Distinguished Lecture in Archeology:
Beyond Criticizing New Archeology

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It is a great honor to have been invited to give the association’s Distinguished Lecture in Archeology; a tradition that has been made truly distinguished by the examples set by my predecessors: Charles Redman (1991), Bruce Trigger (1991), and Elizabeth Brumfiel (1992). The tradition may be short, but it has started off with high standards. I hope I can maintain them.

Individually, each of my predecessors has emphasized different things, but I see important unifying themes revolving around “processual-postprocessual” debates. All agree that the “new” or “processual” archeology of the 1960s and 1970s achieved important things but also had important shortcomings. All see, within the quite heterogeneous movements of the 1980s broadly characterized as “postprocessual,” a mix of highly appropriate criticism, important concepts neglected by main streams of processual thought that need far more emphasis, and some very problematic views that can only impede archeological development.

I share this general outlook, and I will briefly outline my version of good and bad features of preprocessual, processual, and postprocessual approaches. However, I want to carry our discourse further, and I will not dwell long on points that have been adequately (many would say more than adequately) made in debates of the past decade. I argue that the central shortcoming of processual thought has been its adoption of acutely unsatisfactory theory about social phenomena; acutely unsatisfactory because it is excessively materialist and drastically underconceptualizes the individual actors from whose behavior large-scale social phenomena emerge. A virtue of postprocessual thought has been that it, or at least some of its varieties, looks to far better sociocultural theory for guidance. This, however, leads to a severe problem, if not an outright crisis. If we cannot avoid taking account of some aspects of the reasoning, sentiments, and emotions of ancient individuals, how in the world can prehistorians get adequately secure knowledge of these properties? Various ways of dealing with this problem have been proposed, but I do not think much real progress has been made. My central objective is to move us a little further toward effective responses to the difficulties we face.

Achievements of Processual Archeology

I will sketch in a paragraph my version of a history to which Trigger (1989), Willey and Sabloff (1980), and others have devoted sizable portions of books. In the days before processual approaches became prominent, three especially serious deficiencies were widespread in the theory, concepts, and aims of archeology, at least as practiced in North America, although there were some important exceptions. One was an overconfident...
belief that relatively unexamined “commonsense” knowledge, concepts, and reasoning were enough to allow one to make many secure and transparently “obvious” interpretations of ancient remains. Another was the contrastingly skeptical notion that if one wanted to venture beyond these obvious and commonsensical interpretations one had no choice but to enter the realm of untestable speculation. Finally, little visible effort was devoted to using archeologically gained knowledge to suggest, test, or otherwise improve generalizations about sociocultural phenomena.

A principal achievement of the new archeology in the 1960s and 1970s was its effective criticism of these shortcomings. Archeologists in North America and in many other parts of the world were made more aware that common sense is often not enough, and more willing to believe that, by means of appropriate theory and methods, we can go far beyond the commonsensically “obvious,” without being forced into idle conjecture.

**Shortcomings of Processual Approaches**

For both good and bad reasons, many archeologists did not adopt all aspects of processual thought wholeheartedly, and there was always a certain amount of criticism. However, the volume and influence of criticism increased greatly in the 1980s, especially in Britain. The term “postprocessual” covers a variety of newly emphasized approaches that are diverse in many ways but that all explicitly distance themselves from processual thought and practice. Some of these criticisms have great merit; others are exaggerated or problematic, and I think some are quite wrongheaded. In my opinion, processual approaches suffer from four major genuine defects. First, the emphasis on formal and computer techniques, though excellent in principle, has been characterized by startlingly bad practice and by expecting too much from sheer number-crunching. Second, new archeology adopted (or at least paid lip service to) philosophical models for theory and practice that are now seen to be problematic and that, in any case, were actually not followed very well. Moreover, there was excessive confidence in the security that could be imparted to knowledge simply by following the right prescriptions for investigating and thinking. Third, at least in the earlier years, new archeologists paid insufficient attention to problems that arise in trying to connect archeological observables with the entities that interesting theory is about. Fourth, and this is the most serious problem, processualists tended to adopt profoundly unsatisfactory sociocultural theory.

**Remedies for Problems with Formal Techniques**

The remedy for deplorable quantitative practice can be complex in details but is simple in principle: gain competence and don’t expect too much too easily. I have discussed this in more detail elsewhere (Cowgill 1986, 1989) and will not spend much time on it here. There is a large body of mathematical, especially statistical, knowledge of which we can make good use. To be sure, this knowledge is neither complete nor entirely without internal controversy. Moreover, we do not yet have a very large stock of concepts and techniques that are adapted as well as they might be to some important archeological situations and tasks. Nevertheless, what we mainly need is to become more knowing and judicious consumers of mathematical products, neither too credulous nor too skeptical. There is also a continuing need for creative collaboration with experts.

There are, broadly, two roles in archeology for formal approaches. One, which relies heavily on statistical techniques for description and inference, is primarily concerned with helping us to see how our relatively “raw” and obscurely organized experience of remains of the past can serve as telling evidence for or against different stories about the past. Used in this way, formal techniques become an important part of the complex mental apparatuses in terms of which we can debate and bolster the security of interpretations that are by no means transparently obvious.
Some postprocessual lines of thought turn away from quantitative approaches, equating them with a distanced, dehumanizing scientism that is seen as a tool of evil strategies for domination over others. However, not all stories about the past are equally true, and no story that is very unlikely to be true can be a good story, no matter what its other merits may be. Quantitative techniques can be treated lightly only if one believes that remains of the past themselves have little bearing on our judgments about the merits of different stories. The processual mistake is not in overestimating the necessity of the quantitative, but in underestimating its insufficiency. Remedies come not from neglecting it, but from having clearer ideas of what one should be quantitative about, and why, as well as by giving more attention to qualitative matters. For postprocessual as well as processual approaches, quantification cannot be avoided and it cannot be done badly without undermining everything else.

The second major use for formal techniques has been in the actual expression of theory, especially in the form of models or simulations of sociocultural phenomena. Here, my attitude is more equivocal. I have not been attracted to abstract systems approaches, and most simulations I have seen tend to leave out or seriously misrepresent much that I think is important (Brumfiel 1992 provides a more extended critique of systems approaches). However, I do not think that simulation is an ineluctably flawed approach. Simulations that make more appropriate assumptions about social phenomena are likely to become increasingly useful parts of our intellectual tool kits.

