Commentary

New Discoveries and Theoretical Implications for the Last Foraging and First Farming in East Asia

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Abstract: A brief summation of the issue’s articles is presented. This leads to a discussion of thematic issues of concepts, methods, and theory that crosscut the articles. These include use of the EnvCalc2.1 program, some issues of terminology, the theoretical approaches of niche construction as opposed to human behavioral ecology (HBE), and the linkage between technology and subsistence change, notably the difference between biface and microblade production.

Keywords: East Asia; origins of agriculture; paleolithic to Neolithic transition

1. Introduction

Let me first make clear that I am an expert on neither Asian archaeology nor the origins of agriculture. I cannot comment on the specifics of particular Asian archaeological sites or on the botanical histories of the various plants involved in the transition from foraging to agriculture in Asia. I do know something about hunter-gatherers, and about how archaeologists have been thinking about the origins of agriculture in North America, where, as in these Asian cases, we have both domesticated indigenous plants (in the eastern US, e.g., Chenopodium) and already-domesticated imports (maize, from southern Mexico/Central America). My comments here, therefore, result from taking a step back from the specifics of the papers to look more at methods, data, and theoretical constructs. However, first I make some observations on the individual contributions.

2. The Case in Asia

The precise pathways taken toward agriculture in any given prehistoric case—the woodlands of the eastern US, Taiwan, Japan, or mainland China—depend on numerous factors of geography, climate, plant genetics and habitat, other subsistence options, human population growth, and others. However, what I see in these papers is a story that has occurred repeatedly in various places in the world over the past 15,000 years. Human population grew to the point where diet had to be supplemented, first, with lower return-rate-resources, which in many cases included plants that produce small seeds (or below-ground storage vessels, such as tubers). These resources responded to their human use and manipulation by becoming more productive in density and/or seed size; eventually, for better or for worse (a steady diet of maize or rice, e.g., is not good for long-term health), they became the focus of diet—and the plants that support the modern world’s 7.8 billion people.

This collection of papers, however, does show that this general, global process has variations produced by local environmental, historical, botanical, and prior cultural conditions. Investigation of this variation helps us understand variety in the specific pathways to agriculture, and consequently which variables are important under what conditions.

There are different theoretical tools at our disposal to understand this variation. Agriculture was imported from Southeast China to Taiwan about 6000 years BP, and, using the combined perspectives of human behavioral ecology (HBE) and niche variation theory (NVT), Yu [1] argues those Neolithic agriculturalists met a population divided into two basic adaptations: one focused on coastal fishing and the other on mountain hunting. The
arrival of agriculturalists almost certainly increased the island’s population density. The result was most likely increased competition, and niche contraction. HBE would predict increased diet breadth under these conditions, and that appears to be what happened, as previous hunter-gatherers incorporated low-ranked/low-cost cultigens into diet. I will add that the ideal free distribution (IFD) model might be a useful next step, as it would predict the order in which different regions should witness the transition to agriculture. Right now, that appears to be western coastal areas, the mountains, and then the east coast.

Fujio [2] also looks at an instance where agriculture (and, later, irrigation technology) was imported, in this case, to Japan from the Korean peninsula before about 3000 years ago. This seems to have been driven by population packing and competition for land on the Korean peninsula. It is often said that hunter-gatherers solve problems by moving; agriculturalists do, too, but not as a matter of course (hunter-gatherers expect to move, agriculturalists do not), and not after exhausting other possibilities. A hunter-gatherer can live in many places; suitable places for agriculture might be few and far between. In Japan, the Jomon population appears to have grown in size, especially after 5500 years ago, when large pithouse villages formed, and then declined, with movement to dispersed groups about 4900 years ago. This process may reflect a “rigidity trap” one based on more focused use of labor-intensive resources such as horse chestnuts [3]. If so, it might signal that the Jomon population ~3000 years ago was in need of a new food source, and rice agriculture from Korea provided it; the same might have happened in the eastern US woodlands, with the appearance of full-time maize horticultural systems ~1100 years ago (maize appears ~2000 years ago but was not consequential for a long time), after which eastern US societies made rapid changes in social and political organization (the “Mississippian culture”). Fujio [2] is able to examine the timing of settlement of different regions of Japan in some detail, and, again, this offers the potential to test whether this process can be described by the IFD. For both Japan and Taiwan, if the IFD fails, then it is probably pointing to some other factor—such as the presence of foragers—that affected agriculturalists’ choice of where to settle.

