

THE COMPARATIVE ARCHAEOLOGY OF COMPLEX SOCIETIES

Edited by

Michael E. Smith

Arizona State University

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ARCHAEOLOGY, EARLY COMPLEX SOCIETIES, AND COMPARATIVE SOCIAL SCIENCE HISTORY

Michael E. Smith

The chapters in this volume focus on what may appear to be a narrow domain: comparative studies of early complex societies using archaeological data. But this topic is a crucial part of a broad and far-reaching theme in the human sciences. Many institutions of modern society were largely created by the Urban Revolution; that is, by the transformations of farming villages into agrarian states many millennia ago. Chronologically myopic scholars who think that Medieval Europe constituted the deep, dark, and ancient past of modern society may disagree with this claim, but anthropologists and historians who examine the broad sweep of human history will recognize its value. For when we consider that 99 percent of human history was taken up by small hunting bands and tribal farming villages, the Urban Revolution emerges as the single most momentous social transition on the road to the modern era of states, empires, and global processes (M. E. Smith 2009).

Research on the origins of early complex societies – chiefdoms and states – has long been a staple of fieldwork and comparative analysis within anthropological archaeology (e.g., Adams 1966; Childe 1950; Liu 2009; Wright 1977). Nevertheless, the results of this research have had relatively little impact on thinking in comparative history and the social sciences outside of archaeology and anthropology. Indeed, some economists and political scientists, recognizing the importance of the Urban Revolution for human history, have felt free to construct theoretical models of the process unencumbered by empirical data (e.g., Barzel 2002). Not surprisingly, these models tend to be at odds with the archaeological and historical data on early chiefdoms and states.

By the first decade of the twenty-first century, archaeological data on early chiefdoms and states have become quite abundant, but much of the information remains locked up in technical fieldwork reports, specialized

regional publications, and other corners of the scholarly literature in archaeology. To make sense out of the plethora of new data, many archaeologists are convinced that careful comparative analysis is required. The chapters in this volume showcase some of the more productive comparative methods and approaches for archaeological data. These studies advance our understanding of the origins of and changes within early complex societies, and we hope they will contribute to a broader, transarchaeological understanding of the social, economic, and political processes that shaped human societies before the modern era.

Archaeology and Comparative Social Science History

Scholars outside of archaeology have been slow to acknowledge and incorporate archaeological findings into general theoretical and comparative models about chiefdoms, states, and empires. There are several reasons for this state of affairs, many of them originating in the nature of archaeological data and the discipline of archaeology. Much archaeological research on early states simply has not produced the kinds of data that illuminate processes of social change. The archaeological study of early complex societies began in the eighteenth century with the excavation of temples, tombs, and palaces. Although carrying off luxurious objects for museums has been greatly reduced in recent decades, much archaeology in ancient states today continues the emphasis on monumental and spectacular finds that excite public interest. Such research contributes relatively little to a social understanding of historical processes, yet for many nonarchaeologists this is their dominant view of the discipline.

In recent decades, fieldwork on settlement patterns, households, communities, and economic processes has burgeoned and many archaeologists have adopted a comparative social science perspective on early complex societies (Robin 2001). We now have the data, methods, and concepts to begin to model processes such as the origins of social inequality, trajectories of urbanization, the political strategies of kings, the operation of commercial and noncommercial economies, and the dynamics of ancient imperial expansion. The preceding chapters illustrate some of the best of this new comparative social research, and it is our hope that these and other studies will have an impact on comparative social science research outside of anthropology.

Similarly, we invite archaeologists to consider external theory and case studies to elucidate and contextualize their findings. Productive ties between

archaeological research on complex societies – as exemplified by the chapters in this volume – and comparative social science history should be encouraged in both directions. Archaeological findings can inform on broader issues addressed by social historians and comparative social scientists. At the same time, concepts and methods from that literature have great potential for improving the analysis and understanding of the past within archaeology. Although it seems that few archaeologists currently engage the literature in comparative social science history, much current work in the latter field addresses themes such as long-term change, political economy, and practice that are staples of comparative archaeology (Hoffman 2006; Kiser and Kane 2007; Steckel 2007).

A notable recent example of the archaeological value of work in historical social science is Blanton and Fargher's (2008) use of collective action theory from political science to illuminate preindustrial state dynamics. Although not a specifically archaeological study, the concepts they explore have great potential for archaeologists (e.g., M. E. Smith 2008:chapter 8). Models and concepts from economic history and comparative political economy are increasingly being used to illuminate ancient state dynamics. Economists, for example, have used the tools of their trade to model the operations of ancient economies, from the origins of agriculture in Egypt (Allen 1997) to the Roman Empire (Temin 2006). A new wave of comparative historical and archaeological scholarship on Ancient Rome and Greece is using economic models and concepts to study topics ranging from commerce (Bang 2008) to standard of living (Scheidel 2010) to economic growth (Morris 2004).

I would like to suggest in this context that archaeology could benefit not only from the models and comparative data of the historical social sciences, but also from some of the approaches to causality and explanation that are being developed in fields such as sociology and political science. Within anthropology and much of archaeology, postmodern scholarship has led to an emphasis on high-level social theory, or what might be called "Theory-with-a-capital-T." But theory exists on numerous levels (Ellen 2010), and much archaeological research engages theory at a lower, more empirically based level than abstract social theory. Nevertheless, there are few discussions of theory, causality, or explanation in recent archaeology that acknowledge this lower level of theoretical engagement; for many archaeologists, Theory still tends to be capitalized (Johnson 2010). But in sociology and political science, there is an active and productive engagement with lower levels of social theory and archaeologists can learn from this literature.