It is interesting that when postprocessualists do wax quantitative their practice can be remarkably similar, defects and all, to that of the processualists (Shanks and Tilley 1987a:172–240). Whatever postprocessualism is a remedy for, it does not seem to be a cure for what Scheps (1982) labeled “statistical blight.” I suspect the central problem is that, in spite of processual rhetoric about the importance of quantitative techniques and the sporadic denunciation of particularly awful examples (Hole 1980; Scheps 1982; Thomas 1978), there has never been a sustained effort among archeologists to connect statistical competence with professional rewards and penalties. There is still very little peer pressure to get quantitative matters right, and it is still easy to arrive at the status of professional archeologist with a slight and seriously faulty understanding of fundamental concepts.

**Remedies for Problems with Philosophy**

A number of writers have discussed problems arising from processual misuse of or poor choice of philosophical guides (e.g., Gibbon 1989; Kelley and Hanen 1988; Pinsky and Wylie 1989; Salmon 1982; Watson 1991; Wylie 1985, 1992a, 1992b). Many issues are unresolved and remedies are not so obvious as those for misuse of quantitative techniques. This is not my main focus here, and I will simply say that yes, it is true that there have been serious conceptual and technical defects in processual philosophizing, and yes, it is true that personal subjective aspects are involved in all research and there is no such thing as being wholly “objective”; but no, this does not mean that it is either desirable or necessary to regard the past as entirely a product of the present, about which we can tell any story that suits us. The kernel of truth in the postprocessual emphasis on multiplicity of stories is that the past was too complex for any story we could tell or comprehend to get it all. No one story can tell the whole truth, and we have to make choices. This a far cry from thinking that there are no limits on what can be told for true, or that all stories are equally untrue. Archeological remains have a strong bearing on what we can believe about the past. We can no longer imagine that we are doing nothing but passively responding to constraints set by evidence whose full meaning is transparent to all reasonable beings, but, emphatically, neither can we think that the remains might mean anything at all.

The central recognition is that the results we get depend on something within us (our individual psyche), and on something outside us (archeological and other remains).
Change either the psyche or the remains, and the results will be different. This knowledge does not, however, point to extreme relativism, to the idea that the product of any mind is as good as that of any other mind. The painter's knowledge that different brushes lead to different results does not suggest that all results should be on a par; it suggests that one should look to the quality of brushes. Similarly, if our minds are exceedingly important but problematic instruments, then it will pay us to improve our minds and to understand ourselves much better. We need to become far more sophisticated about the complex interplay between what we bring within us to a situation and what, from sources outside ourselves, the situation presents to us.

I will not pursue philosophical issues further here, except to urge that one very important aid to improved thinking is to learn more about the "Bayesian" approach to statistical inference. The classical approach goes to extraordinary lengths in the attempt to extirpate all subjective elements, but it only succeeds in sweeping them under the rug (Urbach 1992). It also succeeds in baffling most students in statistics courses, and often leads them to reason incoherently when they try to apply formal statistics (Cowgill 1977). In contrast, in the Bayesian approach, one's prior knowledge and beliefs can often be built explicitly into the equations and can thus be made overt rather than covert. One can then formalize the interaction between prior beliefs and new data. The approach is not without technical problems and it by no means resolves all the important philosophical issues, but it is a great improvement over classical statistical models for reasoning. It is odd that so few archeologists seem aware of it, although there are important exceptions (Buck and Litton 1991; Buck et al. 1991; Buck, Litton, and Smith 1992; Chernoff 1982; Cowgill 1989; Kadane and Hastorf 1987; Litton and Leese 1991; Read 1975). Iversen (1984) gives an excellent short introduction to Bayesian statistical computations. From a philosophical viewpoint, Earman (1992) provides an up-to-date discussion of Bayesian strengths and unresolved problems for scientific confirmation.

**Connecting Observables with What Interests Us**

Early processualism tended to pay inadequate attention to problems in connecting what is observable archeologically with the past phenomena that interest us. This has long since been recognized by many processualists, and much has been done toward remediying it, known, among other things, as "middle-range theory building" and "behavioral archeology." Even within the realm of past material phenomena, a tremendous amount remains to be done, but the general principles seem quite clear. We must recognize that archeological finds do not become evidence of anything interesting until they are interpreted in the light of things we already believe. It follows that finds must be interpreted in terms of beliefs that other archeologists of a wide variety of persuasions will accept as relatively secure—certainly far more secure than the contested beliefs that are to be evaluated. For example, finds purported to be remains of ancient fortifications will not be good evidence bearing on some theory of ancient warfare unless there is general agreement that the remains in question really were fortifications.

A second crucial point is that the knowledge *used* to connect present observations and past entities must be independent of the interpretations or theories that are to be *evaluated* in terms of ancient phenomena. The beliefs we rely on in converting remains into relevant evidence must not presuppose the truth or falsity of whatever it is we are trying to establish or test. The one set of ideas must be logically independent of the other set (Wylie 1992b). It is fine to interpret remains of a ditch and adjacent embankment as a fortification because of their resemblance to other objects known historically to be fortifications and their dissimilarity to other objects known to be something other than fortifications. However, it would be viciously circular to interpret the remains in question as fortifications or not fortifications because of the predictions of the theory about warfare that is under evaluation.¹
Stated in terms of my example, these principles of security and independence look starkly obvious and are analogous to some practical rules of carpentry: don't sit on the limb you are sawing off or stand on the plank you are trying to lift. I suspect that it's much trickier in actual research. One reason is that, in practice, some propositions of middle-range theory that seem very secure to some archeologists will seem highly contestable to others. A second reason is that, when we really trace out all the logical ramifications of all the middle-range theory needed to convert finds into evidence in a given case, we may find it exceedingly difficult to disentangle them from all ramifications of the propositions we wish to test. That is, the picture of a neat separation between two sets of propositions (those being used and those being evaluated) may be exceedingly hard to achieve in practice. Then again, it may not. The principal thing is to recognize this as a strategy we should strive to follow, and see how well we can make it work.

Better Sociocultural Theory

Archeologists tend to underconceptualize the past. Our mental images of past things tend to be impoverished. It is useful to think of four levels of underconceptualization, although they correspond only very roughly, if at all, to stages in the historical development of our discipline. Probably most publications would defy neat assignment to a single level. On the lowest level, the mental images are of pots and other artifacts occupying particular locations in space and time; people are just invisible. On a somewhat higher level, persons exist but they have no individuality; they are the "faceless blobs" of Ruth Tringham's (1991) memorable phrase. They are thought to do what they do because of shared cultural norms or in response to necessities of survival and reproduction in a given ecological and technological setting. However, they are not seen as planning and thinking actors who pursue goals in addition to survival and reproduction and who act and interact in relation to one another.