Likewise, in Japan, Ikeya [4] is concerned with the process whereby Yayoi agriculturalists replaced or subsumed the existing Jomon hunter-gatherers. Across the globe, agriculturalists gradually replaced hunter-gatherers. In fact, this process is not complete, and is still on-going, with very few populations left who live even partially by hunting and gathering (virtually no population today lives entirely by foraging). Again, the IFD model could help explain the spread of agriculture (keeping in mind the “starting” point, where Korean migrants landed may not have been the “best” place to start on the islands of Japan). Nevertheless, Ikeya [4] is more interested in what governs whether the indigenous (Jomon) people of Japan, simply became Yayoi agriculturalists or remained hunter-gatherers to one extent or another. That is a complicated question, and no doubt has to do with the returns from rice agriculture versus the returns from foraging in different parts of Japan. Determining those returns will require further ethnographic and/or experimental work with foraged foods versus cultigens.

Takakura [5] looks at the archaeology of Hokkaido, Japan’s northern island. Here he finds, as others have, that microblade technology disappears during the Younger Dryas (YD) cooling, perhaps, quite possibly, because the human population itself disappears at this time (although the ages of the boat-shaped tool assemblages are not well dated and may fall within the YD). When people returned to the island after the YD (or when their numbers rebounded), they used bifacial technology, without microblades, and ceramics. The Taisho-3 site assemblage shows that the shift away from microblade technology (and accompanied by ceramics) had happened even before the YD, but after the demise of large Pleistocene fauna on the island. At Tasiho-3 there is evidence for the use of aquatic resources, a subsistence shift that in other places (according to the accompanying papers) was a precursor to an agricultural economy.

In North China, Paleolithic foragers shifted to a more Neolithic-like adaptation, including sedentism in large aggregated settlements, large-scale food storage, and so on,
before full agriculture (millet) appears. In this regard, North China is similar to the Near East; and also portions of the eastern US, where the “Hopewell culture” (much later in time) had a predominantly foraging adaptation, with some low-level use of indigenous cultigens. Hopewell people lived in large, sedentary villages that show evidence of social and political “complexity” in the form of elaborate log-tomb burials beneath mounds, and large, sophisticated earthen enclosures, many in geometric forms. Zhao [6] shows that the transition to a fully Neolithic lifeway with millet agriculture (rice in some portions of North China) appears after the 8.2 k climatic cooling event (that increased aridity in northern latitudes). I suspect the adaptive system was moving in the direction of a fully-agricultural lifeway due simply to population pressure and resulting competition. Population pressure, of course, results from an increase in population and/or a decline in resource abundance. I suspect Zhao [6] is right: the 8.2 k event was a severe “trigger” that sped the Neolithization process up by decreasing food availability in North China. Obviously, what is needed are tighter chronological studies to sort assemblages into pre-8.2 k, 8.2 k, and post-8.2 k intervals, e.g., [3].

Liu and her colleagues [7] show that the Paleolithic-to-Neolithic transition was not a simple phenomenon, especially in those zones that are somewhat marginal for agriculture (marginal without considerable labor investment). They see a geographic mosaic of at least three adaptations in the Middle Yangtze region: rice agriculture, hunting and gathering (a continuation of Paleolithic lifeways), and low-level horticulture coupled with hunting, gathering, and fishing, the last indicated by the appearance of a simple, but consistent and apparently efficient flake tool technology incorporating “ridged-hammer bipolar flaking.” It is regions of such adaptive mosaics that, in the future, can provide some of the best tests of hypotheses, given that they allow us to see the effect of different combinations of environment, geography, and population density.

