence economy can, I believe, be taken as evidence of population pressure as long as certain conditions are met. First, of course, the change in the subsistence economy must be in the direction of increasing the total caloric productivity of the region under consideration. Second, if the changes are to indicate economic stress they should occur in the absence of the diffusion of new or complex technologies or the appearance of new resources in the environment; it should be clear that the altered economy is not simply a response to new opportunities. Third, it should be established that the economic shift occurs in the direction of utilization of resources which are in some manner less desirable than those they replace. For example, the new resources may involve increasing rather than decreasing per capita labor costs. Or the new resources may be calorically productive but otherwise less nutritious than the resources they replace. The new foods, though highly productive, may be foods that most people (in cross-cultural survey) consider unpalatable; or they may require extraction and preparation techniques that people, cross-culturally, consider to be particularly odious, distasteful, or demeaning forms of labor. (The latter condition might be indicated in the archaeological record simply by the fact that the resources were previously unused, although both the foods themselves and the technology for exploiting them were clearly available.)

In short, when a change in subsistence patterns cannot be shown to depend on newly available opportunities, and when the shift appears to be in the direction of calorically dense but otherwise less desirable resources, I believe that we are justified in assuming that such shifts represent compensation for demographic pressure. The following is a list of types of archaeological evidence which may be interpreted as evidence of such situations. The list is derived from the theoretical model of expanding population presented in chapter 2. Since many of these occurrences are capable of alternate explanations in particular cases, we should hope to find several such indicators occurring together to be sure that they represent population pressure. Proving that population pressure results from actual population growth poses another problem, since, as discussed in the previous chapter, disequilibrium between pop-

ulation and resources may well be caused by a variety of factors other than population growth per se. In order to prove actual population growth, we should hope to find these stress indicators occurring in situations where alternative explanations can be ruled out (where, for example, pollen profiles show no climate change), or occurring in such a general manner, widespread both in time and in space, that only a general explanation such as population growth can account for the observed pattern. The list is as follows:

1. When it is possible to isolate the exploitative cycle of a single group making its annual round, evidence that the range covered is increasing (i.e., that people are traveling increasing distances for food) should indicate population pressure. For example, if it can be shown that the travel distance from the base camp to outlying camps (other than special purpose camps unrelated to the food quest) is increasing or that food resources are being transported to the base camp from greater distances, it is a reasonable assumption that the population is encountering increasing difficulty in supporting itself on preferred foods available near its home base. Care must presumably be taken to eliminate the role of nonfood resources or other variables such as political factors affecting the pattern of movement.

2. When a group expands into new ecological zones and territories, population pressure may be assumed, especially if expansion takes place into areas or latitudes which demonstrably present new adaptive difficulties such as extreme heat, cold, high altitude, disease, or danger of predators. The emphasis here is on the word *expansion;* mere migration is insufficient. It should be established that the group both occupies its original zone and has moved into additional regions. One special example of this trend would be the adoption of scheduled transhumance by populations formerly inhabiting only a single zone. Such an economic change might be indicative of the increasing difficulty in obtaining preferred resources during certain seasons. Lynch's (1973) model of agricultural origins (see chapter 1) would fit this concept nicely. If the movement of transhumant groups has become so tightly scheduled that certain resources must be harvested out of season, then the group presumably faces significant difficulty in feeding itself in one or even a few selected locations. Here again, however, care must be taken to evaluate the role of nonfood resources or political factors as motivations for movement.

3. When the inhabitants of a region become more eclectic in their exploitation of microniches, utilizing portions of the environment—such as deserts, coastal areas, or forests—which have previously been ignored, while continuing to exploit the old niches, demographic pressure may again be assumed. It is not necessary, incidentally, that any one group span all of these niches, so long as the aggregate effect among groups of neighboring populations is toward more complete use of available space. Such a pattern might of course emerge by chance in any one region, but a wide-ranging and consistent movement into previously ignored microenvironments would seem to be significant.

4. Similarly, when human populations show a shift toward more and more eclectic food gathering patterns, shown by reduced selectivity in the foods eaten, it may be argued that they are demonstrating the need to obtain more calories from the same territory in order to feed denser populations. As in the index above, this eclectic pattern need not appear in the economy of any one culture group so long as it characterizes the inhabitants of a region as a whole. Individual human groups might even conceivably increase their concentration on particular resources, but as long as the overall tendency among the groups in a region is toward fuller utilization of all resources, population pressure can be assumed.

5. When a group increases its concentration on water-based resources relative to its use of those that are land-based, especially when the resources are shellfish whose exploitation is independent of the invention of any new technology, this shift may be viewed as resulting from demographic necessity rather than choice. (See Harner 1970; Evans 1969; Cornwall 1964; Clark 1952.) This need not imply that fish or shellfish become the dominant food. Parmalee and Klippel (1974) have pointed out that shellfish are probably not as significant as a source of food for human populations as they often appear to be in the archaeological record, nor are they as good a potential resource base as is often assumed. The point here is that shellfish are evidently low-prestige resources of last resort for a great many human populations, so that *any* increase in their utilization is probably significant. In individual cases, of course, the exploitation of shellfish may result from cultural preferences or from environmental changes such as modifications of local coastlines; but a widespread increase in their importance presumably indicates a significant stress on other resources.