A third level of conceptualization puts individuals into the picture, as persons making rational choices in pursuit of goals. Unlike the first two levels, which are patently inadequate for understanding ourselves or contemporary societies, these "rational-actor" models are quite popular among sociologists, economists, and political scientists, such as Gary Becker (1981), James Coleman (1990), and Michael Hechter (1987). The general viewpoint goes back at least to the 18th century and is well represented by some of the writings of Adam Smith and the Utilitarians. The focus is on individuals, although a central tenet is that macrolevel social phenomena are not simply summations of individual actions; they possess "emergent" properties that are consequences of what individual actors do, but that cannot usefully be characterized in terms of those actions.

There is no question that rational-actor models are, up to a point, extremely useful and enlightening. They are a necessary step toward adequate social theory, but not sufficient. Earle (1991) gives a fine statement of their power. He complements pure economic rationality with what he calls "cultural rationality" and some important principles of group solidarity. I think his approach would fit comfortably within concepts of rationality used by Hechter (1987). The central difficulty with these models is that they wildly underconceptualize our constantly experienced knowledge of the complexity of human mental processes and action. This is readily acknowledged by their advocates; the defense is that it is a useful fiction that enables them to generate manageably simple models that, in spite of that simplicity (and in part because of it), come up with enlightening insights and encouragingly accurate predictions.

I would be less confident in my criticisms of rational choice models if I did not know that many sociologists and other social scientists are also highly critical of them. Even within a positivist framework, Moe (1979) demolishes claims by Friedman and others that the rational-actor assumption has the logical status of a covering law. Many others acknowledge that it captures some important aspects of what lies behind human actions but emphasize that it leaves out far too much and has quite limited predictive perform-
ance (e.g., Etzioni 1988; Frank 1988, 1992; Mansbridge 1990; Smelser 1990; White 1990). Among archeologists, Shennan (1991) provides a good characterization of strengths and limitations of rational-choice models. Typical objections are that rational-actor models do not acknowledge the importance of emotions and neglect the role of moral sentiments. Most of these critics do not seem particularly radical politically and are not at all postmodernist in outlook or style. This should make their criticisms of strict rational-actor models carry weight even among archeologists who are not very happy with postprocessual approaches.

Neither the rational-choice theorists nor the critics I have cited above have much to say about Giddens or Bourdieu, two writers frequently cited by archeologists. At least some rational-choice theorists do not find Giddens useful. Nevertheless, Giddens and Bourdieu are worth our attention (e.g., Giddens 1976, 1979, 1984; Bourdieu 1977, 1990). Especially useful are Giddens’s related notions of “structuration” and “duality of patterning”; macro-scale patterning is the medium within which and by means of which individual action occurs, but, at the same time, macro-scale patterning is produced (and reproduced or changed) by means of action. This patterning is constraining, but it is also enabling. The social milieu provides possibilities as well as constraints, and it is by means of individuals’ structured social situations that greater or lesser degrees of power can be exercised. A volume edited by Bryant and Jary (1991) is a recent appreciation of Giddens’s work.

The upshot of all this is that we need a fourth level of complexity in our conceptualizations of human actors. We are at least somewhat rational quite a bit of the time, and this must never be overlooked, but it is often not a strategically useful simplifying assumption to postulate actors whose behavior, for all practical purposes, is guided only by rational calculation. It is quite true that the introduction of emotions, sentiments, and unthought responses sometimes (but not always) complicates matters so much that quantitative predictions are virtually precluded. But what good is a model that permits generation of numerical predictions if the predictions are not very good? Better vague and weakly quantitative predictions that are usually right (e.g., “practically none,” “some,” or “a lot”) than predictions that have a more exact look but are often wrong.

Archeologists who explicitly stress the importance of seeing ancient remains as the outcome of behavior by conscious, knowing actors, negotiating and strategizing in the pursuit of diverse aims, do not always make it clear whether they are, perhaps unknowingly, adopting some version of a “rational-choice” model. I think this may often be the case with “optimal foraging” approaches to hunter-gatherers, but it also finds its way into thought about much larger and more complexly organized societies, including states and empires. Even when internal diversity is acknowledged, the emphasis may be almost entirely on the contrasting situational logics of persons who differ in age, gender, class, or faction (e.g., Brumfiel 1992).

I repeat, however, that taking account of rationality is extremely valuable. It is a vast improvement over not thinking about individuals at all, or thinking of them as dominated by cultural norms or ecological imperatives. It has been a strong element, at least implicitly, in a good deal of my own thinking (e.g., Cowgill 1988). I plan to continue paying it a great deal of attention, and I hope others will also. I would be sorry if my claim that it is not enough were twisted into a supposed warrant for neglecting it. In fact, I think we have hardly begun to realize its potential.

Nevertheless, if we emphasize only rationality, we are in danger of reinventing neoclassical economics, at a time when those ideas are sharply contested within economics itself, as well as in other social sciences. We cannot achieve really satisfactory understandings of many human phenomena unless we take more than rationality into account—we often must conceive the minds of actors in more complex terms. We have gotten rather good at thinking about many aspects of the material worlds in which people of the past found food, shelter, mates, allies, and enemies, and about how they interacted with material things—making, breaking, using, losing, giving, and taking
them. In contrast, the ideational realm remains shadowy. It is important, but not at all well mapped, and we tend to move around in it without really having our bearings. I want to offer a trial delineation of this realm. This delineation is far less detailed or thought-out than our picture of the material realm, but I hope it will usefully sharpen some concepts.