Zhang [8] employs Binford’s [9] macroecological approach to help reconstruct changes in diet, mobility and group size in northern China at the last glacial maximum (LGM), YD, and afterwards. He finds that the evidence matches Binford’s now >40-year-old hypothesis that agriculture should appear in the areas adjacent to those with the greatest density of the wild ancestors of domesticated plants. Binford was thinking specifically of the Near East, and the domestication of wheat and barley. However, the best evidence now suggests that domestication in the Near East began in those areas with high densities of the wild ancestors of domesticated plants, not in the adjacent areas. The North China case, therefore, might be another place to test Binford’s hypothesis rigorously.

Yu [1], Liu and colleagues [7], and Zhang [8] all see an interplay between aquatic resources (riverine in one case, marine in the others) and the incorporation of agriculture into the diet. Cross-cultural studies do suggest that aquatic resources are used in lieu of storable gathered foods, but also in lieu of hunted foods [9,10]. In both cases, an increasing population density would restrict access to food, probably first to large game and then to storable plant foods (but this could depend on specific resource distributions). In any case, Asia offers the potential to examine the trade-offs between the plant, animal, and aquatic components of diet in understanding why some people added agriculture to the mix when they did.

3. EnvCalc2.1 Program

Two papers (Yu [1] and Zhang [8]) make use of the EnvCalc2.1 program [11,12], based on Binford’s [9] exhaustive set of ethnographic and environmental data. With this program one can hypothesize the nature of prehistoric foraging societies by using modern climate data at specific locations to reconstruct past climates, such as the LGM, and then, based on correlations between environmental variables and ethnographic data on foraging societies, predict a number of aspects of prehistoric hunting and gathering societies. It is potentially remarkable, but I have three concerns with the use of this approach.

First, and briefly, the program is not designed to serve in place of reconstructions of the past based on archaeological data. Yu [1] and Zhang [8] are both aware of this, and they
instead offer the approach as a way to frame hypotheses about how we expect prehistoric hunter-gatherers to have behaved, hypotheses that can then be tested against archaeological data. I make this comment only to reiterate and hence avoid any misunderstanding of Binford’s goal: to create a way to use the prehistoric record to improve our understanding of why hunter-gatherers acted the way they did and consequently to understand why prehistory took the particular path it did in a particular region. Yu [1], for example, uses Binford’s approach combined with HBE and NVT to provide some testable ideas of what happened in Taiwan’s prehistory; Zhang [8] uses the approach to suggest when LGM hunter-gatherers might have increased exploitation of storable food resources, such as seeds, again a testable idea.

Second, a shortcoming of Binford’s “macroecological” approach is that no explicit theory stands behind it. Instead, it is empirical generalizations, correlations between environmental and human behavioral variables. Make no mistake: these are enormously useful and, to use one of Binford’s favorite terms, provocative. The idea that stands behind much of Binford’s approach is cultural ecology, or, its updated version, HBE. Although, inexplicably, in my opinion, Binford always rejected HBE, and thus he rejected the most obvious theory to account for the many patterns he demonstrated.

Third, I have concerns about the ethnographic data that stand behind EnvCalc’s predictive capacity. I have only checked one of Binford’s datasets [9], the mobility data presented in his Table 8.04 (I am indebted to Brigid Grund for her dogged assistance in this task). There are 303 cases listed in this table. Of these, 26 (8.5%) are presented as “LRB estimates,” and another 13 (4.3%) are based on personal communications from a relevant ethnographer; the remainder list published references.

I have found no one who knows what “LRB estimate” refers to; they are obviously not based on Binford’s own fieldwork or the field notes of others. In some of those cases it is clear that the group had ceased being nomadic long before any ethnographer could have been on the scene. I have also searched Binford’s archives at Truman State University, but I could find no personal letters from ethnographers in the folders. Although, to be precise, all of Binford’s materials were not yet archived when I made my visit, so they may yet be discovered.

