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Author(s): Mark Nathan Cohen
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Introduction: Rethinking the Origins of Agriculture

by Mark Nathan Cohen

The papers in this special section are revised contributions to one of the Conversations in the Disciplines funded by the State University of New York and the SUNY College at Plattsburgh, held in Plattsburgh on October 7–9, 2007. This discussion, titled "The Origins of Agriculture," brought together scholars with diverse perspectives and methodological approaches on the origins of agriculture. My perception is that the many perspectives and methodologies are not in sufficient contact with one another, and my goal in organizing this collection for the SUNY Conversation and for this special section in *Current Anthropology* was to generate cross-fertilization of ideas. The areas in question include ancient health, paleopathology, paleonutrition, paleodemography, evolutionary theory, genetics, political prehistory, social organization, climatology, human behavioral ecology, archaeobotany, and Neolithic demographic transition theory. As these papers demonstrate, the conversation was successful to a degree in that regard, even though, as is inevitable, we reached no consensus. My perspective was then and is now that such widespread common events require relatively simple common core events and causes. To argue otherwise defies the odds of coincidence given the enormously widespread complex sequences of events occurring in parallel but independently in so many regions of the world. I suggested that we resembled the proverbial blind men, each describing an elephant from the perspective of his or her own incomplete experience. (Hence the reference to elephants in some accompanying papers.) I still insist that there is an "elephant," or common core of events, despite our various incomplete perspectives; however, many participants do not agree that the elephant or core understanding exists.

What follows is my own perspective on the problem, modified and updated by more recent research and, most recently, by participation in the conversation. I take pride of place as organizer and also as the person providing the most primitive, albeit drastically updated, hypothesis.

In *The Food Crisis in Prehistory* (Cohen 1977), I proposed a theory of the origins of agriculture that attempted to explain the parallel and roughly synchronous adoption of agricultural economies in many parts of the world. I insisted—and, as noted, I remain convinced—that such a widespread pattern must be connected by some parallel event or concatenation of events. Any putative cause must match the distribution of the results to be explained. The multiple regional explanations that had been offered (see particularly Reed 1977) missed the point that the parallelism demanded a supraregional explanation. This never denied that beyond this common pattern a great deal of regional variation must exist. In fact, I noted (Cohen 1977) that the broad model was not capable of predicting where the events of domestication and farming would first appear, and why. A further very important argument from various ones of us at that time, now I think very widely accepted, was that we are dealing with demand- or need- or "push"-based changes rather than those based on "pull" or supply-side arguments (i.e., new knowledge invention and new technology). One corollary was that the origins of agriculture would have occurred in far more locations than was then perceived, including not only multiple centers of incipient domestication but multiple domestications within any "central" region (see Barker 2006).

I argued (Cohen 1977) that climate change could not be the main factor because it was a regional or zonal event. I also thought—and think—that politics-based explanations (e.g., Hayden, in this issue) beg the question of the widespread parallelism implied by the fact that new political forms also occurred in many places within a narrow time span and at a particular level of resource "intensification." My partial criticism of Rindos (1984) is that in focusing on symbiosis and mutual "domestication," he failed to note that it was one species—ours—that entered into a spate of new symbiotic relationships within a narrow time period, implying human agency that needs to be explained.

I proposed that the common factor must be an imbalance between population and resources or what was then called "population pressure." The model did not presume that population growth was the only possible cause of the pressure. Declining resources could also be involved, as could, in retrospect, political pressures. Although clearly overestimating the effects of growing population, I was careful to define

Mark Nathan Cohen is Distinguished Professor of Anthropology, State University of New York College at Plattsburgh (Plattsburgh, New York 12901, U.S.A. [cohenmn@plattsburgh.edu]). This paper was submitted 6 IX 08 and accepted 1 V 09.