**Toward Bringing the Ideational Realm into Focus**

So far, I have talked about "rational choice" without defining it very closely. Roughly speaking, rational-choice models postulate rather isolated individuals who have fairly extensive information about their situations and who choose, among available options, those that can be expected to maximize their personal "utility," where utility is some global measure of satisfaction in which various outcomes are ranked in a given schedule of preferences or "tastes." Within this framework, a number of problems arise. A relatively minor complication is that sometimes the preferred strategy may be something other than maximizing expected gain, such as minimizing risks of loss. Altruism is a more serious conundrum as long as self-interest is the guiding principle. We can postulate a taste for seeing others made happier, so that one gains high utility through serving others. But, as has been pointed out, this line of thought can explain any behavior by postulating the appropriate taste (setting fire to oneself evidently provides utility through satisfying a taste for suffering), and therefore explains nothing. Sociobiologists can explain altruism toward close kin as an indirect way of fostering the reproduction of one's own genes, but this leaves altruism toward nonrelatives unexplained. Frank (1988), without moving very far from a narrow rational-actor model, argues ingeniously that one's interests are well served if one is perceived as trustworthy, and that the best and ultimately least costly way of being perceived as trustworthy is to want to be trustworthy, to prefer to do the "right thing" even in situations where one's immediate interests would be better served by cheating. Or, by being perceived as a person who gets angry enough to seek redress for injuries even when it is irrational because the cost of the effort is greater than the possible compensation (e.g., costly litigation over a small sum), one stands a better chance of rarely having to seek redress. Thus, Frank explicitly complicates rational actors by taking account of some of their sentiments and emotional propensities.

A deeper problem is that of explaining different tastes, or changes in tastes. Often these are taken as fixed and external to the model, so that questions of explaining tastes are simply begged. But even if tastes are not considered fixed, changes in them are hard to deal with unless actors are thought of more complexly.

However, the greatest problem of all is that rational-choice concepts so wildly misrepresent humans. I will not belabor points about false consciousness or pursue very far the notion that the very concept of rational choice is an artifact of thought in the service of capitalism—it would be easy to vilify it excessively and oversimplify its roots. Nevertheless, it is such a one-sided representation of my lived experience that I have been tempted to compliment rational-choice theorists on their modesty about their amazing but unheralded discovery of extraterrestrials; creatures compared to whom Mr. Spock and other Vulcans are mushy-headed, touchy-feely types.

Enough of expostulation. So we experience emotions and hold moral sentiments that often influence our actions, and often we act without thinking, either from habit or impulsively. How can we make these ideas a little richer and more structured?

I suggest some terms, not for the sake of jargon, but because I hope they will help our thinking. Humans have many *nonrational propensities*. I am careful to say "nonrational" rather than "irrational" because I do not mean that they are all opposed to rationality—most of them are simply different from rationality. One thing I mean by "propensity" is a tendency or predisposition toward something. A propensity toward anger is different from anger; it means that a state of anger is easily aroused. But I also have in mind other
mental properties and aspects of properties, such as what one is likely to be angry about, or how fast one thinks or how much one can readily remember. Perhaps "propensity" is not the best label for this concept, which is a catch-all for nearly all mental phenomena or processes except rational calculation in pursuit of utility.

I think our nonrational propensities are broadly of two kinds. One kind is universal, shared among all humans; indeed, shared to a considerable degree with many other mammals. A fine example are facial expressions of emotions. This is a topic surprisingly little studied, but the psychologist Paul Ekman (1985), whose popularly titled book belies the seriousness of his research, provides fascinating evidence of how widely shared they are and how hard they are to feign or conceal from a skilled observer. Another well-known example, shared by all humans but significantly not shared with other species, is the propensity of human infants to learn whatever language they are exposed to.

I am not ready to give anything approaching a complete or well-organized list of major kinds of universal nonrational propensities, but I offer a few more examples. There are the familiar strong emotions; joy, grief, rage, lust, disgust, panic, and so on. These all have a bearing on actions, as do boredom and trance and the mental concomitants of bodily states such as hunger, fatigue, injury, illness, and their opposites. Most people rarely experience any of these in intense forms for very long, so it might seem that, on the average, they do not matter much. However, exceptional actions in exceptional states of mind and under exceptional circumstances are disproportionately important at critical junctures in human affairs.

More enduring or chronic propensities that are probably universal include the propensity to emulate individuals perceived as highly successful or highly attractive, irrational degrees of group solidarity, irrational alienation from other groups, the susceptibility to romantic love and other "infatuations," and the propensity to attribute special properties to a category of "sacred" things. There is no room here to "unpack" any of this; I only hint at some topics that need more examination.

Although the specifics are wildly obscure, universal nonrational propensities must have biological bases in our nervous systems, and they are undoubtedly influenced to a significant degree by the genetic makeup of individuals, although it is a truism that the importance of genes versus socialization and other experiences remains very poorly understood. I am scarcely breaking into any new intellectual territory by saying this. The relevance to my main arguments is simply that if good theories of society must take account of actors who have nonrational propensities that interact with their propensities for rational calculation, then empirical explorations of nonrational propensities can no longer be ignored—they become essential for adequate concepts about societies.

Even though these nonrational propensities are probably universal, they are strongly influenced by socialization and social learning. Many of these influences are relatively obvious and direct; moral sentiments are inculcated, beliefs about and attitudes toward people and the rest of the world are learned, some emotions are encouraged and others discouraged, and different modes of expressing or acting upon emotions are rewarded or penalized. In other cases, the teaching may be less direct and intentional, but children will be influenced by other children and the adults around them. Again, this will seem crashingly obvious and familiar in general terms, but the specifics warrant a great deal more research. For example, Fry (1992) documents startling differences in aggressive behavior between nearby Zapotec communities in Oaxaca, and convincingly argues that, although other factors may also be at work, differences in social learning are important.

Unintended consequences of early socialization are probably also important. This idea is not so fashionable nowadays. Problematic claims were made in the past for relatively strong and uniform consequences of infant swaddling or various weaning or toilet-training practices. I do not think these problems discredit the general concept
that experiences in the first few years of life can have lasting consequences for hard-to-change aspects of adult personality.

I suggest that all these facets of nonrational propensities that depend on historically contingent social learning and other experiences and thus tend to differ from place to place and time to time be called local nonrational propensities. The universal nonrational propensities of ancient actors are, by virtue of their universality, accessible to us. But, to the extent that their local nonrational propensities were different, we face a very difficult problem. This is distinct from, and perhaps not very closely related to, the problem of historically contingent meanings, about which Hodder (e.g., 1991) has written extensively. Incidentally, a propensity to act rationally is itself a nonrational propensity. If the reader has trouble with this point, it is because I have not made myself clear.