Of the 264 cases with references listed we could verify the exact data in only 3 (1.1%) cases. We found mobility data in 85 (32%) cases but came up with (sometimes only slightly) different estimates; and in some cases, Binford presents data as miles, but they are actually kilometers. However, some of the cases are ones where I am certain there are no mobility data. For example, the Toedökidö (Cattail-Eater) Paiute in western Nevada, is listed as having 5 residential moves/yr and a total annual movement of 90 miles/yr, and the reference is Fowler’s [13] compilation of Willard Park’s 1930s fieldnotes. There are no mobility data in that compilation; indeed, there cannot be, for the Paiute no longer lived a nomadic lifeway in the 1930s. Perhaps Binford made a mistake and listed the wrong reference. That is possible, and I cannot cast the first stone here as there is at least one such mistake in my own mobility data compilation [10] (for the Netsilik). However, I did my dissertation fieldwork in Toedökidö territory, and consequently I am very familiar with the literature, and there are no mobility data in any of the region’s ethnographic or ethnohistoric references. I do not know where Binford’s Toedökidö data came from.

Binford’s data were gathered over many years, and most likely from the efforts of many students. No doubt some errors have crept in as happens with all large compilations of data. Whether they are enough to negate EnvCalc is something that ought to be investigated. I sincerely hope the answer is no.

4. Terminology: Intensification, Complexity, Climate Amelioration/Deterioration

Archaeology uses terms whose meanings are often thought to be clear, but, upon reflection, are not. Morgan [14] makes clear that one such term is intensification. Part of the problem is that “intensification” is not a substantive thing, but a concept. Concepts are good to think with, but not if they mean substantially different things to different
researchers. Intensification can refer to increased productivity, increased efficiency, or increased productivity with increased efficiency. It almost always implies a process or trend over time, as in “increasing intensification.”

Since the concept of intensification is not a definition, it is not always clear what evidence will prove or disprove an argument for intensification. I have long preferred the language of HBE, whose hypotheses entail expectations such as an increase or decrease in diet breadth, which can be measured by relative changes in the range of food resources evidenced in archaeological assemblages; whether it entails a decline in efficiency (it usually does) and/or an increase in productivity (it can, but usually at an increase in time input). In such cases an increase in diet breadth can be defined as “intensification” but what matters is that the hypothesis permits its testing. Agriculture is a special case because it begins with an increase in diet breadth (including, e.g., small, initially low-return-rate seeds in the diet) that implies a decline in efficiency and a consequent increase in workload [10] (pp. 50,51). However, these small seeds respond to human use and ingenuity by an increase in productivity (through selection of larger seeds to generate next season’s crop, or productivity-enhancing behaviors such as irrigation or terracing—all of which require increased labor input). Being part of a package, a plant with an enhanced return rate due to genetics, and to human behavior aimed at exploiting those genetics can leapfrog other resources, especially when imported in their fully domesticated form to a new region (e.g., rice to Japan, or maize to the US eastern woodlands). What is needed, then, is knowledge of how plants of different potential (genetic) productivities coupled with different human behaviors aimed at exploiting those potentials (e.g., simple floodplain farming, irrigation, fertilizing, etc.) affect the return-rates of different horticultural systems. Barlow [15] does this for indigenous maize farming systems in the American southwest.

Other terms have similar difficulties: climate amelioration/deterioration should be made more specific in terms of precipitation and temperature and, when discussing subsistence, their linkages to food availability (or seasonality, etc.) be made clear. Likewise, “complexity” has long bothered me. On the one hand, it implies that the behavior of nomadic hunter-gatherers was simple, which simply is not true (even an hour’s reading on indigenous Australians’ social organization or Dreamtime theology will leave the reader’s mind spinning); even what appears to be “simple” technology and subsistence is predicated on detailed knowledge of natural history and physics. More to the point, “complexity” masks the actual behaviors at work in producing the artifacts that lead archaeologists to label a case of prehistoric hunter-gatherers (or anyone) as “complex” for these are usually artifacts pointing to the importance of formalized social relations between individuals or groups, or to social, political, and/or economic inequality. A concern with terms is not trivial because imprecise terms lead to imprecise explanations.