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population pressure as "nothing more than an imbalance between a population, its choice of foods, and its work standards which forces the population either to change its eating habits or to work harder, or which if no adjustment is made can lead to the exhaustion of certain resources" (Cohen 1977, 50). By this statement, I also argued that while "carrying capacity" was relevant for particular resources, it had no relevance to human populations as a whole (except in areas severely restricted in resources) given our omnivory, our flexible expansion of food choices and areas and niches utilized, and our capacity to increase investment. I continue to maintain this principle because it is clearly borne out by human history, up to the present. (Despite a temporary crisis of supply as I write, the modern and future world food supply is dependent very much on demand [that, recall, combines desire and need with the ability to command resources, which the poor lack].) When the poor can pay enough, new resources will be found.

Some tenets of the model still apply; others do not. But we have to distinguish babies and bathwater. A convincing case has been made that the flux mechanism, evening out the pressure on resources—always the weakest point of the argument—is not sufficient to have accounted for worldwide parallelism in building pressure (Richerson, Boyd, and Bettinger 2001), although it bears a strong resemblance to more regional models of "free distribution" of populations (Kennett, Andersen, and Winterhalder 2006).

A convincing case has also been made that there are supraregional changes in climate that occurred just prior to the gradual adoption of farming economies that could have set common cross-cultural and cross-regional trends in motion. Richerson, Boyd, and Bettinger (2001) have pointed out that two significant climate changes occurred universally at the end of the Pleistocene: (1) an end to very rapid fluctuations in world temperatures and (2) an increase in atmospheric carbon dioxide. These points are undeniable in the paleoecological record. (These arguments were expanded in the conversation; see Bettinger, Richerson, and Boyd, in this issue.) Both events would have had the very widespread effect of making stable agricultural economies possible. In short, as they noted, agriculture was impossible during the Pleistocene, mandatory in the Holocene. This may well be the major common occurrence in the various sequence. It may, in effect, have "reset the clock." But, as noted below, the reset clock still seems to have been followed by a round of very widespread, parallel responses to increasing pressure on resources in a variety of regions that are not adequately explained by this hypothesis.

Two tenets of *The Food Crisis in Prehistory* (Cohen 1977) proved accurate predictions of data not then available: the data of paleopathology and the data of what was earlier called "optimal foraging theory" (now enclosed in the broader discussion of human behavioral ecology). Early data in each of these areas were reviewed by me (Cohen 1989), and, although the data have since increased enormously, the broad patterns described remain essentially the same.

Declining Health, Increasing Risk

Declining health and increasing "risk" through the archaeological sequences in question are both very widespread and very commonly recognized. This is clear from three separate but convergent sets of evidence (Cohen 1989). Skeletal markers of health and risk, ethnographic studies, and extrapolated patterns of epidemiology all demonstrate that the health of human populations (measured in stature) and the quality of nutrition commonly declined as hunter-gatherers moved toward agriculture, while the frequency of stress episodes, infection, disease, and exposure to parasites all increased with sedentism and increasing community size. It has also become clear that evidence of periodic, serious stresses in children, measured in developmental scars in their teeth, generally increased (Cohen 1989, 2007*b*; Cohen and Armelagos 1984; Cohen and Crane-Kramer 2007; Larsen 1995; Wittwer Backofen and Tomo 2008), although this pattern is not universal. (On present evidence, interesting partial exceptions are found in Southeast Asia, possibly associated with rice rather than maize or wheat as a primary domesticate [see studies in Thailand: Domett and Tayles 2007; Toomay Douglas and Pietrusewsky 2007]). Despite these occasional exceptions, I consider the hypothesis of generally declining health associated with the approach to and adoption of farming economies one of the more robust general arguments about prehistory (Cohen 2007*b*). It is not refuted by arguments about paradoxical interpretations of prehistoric cemeteries (e.g., Wood et al. 2002; cf. Cohen 2007*a*).