I do not suggest that differences in local nonrational propensities are the primary reason for different historical trajectories in different places. Nevertheless, I think they are too important to be ignored in good explanations. For example, however problematic Weber’s (1958[1904–5]) original formulation was in The Protestant Ethnic and the Spirit of Capitalism, the central question that he raised has not gone away. In a similar vein, Kroeber (1963:102, 106) mused inconclusively but intriguingly about cathedrals, counterfeit clocks, calculus, and credit as a possibly related syndrome in the mentality of (at least some) early modern Europeans. We should take seriously the idea that the rise of capitalism and the industrial revolution in Europe are not wholly explained by technology, internal social relations of production, and external core-periphery relations. It is possible, perhaps likely, that increasing prevalence of individualistic and entrepreneurial and “rational” styles of thought and feeling, diligence, and a fascination with machines for their own sake were also important. Once the system got going, these propensities may have become still more prevalent because those who exhibited them were equipped to be unusually effective actors in the new system, which had not necessarily been the case previously, and these successful persons were emulated by others. Emulation of persons perceived as highly successful is probably a universal nonrational propensity, as I noted earlier (Earle 1991) connects it with group identification, and it would induce even those who did not possess the traits as a result of early socialization to attempt to acquire them and/or inculcate them in their children. However, it is not at all clear that these propensities originally began to become prevalent as direct or predictable consequences of earlier socioeconomic systems. Admittedly, I am not prepared to discuss alternative explanations for the onset of their increasing prevalence.

A third part of the ideational realm constitutes what I call local rules. By this I mean ideas about how things are to be done that are fairly widely shared in a given place and time, at least among those of some particular age, gender, class, and factional category. It covers the range from obligatory law to much less binding standards of “proper” or “normal” behavior, includes various kinds of style, and spans the range from fully articulate to altogether inarticulate. The concept of local rules could usefully be subdivided, but for the present it stands as a broad category in contrast to individual propensities. Individuals act in the context of, with regard to, and often by means of, local rules (among other things). Local rules are often modified, intentionally or unintentionally, in the course of action. To the extent that action is rational, local rules are part of that in regard to which one is rational. To the extent that action is unthinking, local rules supplement instincts in providing schemata to be followed. To the extent that action is driven by emotions and sentiments, local rules play a role in helping shape socialization processes that affect attitudes toward emotions and preferred modes of their expression.

Local rules are similar enough to culture to make the introduction of a different term perhaps seem gratuitous. However, accepted usages of culture include considerably more than local rules, and perhaps in some ways less. Culture encourages us to think of an
inchoate mass of ideational phenomena (and often enough material phenomena as well), while I hope that local rules will encourage a narrower and more analytical focus.

Implications for Archeology

If we cannot properly understand the past without taking serious account of the minds of ancient individuals, we face disturbing problems. Friends of postprocessual approaches are embarrassed by the shallow and unconvincing efforts to deal with prehistoric situations that have been produced so far. Reasonably satisfying examples come from historically documented situations. The obstinate fact is that it remains much harder to make convincing inferences about what or how ancient people were thinking than about what they were eating, where they lived, to whom they were biologically related, or how much of what kinds of objects they moved from one place to another.

One purported solution, of course, has been to argue that what they were thinking doesn't really much matter; even if ancient people had thought and felt differently, all the important things would have happened pretty much the same anyhow. Another is that, given the circumstances, there is only one way any humans would have thought. A variant is the argument that because it is excessively difficult to get at ancient ideation we should build theories that do without it. This is logically akin to the drunk who loses his keys in a dark section of the street but hunts for them under a streetlight, because the light is better there.

I see three approaches to the impasse created by acknowledging both the importance of ancient ideation and the difficulties in making good inferences about it. One is to ignore the difficulties in practice and claim that one has actually succeeded in making very fine inferences about ancient mental phenomena. The trouble with this approach is that those who make such claims convince hardly anyone else, not even those who would like to see persuasive examples.

A second approach argues that everything depends on the observer anyhow, and, hence, one interpretation is as good as another. Or, more carefully, all the relevant criteria for judging the merits of an interpretation pertain to the present rather than the past. Rightly or wrongly, this view has been associated with Michael Shanks and Christopher Tilley (1987a, 1987b). It has been pointed out repeatedly that, taken literally, it leads to meaningless relativism. Shanks and Tilley reply that, of course, they didn't mean it that literally, and that they do take seriously the notion of a real past, whose fit with our interpretations it makes sense to evaluate (Norwegian Archaeological Review 1989). But, to the extent that this is correct (and I am sure it is), one is back face-to-face with the problems of making a good case that one's inferences about ancient ideation are usefully close to being right.

I advocate a third approach, which is to acknowledge the unavoidable difficulties, roll up our sleeves, as it were, and try to deal with them. This leads to three suggestions: First, the argument that rational-actor models are too simple and that we must take serious account of nonrational propensities need not mean that nothing short of Tolstoyan or Proustian complexities will do. Do we really need to know a great deal about ancient actors before we can make any secure inferences about anything interesting? How much can we get away with not knowing about ancient ideation? Tringham (1991) introduced the concept of “faceless blobs” to refer to acutely underconceptualized ancient actors. I hope that this term will be one of those that catches on and forever affects our thinking, much as did Kent Flannery’s “telephone booths” a generation ago. To pursue the trope, it is perhaps not necessary to replace faceless blobs with subtle and nuanced portraits comparable to the Mona Lisa (the more so because this portrait is famed for its inscrutability). Hodder (1991) seems to argue that we must know a great deal about what was in people's minds before we can get anything else about the past even approximately right. I suggest that this view is too drastic and that we can almost surely understand quite a bit about the past on the basis of rather few and somewhat
simple ideas about ancient minds—although never assuming consensus and always taking account of mental differences related to social situations such as gender, status, occupation, and many other things. An almost cartoonish simplicity about local rules and nonrational propensities, while less than we could wish for, may be enough. What I have in mind is suggested by the "faces" invented by the statistician Herman Chernoff (1973). They are intended to convey values of many variables simultaneously. They make no pretense of subtlety, but they get us far beyond faceless blobs or rational calculators (Figure 1).