5. Niche Construction versus Human Behavioral Ecology

Long before maize appeared in the eastern US, inhabitants in the mid-continent cultivated indigenous plants, including squash (Cucurbita pepo), sunflower (Helianthus annuus), marshelder (Iva annua), and chenopod (Chenopodium berlandieri). Other possible cultigens (of lesser importance if they were cultivated) are giant ragweed (Ambrosia trifida), little barley (Hordeum pusillum), maygrass (Phalaris caroliniana), and knotweed (Polygonum erectum). The process of cultivation was underway sometime between 5800 and 3200 cal BP, but was not significant until well after 3200 cal BP. By ~1100 cal BP, maize supplanted these cultivated indigenous plants and long-time horticulturalists/foragers in the eastern US became agriculturalists.

In recent years, some researchers have used niche construction theory (NCT) to explain the origin of agriculture in the eastern US [16]. This approach is not used heavily in these papers (see Yu [1]), but I am certain my Asian colleagues are reading this literature, especially those interested in the origins of agriculture.

Briefly, NCT focuses on how behaviors emerge from the effects that interactions among organisms have on one another. Exploitation of a plant leads to a change in the plant’s
productivity, which leads to a change in human use of the plant, which further affects the plant’s productivity, which further changes how humans use it. Such a process may entail both intentional and unintentional behaviors. Many of the early cultigens of the eastern US grew in the floodplains of rivers, and especially in disturbed habitats. Human occupation of those floodplains, and the removal of trees, e.g., for house construction, or burning to enhance forage for wild game, would have increased the abundance/density of these weeds, possibly increasing their return rate, making them a more attractive food choice. Human foragers might realize that they could enhance this process, and horticulture/domestication would be underway.

An alternative to NCT is HBE. HBE sees subsistence change as driven largely by changes in the availability of high-return-rate resources, changes produced by climate—linked environmental change and/or human population growth that results in a food’s over-exploitation and consequent decline in abundance, a situation described as “resource depression.” Changes in the abundance of low-return-rate resources (such as small seeds), changes that do not increase a resource’s return rate, are not anticipated to have an effect on diet composition.

Although proponents present NCT as an alternative to HBE, it is not. In fact, the predictions of NCT fall quite comfortably within the models used by HBE [17,18], although HBE has admittedly focused on resource depression and not on how human use of resources might have the opposite effect—increasing their return rate by increasing their abundance and/or by technological innovations. (Note, however, that increasing abundance does not automatically increase a food’s return rate. However, under some conditions, an increase in density can increase a resource’s return rate by making the resource more amenable to mass collection methods that raise its return rate by raising the efficiency of collection. A new technology is usually involved: Imagine the return rate from scattered seed-bearing plants, harvested plant by plant as opposed to that of a dense field of the same plant, harvested with a wicker paddle and basket. This process can be enhanced by humans through selective breeding, e.g., for larger seed size.)

The difference between NCT and HBE matters because NCT proponents argue that human population growth was not responsible for the origins of agriculture. Zeanah [17] argues there is good reason to expect that small seeds such as chenopod, even with a density enhanced by human activity, would not be incorporated into the diet until high-return-rate foods were diminished in abundance. He shows that the archaeologically-documented patterns of use in the eastern woodlands—heavy use of hickory, with the later addition of walnut over small seeds, and then diminished use of walnut in favor of the enhanced returns from cultivated small seed plants—fits with an HBE model that predicts cultivation as a response to declines in the abundance of higher-return-rate resources. Miller [19] shows that intensive use of lower-return-rate seeds and cultivation of indigenous plants appears in the eastern US after evidence appears of both population growth and the reduced use of high-return-rate resources.

In other words, the origins of agriculture in the eastern US seems to be completely consistent with a model of changing subsistence driven by population growth, reduction of high-return-rate food resources, coupled with the effects of human activity on the abundance of weedy, small-seed-producing indigenous plants. NCT is an important addition to our understanding of the origins of agriculture, in both primary and secondary settings, but its legitimate concern with the effects of human behavior on the environment and their subsequent effects on subsistence choices falls comfortably within HBE, which provides a sounder foundation in evolutionary theory.