Human Behavioral Ecology

The optimal foraging studies (e.g., Kennett and Winterhalder 2006; Winterhalder and Goland 1997; Winterhalder and Smith 1981; B. Winterhalder and D. Kennett, unpublished manuscript, 2008) demonstrate that labor efficiency in terms of caloric (labor) inputs to caloric (food) outputs declined as economies shifted from the more focused hunting of large animals and selected vegetation to a "broader spectrum" of resource use (or, in more modern terms, greater diet breadth), added cultigens to foraging activities, and ultimately shifted to dependence on agriculture (see Russell 1988 regarding the low returns from both wild and early domestic wheat and barley and discussions of the inefficiency of exploiting teosinte, the ancestor of maize, in Kennett and Winterhalder 2006).

Two partially offsetting factors were described in this conversation. The first is the fact that relative labor costs are offset if certain activities can be carried out in periods that are otherwise times of enforced leisure rather than being in direct competition with otherwise more efficient foraging strategies ("opportunity costs"). But while a certain amount of attention has been given to this principle as an abstraction, the issue of opportunity costs does not seem to interfere with general interpretations of diet breadth.

The second argument is that as resources improved after domestication (e.g., as maize varieties were improved), increases in the productivity of crops would move them higher in the ranking of resources, adding to their desirability—a time at which the pull of new technology might be more important in resource choices than the push of increasing demand. But this is a relatively late development following the very low returns of intensive foraging and incipient farming that were clearly push motivated.

Human behavioral ecology has added two important new pieces to the puzzle: future discounting and risk avoidance. The implication of future discounting is that resources that provide immediate returns are more desirable than resources available only in the future, for two reasons. First, the psychology of immediate consumption versus delayed consumption adds weight to the preference for food immediately available. Second, resources to be eaten in the future (e.g., those that are stored) are discounted because they carry with them the very real risk of resource loss as a result of events occurring between food acquisition and food consumption, such as rot, storage failure, and human expropriation.

The implication of the risk reduction is that economic strategies are also designed not only for maximum returns but to minimize the risk of serious food shortages, a problem that apparently became more severe through the various sequences. B. Winterhalder and D. Kennett (unpublished manuscript, 2008) add some additional factors, referring particularly to inertia resulting from the social and organizational costs of altering behaviors and social organization to deal with a new economy. I would add a further risk not yet explored: that such strategies must also have taken into account behaviors that increased the reality, and the perception, of other health costs. That is, labor efficiency must also be considered against the salient background of declining health.

All of these arguments mean that resistance to economic change would have been even greater than the decline of output/input ratios would suggest, further supporting the argument that change was push-based, whatever the source of the push. It is important to note, also, that the issue of declining health itself has to be considered against a backdrop of two potential complications.

First, as Lambert (in this issue) argues, health and well-being may be less important than considerations of fitness. I would argue that as a remote, rather than proximal, goal, fitness would have been far less salient than immediately obvious (proximal) changes in health in terms of decision making. Even if fitness were in theory the more important motivator, changes in fitness would on average have been minuscule—in fact, probably invisible. Health would have been the determining factor.

Second, as Gage and DeWitte (in this issue) as well as others have pointed out, visible morbidity and mortality may be inversely correlated. I dispute that argument. Steckel and Rose (2002) found that high rates of pathology did correspond with lowered life expectancy and, hence, higher mortality. The increase in the frequency of markers of systemic "risk" in children almost certainly means higher, not lower, mortality (Cohen 2007*a*). Moreover, my own reconstruction of health combined with demographic evidence (below) provides convincing evidence that life expectancy declined and mortality increased as health declined and morbidity increased after the adoption of sedentism, cultivation, and agriculture.

The combination of these things creates an interesting paradox. Climate change permits agriculture for the first time, and the smoothing out of climate fluctuations would presumably reduce the risk of crises, yet the data clearly indicate declining conditions of efficiency and health. The adoption of agriculture still seems more a matter of necessity than of invention and choice. Despite apparent climatic amelioration, foraging was becoming more difficult prior to the adoption of cultigens and cultivation-based economies. Apparently, post-Pleistocene economic trends carried with them increasing risk despite the smoothing out of temperature fluctuations and supposed reductions in episodic stress associated with sedentism and food storage.