Second, the direct historical approach has to be taken far more seriously. This means more than simply paying attention to ethnographic and historical data that are possibly relevant. We also need to become more sophisticated in their use. One disturbing fallacy has been a tendency to think that ethnohistoric documents are to be understood as fragmentary and corrupted remains of stories that once existed in the same conceptual framework as recent European narrative history, or at least chronicle. On this view, the ethnohistorian’s task is like a threshing machine, winnowing out "fact" from legend, myth, fiction, and simple error—attempting to piece together the real story. But I doubt very much whether, in most cases, somewhere “behind” existing texts, there was ever a text in the form of a modern European history. Thus, efforts to sift and discard, cut and paste, to piece together a “real” history from the materials available, suffer from far more than a shortage of materials; they suffer from a drastic misconception of the nature of these materials. The “winnowing fact from fiction” approach is almost guaranteed to produce tedious nonsense and to miss the true significances of the texts. More critical,
"postmodernist" approaches are by no means proof against error either, but at least they open our minds to more promising possibilities. In general, ethnohistoric materials are probably more valuable for what they tell us about concepts and symbols (which may or may not have survived without major changes from ancient times) than for what they purport to say about events.2

Third, we need to do far more to develop generalizations of reasonably wide (if not universal) validity that will connect present-day archeological remains with ancient mental phenomena. Trigger (1991) also suggests this, but the approach needs elaboration. I propose to call such a body of generalizations "middle-range theory of the mind," or, for short, MRT of the mind. Since this term echoes Renfrew's (1982) "archeology of mind," I should point out that although I think his article points in this direction, I wish to carry the idea considerably further.

All this relates to what I said earlier about the need to understand ourselves better, and it points squarely toward the centrality of topics in psychology that we have mostly ignored. By "MRT of the mind" I do not mean uncritical and uncontrolled intuitionism, although I do think that intuitions that we cannot easily put in words are among the techniques at our disposal. The kinds of knowledge by which we can better understand our own unnoticed local rules and our own nonrational propensities are the same as those by means of which we can better read the signatures of ancient ideation.

**MRT of the Mind**

In the light of the discussion so far, two points about MRT of the mind can be made at once. First, like all MRT, its assumptions should be relatively secure and independent of the issues in regard to which inferred ancient ideation is to be used as evidence. Second, MRT of the mind can pertain variously to ancient local rules, universal nonrational propensities, or ancient local nonrational propensities.

We should be more adventurous in looking harder for wide-ranging generalizations connecting ideation and material things. Hodder (1991), in stressing the historically contingent character of symbols, is, in effect, casting doubt on the possibility of a useful MRT of the mind. He may be right, but I am not convinced.

With regard to local rules, archeologists have recognized, at least obscurely, that if one finds, in the assemblage of materials from an ancient community, that not every feasible kind of object was actually made, it almost certainly tells us about something more than our system of classification of the objects; it strongly suggests that these discovered categories tell us something about the ancient community's categories. For example, if we find that nearly all projectile points are longer than ten centimeters or shorter than five centimeters, and very few are in between, they would have noticed that a 7.5-cm point did not fit comfortably into their system. The late Albert Spaulding (1953, 1982) was particularly explicit and elaborate about this. I emphasize it because it is the best example we have, so far, of a general means for getting at something about ancient systems of thought and being able to present the evidence in a way that is amenable to verification and testing, with subjective elements relatively limited, explicit, and well-controlled. This, in turn, opens the way to other questions. For example, we can explore aspects of standardization that bear on issues of social relations of production and exchange. It is also possible that strictness or laxity of adherence to norms can give important and reliable insights into other aspects of personality.

Concerning universal nonrational propensities, we can get quite a bit of mileage out of notions such as that larger and/or higher usually (though possibly not invariably) means superordinate and/or more important. Some facial expressions and other aspects of body language are more than human universals; they are often easily read, cross-species expressions of emotions and states of mind (Ekman 1985). The implication is that, if representations of facial expressions survive at all, we are not likely to misread the intended message. Whether that message was true is of course another matter; rulers
may be misrepresented as braver, stronger, or wiser than they really were, and so on. The point is that, even if our questions are about ideology, mystification, or misrepresentation, we can read facial expressions as secure evidence.

These examples are only a beginning. We should make a much more concerted and hard-nosed search for widespread, if not universal, aspects of symbolization. Trigger (1991) notes some suggestive possibilities in animals as symbols, for example. Semiotic approaches derived from Saussure, Peirce, and others emphasize arbitrary aspects of signs and symbols but also provide frames for considering nonarbitrary aspects. Studies such as those of some Jungians (e.g., Jung et al. 1964) and Joseph Campbell (1988) suffer from a tendency to make unwarranted assumptions and to look at items out of context, but nevertheless may provide leads.

Another important distinction is between intended and unintended messages. The importance of understanding what ancient remains were intended to mean is self-evident. I do not have space to deal with art-historical approaches, but I note that art historians have been doing this for a long time, often splendidly. Much of their work is central, not peripheral, to archeological interests.

In addition, however, there is reason to think that people may unwittingly, even unwillingly, reveal states of mind or aspects of personality through content and style of their material productions. I know of only a few efforts to use psychological approaches to interpret ancient images and styles.

In the late 1950s and early 1960s, the ideas of psychologist David McClelland stimulated a mild wavelet of archeological interest. McClelland (1958, 1961) argued that varying amounts of a personality trait he labeled "need for achievement" (abbreviated n Ach) could be related to economic and cultural growth and decline of societies. He exaggerated its importance, and his efforts to test it were naive and betrayed many ethnocentric assumptions and a deep insensitivity to effects of imperialism on underdevelopment. Nevertheless, it is an example of the kind of nonrational propensity that seems worth knowing about. Most relevant for MRT of the mind, Aronson (1958) developed a scheme for scoring graphic expressions that purported to be a good measure of n Ach. This was actually applied to ancient ceramics from Greece and the north coast of Peru (McClelland, Lathrap, and Swartz 1961) and from Teotihuacan (Levy 1966). The results are ambiguous and problematic, partly because of poor data, and partly because two issues were conflated. One was the reliability and validity of the scoring system as an indicator of anything about the personalities of the producers of the objects. The other was the relations (if any) between characteristic personalities of individuals in a society and the trajectories of those societies. Nevertheless, and I choose my words carefully, it seems to me that the efforts were not wholly unpromising. It is distinctly possible that the main reason the approach has not been carried further by archeologists is that the intellectual climate of the 1960s and 1970s in archeology did not encourage interests along these lines.

