

Comparative Archaeology: The Camel's Nose?

A Review of *The Comparative Archaeology of Complex Societies*, edited by Michael E. Smith (Cambridge University Press, 2012)

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“Everything is known through comparison” (Spirkin 1983:47). Even the most fundamental descriptions employed by archaeologists (for time, space, ‘types’ of ceramic and lithic materials, etc.) dissect some continuum of variability to create categories of difference. Since difference must be defined through comparison, even apparently basic description is implicitly comparative. Archaeology is nothing if not comparison!

So it might seem to the non-archaeologist that the word ‘comparative’ in the title of the work reviewed here is redundant—why not just call it *The Archaeology of Complex Societies*? Alas, not all comparisons are equal, and as Jeremy Sabloff points out in the *Foreword* to this volume, cross-cultural comparison in anthropology (including archaeology) largely fell out of use in the 1980s and 1990s in favor of methods intended to delve into social processes in particular (ancient) societies, hoping to decode the specific meanings attached in that society to phenomena that cross-culturally might appear of superficially similar function. In this way, according to Michael Shanks and Christopher Tilley (1987:95), we could avoid combining the “hammer of function” with “an even more powerful tool, the sledge hammer of cross-cultural comparison.” The end point of this new way of doing anthropology would not be to arrive at a comparison of these processes and meanings and, eventually, an explanation for their differences, but rather an immersion in the perceptive field of an (ancient) other as a project of individual or societal self-awareness. For some this led, or was supposed to lead, to political action in contemporary society; others were content to embrace phenomenology and reject the variants of materialism anchored in a realist philosophy of science that had previously guided most archaeological inquiry.

The Comparative Archaeology of Complex Societies (and the many references herein) provides welcome evidence that archaeology is returning to its comparative roots. While not completely rejecting phenomenological and

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other 'post-processual' approaches, contemporary archaeology, buoyed by the massive and well-controlled datasets accumulated in many parts of the world over the last three decades, is enthusiastically re-embracing comparison of archaeological sequences as one of our fundamental methods for understanding prehistory. And this comes, by the way, not a moment too soon, as a variety of other disciplines recognize the power and interest of these same burgeoning datasets. By now who cannot name at least one biologist, economist, ecologist or Big Historian who has come to be as well known as a comparative interpreter of archaeological data as for research in his home discipline? This convergence of interests—which actually should be welcomed by archaeologists as a sort of coming-of-age for the discipline—is acknowledged in the volume's closing chapter, by Michael Smith.

This book can be read profitably (and should be) for two rather different reasons. First, though it is certainly not a manual, it provides a source for models on *how to do comparative archaeology*. These chapters illustrate a range of options, from intensive comparison of two or a few sequences along a number of dimensions (represented here by chapters comparing strategies of provincials in empires, by Barbara Stark and John Chance, and household economies in the Aztec and Inka empires, by Timothy Earle and Michael Smith), to systematic comparison, often quantitative (but here, not often statistical), of many cases along a few dimensions. These options are laid out and discussed in an opening chapter by Michael Smith and Peter Peregrine—both veteran comparativists—who further divide this dichotomy along a number of additional dimensions (such as degree of contextualization for each case; whether the comparisons are synchronic or diachronic; etc.), locating each of the chapters to come along each of these dimensions. Fittingly (in my opinion) they do not exhibit strong preferences for the 'proper' location of a comparative archaeology along these dimensions. A brief position paper signed by all the contributors to the volume, which serves as Chapter 1, holds that "...there is no single best [comparative] method. A holistic perspective for studying the past requires a range of comparative approaches in concert" (p. 2). The exception is that Smith and Peregrine strongly believe that to the extent possible comparative work should begin from primary data, rather from the high-level interpretations of those data by others.

Second (and this was the pleasant surprise for me as a reader), one might well choose to read this volume for what one can glean about particular phenomena in the archaeological record from a comparative perspective—thus modeling *why we might want to be comparative*. Who knew (I didn't) that the size, degree of isolation, and social circumscription of islands critically affects the nature of their monumental constructions (see the chapter by Michael Kolb). Or that 'sacred' (as opposed to profane) strategies for legitimizing political power are especially common in the presence of descent groups (see Peregrine's contribution; Peregrine's analysis is unique here in involving only

ethnographic data). Or that corporate (as opposed to network), voluntaristic (as opposed to terroristic) and sacred strategies for constructing, implementing, and legitimizing power are apparently the more stable options? (Again, see Peregrine.) These are just examples; many of the contributions herein come to similarly interesting conclusions.

Two paired chapters, by Robert Drennan and Christian Peterson (in both possible orderings) provide especially compelling demonstrations of how and why to do comparative archaeology; accordingly I'm permitting myself to discuss their contributions in some detail. (Drennan has made the comparative archaeology of the social evolution of chiefdoms a long-term focus and currently directs the Center for Comparative Archaeology at the University of Pittsburgh.) Both chapters deal with the processes by which societies within the general range of chiefdoms become larger in demographic scale and generally more hierarchical in sociopolitical organization. For these authors, the former is the "center of gravity" of research, with the "sources of variability in how central power relationships are to human social organization" a matter for empirical discovery (p. 66).