6. When a group shifts from eating large huntable land mammals to eating smaller mammals, birds, reptiles, and land molluscs, demographic stress may again be assumed. Large mammals make up a relatively small portion of the local biomass in any region (Deevy 1968), but they are apparently a highly favored food in most cultures (Murdock 1968; Clark 1970). Conversely, the smaller fauna are less desirable, low-prestige items (Binford 1968), but they make up a relatively large portion of the animal biomass. A shift in favor of the consumption of the smaller fauna clearly represents the sacrificing of quality for quantity. Such a change might occasionally occur by cultural choice, but a widespread trend in this direction would appear to be a significant indicator of population pressure.

7. When a group shifts from the consumption of organisms at high trophic levels to the eating of organisms at lower trophic levels (in particular, when it shifts from meat to plant foods), population pressure may again be assumed. This change in diet will increase the consumable biomass, but it runs counter to both widespread prestige values and widespread food preferences (Clark 1970; Yudkin 1969; Murdock 1968). As with other indicators discussed above, this change may occasionally happen by chance, and its reliability as a measure of population pressure depends in considerable degree on the extent of the geographic area over which the trend can be observed. This indicator, too, is particularly sensitive to interference from differential preservation since the relative proportions of meat and vegetable foods in the diet as reconstructed archaeologically depends almost totally on the quality of preservation.

8. When a shift occurs from the utilization of foods requiring little or no preparation to foods requiring increased amounts of preparation in the form of cooking, grinding, pounding, leaching of poisons, etc., population pressure is again indicated. Such practices clearly expand the range of edible foods, but at high labor cost. This index is particularly amenable to archaeological analysis since many of the tools for food preparation are highly distinctive and imperishable. Erroneous identification of food processing tools may, however, be a serious source of error.

9. When there is evidence of environmental degradation suggesting human efforts, particularly through the use of fire and land clearance resulting in the maintenance of subclimax vegetation, it may be argued that larger human populations are increasing their interference in natural ecosystems to augment the productivity of their preferred foods. One of the primary problems here, of course, is to distinguish between environmental changes caused by man and those occurring naturally. As will be discussed in subsequent chapters, there is in fact considerable controversy about the relative importance of man as a modifier of the environment, and in particular about the role of man (if any) in the creation of open environments.

10. When skeletal evidence of malnutrition increases through time, it may be argued that demographic stresses are resulting in reduced quantity or quality in the diet available to each individual. Hayden (1975), although he is not concerned with population pressure in quite the sense that the concept is used here, does suggest that the relationship between a prehistoric population and its food supply can be gauged through the study of skeletons by measuring the actual incidence of morbidity or mortality resulting from malnutrition. The techniques for identifying malnutrition from skeletal evidence have been provided by a number of other studies (see Garn et al. 1969; Acheson 1959; Garn et al. 1968; Jones and Dean 1956). For an actual application of this measurement of stress, see Cook (1975).

11. When the size or quality of individuals exploited from a particular species shows a steady decline through time (when, for example, the size of molluscs in one or several shell middens decreases), it may be argued that human populations are consuming resources beyond their carrying capacity, resulting in the degradation of the exploited population. This index is particularly vulnerable to misuse since a variety of environmental factors might affect the size and health of exploited species. The key to its successful use would be the elimination of such alternate explanations by careful control of other related variables.

12. When an exploited species disappears from the archaeological and fossil record, it may be argued that the species was exploited beyond its carrying capacity. Here again, however, there is an enormous problem in distinguishing between extinctions caused by human agency and those resulting from other causes. This is another source of major controversy, and will be discussed in detail in subsequent chapters. One point to note, however, is that the disappearance of an exploited species from whatever cause will presumably result in disequilibrium and increased population pressure.

The preceding types of evidence are relatively straightforward and self-explanatory. In addition, I would like to suggest two other kinds of evidence which may also indicate population pressure, although in both cases the argument is somewhat more tenuous and controversial than those discussed above.

13. It has already been pointed out that hunting and gathering cultures are characterized by great fluidity in the structure of their local groups. People move back and forth easily from camp to camp. I have argued that one result of this fluidity will be widespread homogeneity of artifact styles and have called attention to a number of far-ranging artifact style-horizons in the Pleistocene which I believe are indicative of this kind of movement. Conversely, local specialization in artifact styles should be indicative of relative isolation among populations. Local isolation, I suggest, will result from a combination of two forces. First, if desired resources are scarce, the group will be increasingly jealous of its local resources and tend to become closed to outsiders. Second, scarcity, as discussed in chapter 2, will require a group to invest more and more labor in the future productivity of its resources. Since available food thus becomes more and more a function of the group's previous labor investments, the group will again become increasingly jealous of outsiders. Thus, increasing scarcity of resources should result in the gradual breakdown of the system of open population-flux. If this is true, then regional specialization of artifact styles may itself be an indicator of population pressure. (For an alternate explanation of this phenomenon, however, see the work of Wobst [1974, 1975, 1976] discussed in chapter 2.)