A second intriguing effort was J. L. Fischer's (1961) study of art styles as cultural cognitive maps. He argued that certain properties of style, such as simple versus complex designs, empty versus crowded space, symmetrical versus asymmetrical designs, presence or absence of enclosures around figures, and preferences for straight or curved designs, should be related to social features, such as degree of social stratification and gender relationships. His arguments connecting design properties with social features seem more ingenious than compelling, and some of his assumptions about gender look quite curious in the 1990s. Nevertheless, for a cross-cultural sample of societies independently rated on a global index of social complexity and on forms of marriage and postmarital residence, Fischer found more fairly strong relationships with art-style properties than would be very likely by chance. Choosing my words carefully again, I can say that his study looks no worse to me than some "classic" processual studies of the 1960s. The only follow-up I know of was Dressler and Robbins's (1975) study of Greek vase painting. They found evidence that Athenian vase decoration was more complex
and had more enclosed figures and less unused space during a period when the society was more stratified than it was before or after. One case is far from overwhelming evidence, but it is very important that all three of these properties were in line with the predictions made some years earlier by Fischer, rather than properties developed by Dressler and Robbins. It is interesting to ask why processual studies at least as problematic as Fischer’s were so influential, while his study got so little attention. His effort had many limitations, but it offers hints that may be worth pursuing. He concluded by saying

one of the most exciting possibilities that the study of art styles and social conditions opens up is the application to extinct cultures known only through archeology. If we can learn enough of the pan-human implications of art styles for social structure and the resulting psychological processes, we should eventually be able to add a major new dimension to our reconstruction of the life of extinct peoples known only from their material remains. [Fischer 1961:90]

In working toward a reasonably secure and useful MRT of the mind, a collaboration with the right kinds of psychologists seems called for, although the last thing we need is the too-narrow and ignorant appropriation of the work of some particular school regarded as problematic by most other psychologists. There is another worry, also. The physical sciences are at a stage of development where they are “ripe” for such applications as radiometric dating, genetic characterization of ancient DNA, and the like. It is not at all clear to me that psychology is at a stage where it is “ripe” for a worthwhile MRT of the mind, and perhaps it never will be. However, we won’t know unless we give it a serious try. In the course of the effort, we may well find insufficient interest in our questions on the part of psychologists, and be driven to develop our own investigations, various kinds of “psychoarcheology.” Ethnoarcheology has profited greatly from the fact that archeologists usually have general anthropological training. Because we are usually quite ignorant of psychology, which is in different academic departments, the institutional barriers to psychoarcheology are far greater. Nevertheless, it is worth trying to overcome them. If we cannot develop a good MRT of the mind, one way or another, then the obstacles that confront prehistoric archeology are formidable indeed.

Teotihuacan

My discussion so far has been overwhelmingly abstract and programmatic. Also, although I insist that any adequate social theory cannot avoid paying attention to social actors conceived as having both rational and nonrational propensities and acting in the context of local rules, I have said almost nothing about what more might be involved in good theory or good explanations. Most of that has to be left for other occasions. However, I will give a very brief example, selected from my work with materials from the huge prehistoric metropolis of Teotihuacan, in central Mexico, that shows some consequences of thinking of actors as important and more than just rational. These results owe a tremendous amount to insights by others, especially to archeologist René Millon and art historian Esther Pasztor. I am indebted also to many others, notably Clara Millon, Janet Catherine Berlo, John Carlson, James Langley, Saburo Sugiyama, and Karl Taube. By now it has become such a collective effort in my mind that it is excessively difficult to trace back who first suggested which aspect of which idea when, and all I can easily say is that I can take only a small part of the credit for the ideas I present here. At the same time, the scholars I have listed hold a diversity of views, and none should be held responsible for my version. I will omit documentation, which can be found in Cowgill (1983, 1992a, 1992b, 1994), Millon (1988), and several chapters in Berrin (1988) and Berlo (1992).

By about A.D. 200, Teotihuacan had become a city of around 100,000 people, by far the largest settlement of its time in Mesoamerica. Some of the largest pre-Columbian structures in the New World had been built, including the Sun Pyramid, the Moon Pyramid, and the monumental 40-acre enclosure known as the Ciudadela. These and scores of other public and elite buildings were laid out along a spacious avenue several
miles long. The overall complex is overwhelming and has no close parallel anywhere in
the New World. Teotihuacan endured for another four or five centuries. It seems to have
been secure and prosperous for at least most of that interval, although no new projects
of monumental building on such a scale were undertaken. The area under direct
political control may have been not much larger than Vermont and New Hampshire
combined, with a population of a half million or so. Trading relations extended
throughout Mesoamerica, but the scale and significance of Teotihuacan commerce is
highly contested. However, symbols related to Teotihuacan, often connected with
warfare, are widespread in Mesoamerica, and it seems likely that the prestige and/or
perceived sacred efficacy of Teotihuacan were so great that identification with the city
could be an important symbolic resource in local political strategies (e.g., Stone 1989).

Everything that I have said about Teotihuacan—its exceptional size, the scale and
overwhelming character of its public buildings, and the distances over which its influ-
ences spread—suggests that Teotihuacan had an exceptionally strong, centralized, and
effective leadership, and an ideology that supported that leadership in ways that were
not too subtle. If so, one would expect the rulers to have sponsored some sort of
monuments to themselves that were readable easily enough and on a large enough scale
that recognizable remains would have survived. This was certainly the case with other
Mesoamerican societies before, during, and after Teotihuacan, including the Olmec,
Zapotec, Classic Maya, and Aztec. Not all of them produced known monuments
representing individual rulers, but many of them did, and others, such as the Zapotec
and the people of Cacaxtla, memorialized defeats inflicted on others. There are vivid
scenes of battle and capture and execution of prisoners. At least among the Classic Maya,
scenes of overt hierarchy are also well known. Recent spectacular advances in decip-
ment of Maya writing enrich our understanding of these scenes, but the basic points are
clear enough without the writing; differences in height, dress, gesture, and expression
suffice (Figure 2).

At Teotihuacan, the situation is strikingly different. Many representations of humans,
deities, and entities with animal or plant attributes and religious significance survive in
murals and painted and modeled ceramics; more than is generally realized survive in
carved stone. However, although persons are sometimes shown subordinated to a deity,
they are never in hierarchical relations to one another; hierarchy is only implied by the
fact that costumes are often much too elaborate to have been worn by ordinary people
on ordinary occasions. Warfare and sacrifice are not illustrated but are implied by armed
persons and by celebrants bearing hearts impaled on obsidian knives. Named individu-
als are unknown except perhaps in one late mural group, where otherwise undifferen-
tiated figures are each associated with different glyphs. These glyphs may be personal
names, but they are still undeciphered and may instead be titles of offices, names of
places or lineages, or something else.