6. Population

Several papers rely implicitly or explicitly on the relationship agriculture had to population growth. It has long been logically assumed that agriculture leads to population growth, what is known as the Neolithic Demographic Transition, and recent studies demonstrate this fact from the data of prehistory [20–22]. At the same time, studies show
that the long-term rate of growth of hunter-gatherer and agriculturalists is the same, \(~0.04\%\) [23], but with a difference: agricultural populations can grow at high rates over short periods of time but are more susceptible to periodic population crashes [23,24]. Future research should give specific attention to measures of population growth/density to test demographic hypotheses about the relationships among agriculture, social and political organization, warfare, and agriculturalists’ foraging neighbors. Asia offers an excellent laboratory for this project.

7. Technology

The stone component of the technology that immediately precedes agriculture in eastern Asia entails microblades. To an archaeologist of the continental US, microblades are quite foreign and I am not qualified to discuss their various methods of production or use. However, let me make two comments that might be useful in understanding why microblade technology disappears. First, Takakura [5] and Zhang [8] associate the decline in microblade technology with the demise of the large Pleistocene fauna of Hokkaido—and that association indeed appears to be true. Although in the US, large fauna, including mammoths and mastodons were hunted with Clovis spearpoints, large bifacial points, a technology that, invented by a population that migrated to North America from Asia about 15,000 years ago, almost certainly came out of a microblade tradition. Microblade technology is certainly not necessary for the hunting of large game. In fact, I have long thought that microblades and bifaces are two different ways to accomplish the same goal of ensuring maintainability, the former relying on modularization-with-replacement and the latter on resharpening.

Second, my sense, and it is only a sense, is that microblade technology is more difficult and time-consuming to learn than bifacial technology. The best candidate in my opinion for the precursor to Clovis technology is the Yubetsu microblade method, which first requires production of a biface, one edge of which is then removed (with one or more “ski spalls”) to create a platform for the removal of standard-sized microblades. That is, the Yubetsu method requires biface technology and creation of a core to produce pressure-flaked, standard-sized microblades. The simple fact that there is a second production stage tells us the microblade method will be more difficult to master. (Clovis retained some of this difficulty with the production of flutes, and, in some places, macroblades from cores, but the fluting technique was dropped, after it was used most prominently in the slightly later-in-time Folsom spearpoints. Macroblade production also disappears.)

What these two facts suggest is that microblade-using populations replaced one technology, microblades, with another, bifaces and flake tools, that (probably) turned out to work just as well for hunting and yet allowed time for children to be enculturated in other tasks that had become necessary, and thus more important than microblade “education,” due to environmentally-induced changes in food resources and/or an increased population, e.g., ceramics, plant gathering, and the hunting of a wider range of animals with all their various behaviors (I will note that the lack of blade standardization at Donghulin may point to less enculturation in proper production methods). Thus, the key to the decline in microblades may lie in a shift in the importance of tasks that children had to learn to be successful adults in a post-Pleistocene world.

8. Conclusions

From an outsider’s perspective, the Near East has dominated our understanding of the origins and development of agricultural and of early agricultural society. Clearly Asia, as an independent center of agricultural origins, and one that contains plants of quite different needs (millet and wet-farming of rice) offers the possibility of testing hypotheses generated by research in the Near East and elsewhere. To do so will require: (1) a better understanding of how climate affects the distribution and abundance of the local, non-domesticated food resources over the past 18,000 years; (2) the collection of data (such as radiocarbon dates, but also measures of site frequency and artifact densities over time) that will permit testing
hypotheses relating agriculture’s inception and development to population growth; (3)
better understanding, generated by ethnographic, ethnohistoric, and experimental work on
the productivity, constraints and possibilities of different agricultural plants and systems;
and (4) better understanding of technologies, their constraints and possibilities. Although
NCT certainly points to the importance of understanding how one generation’s behavior alters
the environment to which the next generation must adapt, I think that HBE is a productive
theoretical approach within which to understand the material possibilities and consequences of
such a process. I, for one, look forward to this research in Asia with enthusiasm.

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