14. I would argue also that sedentism and the practice of artificial food storage may indicate population pressure, particularly if they are not linked to the availability of new resources or technologies, but rather are combined with intensive exploitation of old resources by traditional means. As has already been discussed (chapter 2), sedentism, despite certain advantages, implies high labor costs in the collecting of many food items and reduced dietary variety. The incidence of disease rises, and labor costs in the preparation of food for storage and in storage itself grow. Sedentism increases the threat of the loss of stored foods by rotting or rodent action; intensifies the danger of expropriation by other human groups; and, by tying it to a particular location, greatly increases the vulnerability of the population to exploitation and

enslavement by other groups. I suggest that sedentism *in most cases* occurs, not because of newly discovered resources which *permit* year-round residence in a single location, but rather because of the decline of resources associated with other parts of the traditional annual cycle, or because of territorial impingement by other groups. Either of these events would make it necessary for a group to stay in one place and stretch the resources of a particularly productive season to cover those other periods of the year when seasonal foodstuffs are no longer sufficient. In defense of this argument I would point out that the early history of sedentism (see chapters 4, 5, 6) is closely linked with the exploitation of resources (water-based resources and vegetable foods) which appear to have been largely ignored by earlier human populations in the same environments where sedentary communities later emerged. Moreover, these foods are today widely regarded as low-prestige "necessary" foods rather than as desirable ones. This picture is much more consistent with the assumption that human populations settled down out of necessity than with the assumption that sedentism was the strategy of choice.

It may seem paradoxical that population pressure can be cited as the basis both for sedentism and for seasonal transhumance, but I suspect that such is in fact the case. Under ideal conditions of low population pressure, human groups presumably adopt a fairly informal pattern of small-scale movements, which maximizes their harvest of the most favored resources without committing them either to the problems of sedentism or to the rigors of tightly scheduled seasonal movements. Increasing population pressure might well force human populations either to begin relatively rapid movement or to assume a sedentary posture, depending on the distribution of resources and other human groups on the landscape. In some environments, particularly those characterized by migrating game populations or rapid seasonal progression of resources (as in mountainous terrain), pressure would eventually force human groups to move seasonally. In regions where abundant but low-quality foods are available seasonally, population pressure might force a group to rely more and more heavily on these secondary foods and ultimately to settle down and develop storage facilities for their maintenance.

As I have tried to indicate above, none of these lines of evidence is alone sufficient to demonstrate population pressure, since all are sus-

ceptible to alternate explanations. But several occurring together, all indicating behavior that runs counter to the manner in which we would expect human populations to act by choice, surely must represent attempts to adjust to the need for more calories or to the loss of traditional food sources. Moreover, if these behaviors occur widely enough in time and space, and thus separated from particular events of climate change or other localized variables, population growth would seem to be the only plausible explanation.

If we now retrace the Pleistocene prehistory of the various regions of the world, watching not only for changes in site density, but also for changes in settlement patterns and economic strategies of the type outlined above, a picture of prehistoric culture change emerges which differs significantly from our traditional models. In particular, population growth appears to be much more continuous, and population pressure much more significant as a motivation for culture change, than is usually assumed.

4 THE CASE FOR THE OLD WORLD

If we review prehistoric events in the Old World in a broad enough perspective, noting changes in subsistence and settlement strategy of the type outlined in the previous chapter, a reasonable case can be made that agriculture is the culmination of a buildup in population pressure which embraces the whole hemisphere. Although there are significant local variations resulting from local climate phenomena or random cultural trends, the buildup of population pressure is observable in almost all the major regions of the Old World, and the observed patterns are surprisingly alike. This growth in population pressure appears to be synchronous throughout the Old World, suggesting that population flux mechanisms were acting with considerable effectiveness. Most important, perhaps, the buildup seems to be a process of considerable duration. Indicators of population pressure of the type described begin to accumulate well back in the Pleistocene, suggesting that the process of economic change that culminated in agriculture was not simply a post-Pleistocene event.

Very briefly, the archaeological record suggests that agriculture occurred throughout the Old World (1) when human population had completed an expansion from tropical to temperate and finally arctic latitudes; (2) when, starting with a high degree of selectivity in his choice of niches in each latitude, man had begun to exploit an increasing number of different habitats; (3) when the density of occupation sites had increased in most regions; (4) when the selectivity of man's diet had been greatly reduced and broad-spectrum consumption had become the rule; (5) when aquatic resources were being increasingly exploited almost everywhere; (6) when there was a growth in the consumption of small animals rather than big game animals; and (7) when increasing quantities of vegetable foods, including those involving complex preparation, were being eaten. In addition, although possibly less significant, agriculture appears to have occurred in a context in which many people had already become sedentary and in which artifact styles