What Teotihuacan art, both public and domestic, does not represent is made plain by
comparison with other Mesoamerican styles. Properties it does have include imperson-
ality, formality, multiplicity, and replication. With few exceptions, humans are so loaded
with regalia that nothing of their bodies is visible except faces, hands, and lower
extremities. Faces rarely show any emotion except a distanced concentration that I read
as solemn and devoted intensity, worshipful high seriousness. Almost by default, atten-
tion is drawn to costume and the wealth of signs and symbols it carries. Emphasis is on
the office rather than the officeholder. A common type of composition shows a
procession of celebrants, all dressed just alike and all in identical poses, maintaining
their distance from each other but otherwise not interacting or taking any visible notice
of one another (Figure 3).

To those for whom aesthetic appreciation is not a sufficient end in itself, what is the
good of noticing all this? Several answers are possible. One is that much of the art to
which I refer would have been sufficiently costly that it must at least have been consonant
with, and probably actively promoted, the interests of the most powerful groups in the
Figure 2
A Teotihuacan processional figure in a mural from the Techinantita apartment compound. The object to the lower right is a sign of unknown meaning that may or may not be the name of the individual depicted. From Berrin (1988:114). Original drawing by Saburo Sugiyama, reprinted with the permission of the Fine Arts Museums of San Francisco.

It is conceivable that the art fairly accurately represents the society, but it is far more likely that it systematically misrepresents it. Misrepresentation of some kind is, no doubt, a universal aspect of ideology, hegemony (in the Gramscian sense), and asymmetrical social relations. But why this form of misrepresentation, so different from that of other Mesoamerican societies, as well as many societies in other parts of the world? It is most unlikely that Teotihuacan was sociopolitically very similar to other Mesoamerican societies and just accidentally happened to use a different style and different themes in its public art. What, then, was different about "social reality" at Teotihuacan?

One possibility that is being seriously considered is that in Teotihuacan's most dynamic period of urban growth, before about A.D. 200, there were very strong rulers who exercised great power and authority and who expressed this in lasting form by mobilizing the resources to erect colossal monuments—the Moon and Sun pyramids and the Ciudadela—and perhaps also by great royal tombs. This possibility is enhanced by recent discovery at the Temple of Quetzalcoatl (within the Ciudadela) of massive human sacrifice and large looted pits, which may—or may not—have contained remains of rulers (Cabrera, Sugiyama, and Cowgill 1991). The existence of earlier royal tombs within the Moon and Sun pyramids remains untested. After the Temple of Quetzalcoatl, in the centuries for which there is no evidence for memorialization in either monumental architecture or any representational media, nor anything such as grandiose palaces to suggest extremely great accumulations of private wealth, there may have been a change in local rules, to some collective form of rulership, with the powers of individual rulers sharply circumscribed. We are not accustomed to thinking along these lines—after all, ancient empires imply ancient emperors (and the occasional empress)—but it
is a perfectly reasonable possibility. In suggesting it, I am not presenting it as an answer, but as an example of the kinds of questions bearing on overall social trajectories that are posed (and will in part be answered) by attention to ancient ideation.

Other insights flow from considering ideational propensities (as distinct from local rules). The formality, impersonality, multiplicity, and replication seen in Teotihuacan art may express character traits and world outlooks prevalent in Teotihuacan society. Indeed, some of the representations may have helped to inculcate such propensities. The properties in question are visible not only on stone monuments, costly murals, and fine ceramics, but in forms, such as figurines and censers, that are ubiquitous in the city and associated with households or with small groups of households co-residing in apartment compounds. This suggests that the propensities were shared to a considerable extent across differences of class, faction, and gender. I do not suggest that all Teotihuacanos in fact acted, thought, and felt like the images in their art, but it does seem very possible that much social learning pushed them in that direction, more so than in other Mesoamerican societies.

Hassig (1992) has recently argued that, unlike among the Maya, war at Teotihuacan was not a preserve of the elite, and that men of ordinary status were an important part of Teotihuacan armies. He bases this on several lines of evidence, including the kinds of arms and equipment emphasized and calculations of probable sizes of Teotihuacan armies. He characterizes Teotihuacan society as “meritocratic,” in which commoners are motivated to fight by the perceived prospect of upward social mobility through successful performance in war. For him, Teotihuacan art is explainable by the meritocratic nature of the society, and it becomes part of the evidence supporting that interpretation.

The prospect of social mobility may have been an important part of the motivation of the Teotihuacan rank and file. However, to think that it must have been is, tacitly, to adopt a strong version of a rational-choice model, in which nothing except perceptions of fairly narrow self-interest could have provided sufficient motivation. To be skeptical of this is not to rush to the opposite extreme and suggest that Teotihuacanos went to war in frenzies of ideological fervor, convinced that the world would fall apart if they did not do their bit. What I do suggest is that it is not obvious that ordinary Teotihuacanos were encouraged to think that they had much chance of rising in the system. They may well have been sufficiently motivated to do their part in making Teotihuacan armies highly effective by a conviction of duty and a shared belief that there was virtue in being a good and well-disciplined Teotihuacan soldier.

Summary

The main defects of processual thought and practice seem clear, and, for most of them, we have some idea of how to remedy them and proceed. However, the need to take serious account of ancient mental phenomena threatens us with an impasse. I suggest three possible lines of progress: (1) see how far we can get with rather limited knowledge of ancient ideation, (2) become far more sophisticated about direct historical approaches where they are applicable at all, and (3) work hard and imaginatively (but responsibly) to develop a worthwhile MRT of the mind.

Postscript

Gender is among important issues that I do not explicitly deal with in this article. However, I am well aware that some of the views I criticize, especially neoclassical concepts of highly autonomous and highly rational actors, have been characterized as androcentric by several writers on gender and science, as has the relative neglect of emotions, sentiments, and collaborative enterprises. I have learned much from this literature (e.g., Gero and Conkey 1991; Wylie 1992a), and it has affected my thoughts
on these and other topics. However, what I say here is a development from views that have seemed congenial and "natural" as long as I can remember. Surely they can readily be integrated into the thought of archeologists of all genders.

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Notes

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1. Feinman and Nicholas (1992:155–156) offer a case in point. In interpreting their data, several archeologists have used a version of Ester Boserup's notion that exogenously caused population growth explains key episodes of socioeconomic change. More recently, Boserup has claimed these same results as confirmatory support for her original thesis.

2. Gillespie (1989) develops a very similar viewpoint in more detail.

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