The first of these chapters, "Challenges for Comparative Study of Early Complex Societies," begins with a history of the attempts to characterize and explain the increasing complexity of such societies. Then we arrive at the "challenge," which is appropriate conceptual development to help us avoid using our new wealth of archaeological data simply to provide more examples of archaeological sequences to fit into existing pigeonholes. The method proposed is empirical, beginning with archaeological sequences and using them to work towards abstract generalizations, "carrying out consistent analyses of primary archaeological data sets from many places so as to avoid the impact of incompatible ways of interpreting the evidence that are embedded in the conventional wisdom for different regions" (p. 71). The importance of this strategy is nicely illustrated through a comparison of the most elaborate burials from three sites, two of which have been interpreted as having "rich and powerful chiefs" (Sitio Conte in Panama and Moundville in Alabama), with the other (Pueblo Bonito in New Mexico) often (though less so in the last decade) attributed to some sort of nonhierarchical organization. In fact, the burial accompaniments in the richest graves in Pueblo Bonito are far more numerous and elaborate than those from Moundville, though perhaps less impressive than those of Sitio Conte.

So, skeletal remains and their associated artifacts are clearly one 'data thread' that will be useful here. Others include—

1. local community structure (sizes and degree of nucleation)
2. supra-local community scale (size and estimated population)
3. supra-local community centralization (in size, and especially in the range and importance of economic, political, social, or ritual activities that could explain the centripetal tendencies creating centers)

4. demographic density
5. public works investments (labor requirements for such things as temples and other religious or symbolic spaces, palaces, fortifications, monuments, terraces, canals, etc.)
6. tax rate (investments in public works divided by population size)
7. conflict (internal or external, as assessed from regional settlement patterns, household artifact assemblages, residential architecture, fortifications, mortuary remains, iconographic analysis of monuments, etc.)
8. wealth differentiation (possibly a bundle of related threads, measurable from mortuary remains, household artifacts assemblages, and residential architecture)
9. ritual differentiation (potentially recoverable from mortuary remains, monuments, and household artifact assemblages)
10. prestige differentiation (same data sources as the previous two, distinguished analytically), and
11. productive differentiation (e.g., craft specialization, using generally the same data sources as the three previous threads).

Other relevant threads, mentioned but not discussed, include type and intensity of exchange, land-tenure practices, degree of settlement longevity and potential for inter-generational wealth transfers, and so forth.

The next chapter, *Patterned Variation in Regional Trajectories of Community Growth*, employs the first 7 of these threads in a narrative analysis of 11 trajectories from 6 major regions (highland South America, Mesoamerica, the US Southwest, the US Southeast, west Africa, and north/northeastern China). Their discussion immediately points out one of the general difficulties of comparative approaches, since not all the data threads can be completely assembled for each case, even though these cases were chosen for their relatively complete and comparable data. (For example, regional survey in the Moundville area is not up to the task of allowing population size estimates for the initial Neolithic communities.) This is probably one reason for the decision not to address these data in a statistical analysis. An attractive feature of the presentation is a series of comparable figures representing the spatial distribution of population through time in these sequences, although the number of time slices that can be distinguished in various sequences is quite variable. (As an aside, it would be useful to keep the z-scales on such figures uniform through time and across sequences. Although it is not shown, it appears to vary.) Sequences are 'aligned' by considering the date by which each developed a Neolithic way of life to be the zero point, which varies greatly across this sequence, from about 6500 BC in China to about AD 900 in the US Southeast.

There is a great deal of variability among these sequences in the pace of movement from the initial Neolithic through the end point (either the local

development of, or incorporation into, a state, or the collapse of the trajectory towards increased complexity, as in the two Hohokam cases). The authors suggest that punctuated increases in sizes of social formations are generally connected with increases in regional population size. Obvious extensions to the analyses presented here would be to quantify rates of population growth (using the simple methods developed by Bocquet-Appel 2002) and to work towards quantifying ecosystem characteristics, potential production, and 'reachable' intensifications of production—since presumably all of these greatly affect rate of population growth.

An interesting and unusual feature of the presentation is an emphasis on 'tax rates' which, using the calculation described above, turn out to be, in general, "truly negligible"—on the order of less than one day/worker/year (p. 123). (One wishes that the island societies erecting the monuments discussed by Kolb, which include Rapa Nui, were included in this sample!) For some reason, two of the North American sequences (Pueblo Grande and Moundville) present the highest tax rates. Both of these sequences terminate without leading to larger social formations. Peterson and Drennan in fact suggest that the tax rates calculated for Moundville may not have been enforceable in the long term (p. 126).