The Significance of Increasing Risk regarding Sedentism and Storage

I have presented theoretical arguments (now supported by paleopathological data cited above) that salient risk of food shortage and disease should have increased—and did increase—with the adoption of sedentary agricultural economies. (I need not reiterate the theoretical arguments; see Cohen 1989.) A good deal has been said in this conversation (Kuijt, in this issue) about the role of storage facilities in offsetting risk. In fact, the role of storage emerged as one of the most obvious common themes of the conversation. I would argue, however, that storage is, in effect, a "low-ranking" means of avoiding risk compared to mobility, broad dietary variety, effective sharing mechanisms, and the potential use of broad-spectrum foraging as a buffer against shortages of high-ranking resources.

Storage would have been more labor intensive than the other options—less reliable, more dangerous—and it would have resulted in, and did result in, declining nutrition. Storage systems, even in the modern world, are notoriously inefficient. Most storage systems at the time of the Neolithic transition demanded sedentism, itself risky; and stored resources are prone to expropriation and make the populations involved prone to conquest. Also, they attract disease- and parasite-bearing vermin.

The Neolithic Demographic Transition

A very important but unresolved question relates to the changing structure, growth, fertility, and mortality in human populations associated with various stages in the emergence of agricultural economies, and the papers in this conversation come to some contradictory conclusions. It is possible, of course, that no nontrivial demographic change occurred at all. The more common assumption is that population growth rates accelerated a very small amount after the adoption of sedentary low-level agricultural or full agricultural economies. I have shown repeatedly, however, using compound growth rates, comparable to compound interest formulae, that, on average, the increasing rate of growth must have been extremely small, remaining very close to 0, at ca. 0.1%.

This has two consequences in this context. First, as I have noted, it means that any fitness advantage (if in fact that was a directly felt motivation at all) must have been so small as to be imperceptible. If any individuals had increased fitness (Hayden's big men?), others involved in the new economy must have suffered a decrease. Second, it means that any changes in fertility or mortality must have tracked each other with almost perfect precision. Increased fertility had to mean increased mortality: declining fertility, declining mortality. So what happened?

The common assumption has always been that fertility increased (ergo, increasing mortality). It is the position I have taken. Several theoretical arguments have been made in support of this conclusion, including, for example, discussions of breast-feeding and the marginal utility of children. Winterhalder and Leslie (2002) have discussed risk-based fertility, making the argument that, at least in theory, increased risk leads to increased fertility. As described above, the only direct evidence of risk trends associated with the adoption of agriculture indicates that risk went up—ergo, by this argument, fertility went up.

There are also two new kinds of evidence, one indirect and one direct. John Lukacs (quoted in Cohen 2008) has found what he believes is indirect evidence of increasing fertility in the mouths of mothers. Dental caries go up in frequency almost universally with the adoption of agriculture. But, he notes, the increase is significantly greater among women than among men. He also notes that there is a decline in cariostatic enzymes in the mouths of pregnant woman. The differential increase in female versus male caries with agriculture, he suggests, represents the increase in the proportion of her adult life that a woman is pregnant. The more direct evidence is provided in the profile of archaeological cemetery populations, which Bocquet-Appel (2002; Bocquet Appel and Naji 2006) believes represents a "signature" of increasing fertility after the origins of agriculture, followed somewhat later by an increase in mortality.

All of this adds up to the conclusion that post-Pleistocene parallel movements toward agriculture represent an imbalance of resources and demand, or (population) pressure, in the wider sense discussed. In two areas of the world, resources clearly declined: Europe by virtue of increasing warmth, and southwestern South America by virtue of increasing moisture. But can the same be said of all the many regions, with many climate regimes where increasing niche breadth preceded agriculture? We still have the problem of explaining a common increase in pressure that occurred across a range of regional post-Pleistocene climate regimes.

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