It is clear that something like tax rates must cut to the heart of understanding how social formations work and why they increase in size. One could take a functionalist view that larger social groups appear in order to minimize tax rates while maximizing positive returns to scale from group size. But not many would want to do that today, given the successful attacks on such positions from all sides over the last four decades. Or one could take the more Marxian view that aspiring elites attempt to enlarge their communities, allowing for the value of (that is, the size of their skim from) the aggregated tax to be greater, or the tax rate lower (and therefore, their security as leaders higher). Peterson and Drennan tread a fine line between these alternatives: "a combination of the motivations of self-aggrandizing elites, resistance, the social and practical functions of public works, and mobilization of labor makes sense of a pattern of growth that seems recurrent among this dozen trajectories at least" (p. 125).

I suggest that a more elegant way out of this box, that would also allow a more systematic exploration of the trade-offs between tax rates, the public goods they enable, group size, and the advantages and burdens of leadership, is through an evolutionary public goods game framework (see Hooper et al. 2010; Kohler et al. 2012). Game-theoretic frameworks for explanation, and allied evolutionary approaches, are unfortunately not explored in this volume. What they could bring (which is generally missing in this volume) is a framework for analysis in which the payoffs to all members of a society (not just the elites) are systematically taken into account—a critique also made by Gary Feinman in his chapter *Comparative Frames for the Diachronic Analysis*

of *Complex Societies: Next Steps* (p. 35). A slightly different way of addressing similar concerns is illustrated by Blanton and Fargher's recent use of collective action theory (2008).

For a volume in which many chapters deal with social evolution, it is also surprising that the fundamental question as to *why* social groups often increase in size is generally ignored. Peterson and Drennan seem as close as any in this volume to grappling with this problem when they characterize the current understanding as "the idea that larger social formations are the product of self-aggrandizing elites and the social resistance they spark in a process of faction-building and the creation of political economies" (p. 127). Presumably Kolb, given his emphasis on social competition as the driver for the monumental excesses on some of the island societies discussed, would favor inter-group competition as the ultimate driver here (as do I: Kohler 2012).

I don't want to leave the impression though that this volume is all about social evolution. One of the most engaging chapters, by Roland Fletcher, centers on the demise of the low-density urban systems identified in the Maya world (e.g., Tikal), central Cambodia (e.g., Angkor), Sri Lanka (e.g., Anuradhapura) and elsewhere in Southeast Asia. In Fletcher's view these systems all featured extremely homogeneous spatial patterns over vast areas, anchored by a massive infrastructure, especially for water management. The set of climatic conditions under which these systems would function properly was (he claims) very limited, creating vulnerabilities to climatic variability. Extensification of land use surrounding these dispersed urban zones, if production faltered within them, would have further damaged their elaborate water-transport networks through sedimentation.

In his closing chapter Smith ponders the most productive next move for comparative archaeology. He suggests that future analysts should pay more attention to causality, and to examining the power of mechanisms for change proposed by middle-range theory. Feinman suggests less emphasis on understanding how societies move into a category (such as 'the state') and more emphasis on understanding multi-dimensional variability among archaeological sequences in general.

My own suggestion (not out of line with these) is to start from the assumption that most of the variability in the various 'data threads' we see in the archaeological sequences for the sorts of societies discussed in this volume results from different 'strategies' (using this term broadly and not necessarily in connection with agency) for achieving efficiency in inter-group competition. Imagine an $n+1$ -dimensional space, where n is the number of data threads that can be measured, and we do this through time (the additional dimension) for as many sequences as possible. Consider the result to characterize 'behavior spaces' for societies (analogous to McGhee's [2007] 'morphospaces' for species) through time. Most sequences will fall into the interiors of the shapes

defined by the synchronic consideration of these dimensions; societies at vertices (extremes in any one dimension) provide relatively pure examples of what a society specializing in that behavior 'looks like.' Consider the synchronic behavior of each sequence on all its measured dimensions to constitute evolutionary trade-offs, and look for regularities in those trade-offs, both across sequences and through time.

This approach, inspired by current research in evolutionary biology (Noor and Milo 2012; Schuetz et al. 2012; Shoval et al. 2012) should help us deduce what social behaviors (construed broadly, to include structures) are important for fitness, what exactly is being maximized by particular behaviors or structures, and what the trade-offs for achieving that are. Given that the successes and failures of various sequences are known (at least, up to the current time) are there behaviors that are usually successful (or that usually lead to failure)? Or is all dependent on the nature of the competition at the moment? Or in fact are there regular sequences (similar to successional stages for ecosystems) through which interacting groups of societies typically pass?

Of course, if no understandable patterns emerge from this approach, it would strengthen the position of the more extreme post-processualists, that such variation is essentially stylistic and that adaptation plays no significant role in social change. For a discipline that by doctrine prefers proximate to ultimate explanation, conceptualizing our task as I suggest above is a radical departure from business as usual. But once the camel's nose of comparison has cracked open archaeology's somewhat claustrophobic tent, who can tell what fresh air might get in?

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