Much of this work in sociology and political science can be categorized as what sociologist Robert Merton (1968) termed “middle-range theory.”¹¹ In the words of Peter Hedström and Lars Udéhn, middle-range theory is:

a clear, precise, and simple type of theory which can be used for partially explaining a range of different phenomena, but which makes no pretense of being able to explain all social phenomena, and which is not founded upon any form of extreme reductionism in terms of its explanans [the factors invoked to explain a phenomenon]. It is a vision of sociological theory as a toolbox of semigeneral theories each of which is adequate for explaining a limited range or type of phenomena. (Hedström and Udéhn 2009:31)

An active area of middle-range research in sociology and political science today focuses on the concept of “mechanisms,” which can be defined as “the pathway or process by which an effect is produced or a purpose is accomplished” (Gerring 2007:178). “Mechanisms consist of entities (with their properties) and the activities that these entities engage in, either by themselves or in concert with other entities. These activities bring about change, and the type of change brought about depends on the properties of the entities and how the entities are organized spatially and temporally” (Hedström and Ylikoski 2010:51). In other words, mechanisms are the ways in which actors, processes, and structural constraints interact to bring about particular situations or changes; they are not universal forces or causes but context-specific explanations of social dynamics and change. One of the few anthropologists who has focused explicitly on causal mechanisms is Andrew Vayda (Vayda 2008; Walters and Vayda 2009).

Most of the chapters in this volume discuss the mechanisms that most likely accounted for changes and dynamics in past social systems, although the authors do not use the phrase “mechanism.” Peregrine’s alternative strategies of rulers (Chapter 8), for example, or the processes of market exchange, local political dynamics, imperial conquest, and household production analyzed by Earle and Smith (Chapter 10), or the social competition that generated monumental constructions as discussed by Kolb (Chapter 7) are all examples of causal mechanisms that brought about the changes documented in the archaeological record. One of the few explicit applications of this approach to ancient complex societies is the philosopher of science Benoît Dubreuil’s (2010) wide-ranging analysis of the evolution of hierarchy and inequality in human societies. Discussion of the role of middle-range theory and mechanisms in archaeological explanation would improve the conceptual precision of our models of the past and at the same

time improve communication between archaeologists and other comparative historical social scientists. For further discussion of the potential of Mertonian middle-range theory in archaeology, see M. E. Smith (2011).

Methods and Approaches

The central message of this book is that a comparative approach can greatly advance understanding of social processes in the complex societies of the past. Some scholars might think that what is needed is a manual of methods or a description of best practices. In our discussions at the symposium, however, we decided to avoid such an approach in favor of a series of exemplary case studies. The reasons for this are simple: compared to most data in the historical social sciences, archaeological data are quite refractory, varied, and resistant to standardization. The authors of these chapters are in agreement that there is no single best method for comparative analysis of archaeological data. The varieties of comparisons employed in the preceding chapters are discussed in Chapter 2.

The units of comparison and analysis vary widely among these studies. Monica L. Smith (Chapter 4) focuses on the actions of individuals in a comparison of three very different world regions, whereas Earle and Michael Smith (Chapter 10) focus on households to compare two early empires. Peterson and Drennan (Chapters 5 and 6) compare regional settlement trajectories in a number of world regions, while Kolb (Chapter 7) uses monuments to compare chiefdoms around the world. Fletcher (Chapter 11) compares examples of a particular type of ancient city, whereas Peregrine (Chapter 8) compares a variety of nonwestern polities. Finally, Stark and Chance (Chapter 9) compare several New World empires – Pre-Hispanic and Spanish – to explore the variation in provincial strategies.

The kinds of archaeological data employed in the chapters are equally diverse, ranging from counts of domestic artifacts to sizes of stone monuments and cities, to measures of settlement distribution. Some chapters make considerable use of documentary data (see especially Stark and Chance, Chapter 9), whereas Peregrine (Chapter 8) compares standardized ethnographic data to draw archaeologically relevant conclusions. Some authors (e.g., Feinman, Chapter 3) argue for a larger role for theory in our comparisons, while Drennan and Peterson (Chapter 5) argue that before we can generate useful theory we must understand the empirical archaeological record in more detail and in more locations.

Given this great diversity in data, methods, questions, and concepts, it is reasonable to ask what these chapters have in common. I see two important

commonalities running through the case studies described here: a scientific approach to the past, and an emphasis on the use of primary data. First, there is a commitment to a scientific understanding of the past. Notably, all authors employ some kind of methodological uniformitarianism, the notion that processes and conditions are consistent in operation through space and time (Gould 1986). For anthropology and other social sciences, methodological uniformitarianism produces the assumption that the processes we model operated among numerous human societies throughout the world and throughout history (and prehistory).

Second, the authors in this volume are committed to the analysis of primary archaeological data. As discussed most fully by Drennan and Peterson (Chapter 5), the common practice of comparing the interpretations of diverse archaeologists incorporates too much bias and error. These can be greatly reduced by focusing on the analysis and comparison of primary data – the actual measurements of the archaeological record, rather than the second-level interpretations of diverse scholars. This is not an easy task: Drennan and Peterson spent countless hours determining which archaeological survey data they could use for their comparisons, and Earle and Smith had to make major efforts to get their archaeological data into a basic and standardized format for comparison. But the investment in effort pays off in terms of the empirical results obtained.

Archaeological Comparative Analysis into the Future

By the end of the twentieth century, comparative analysis in archaeology had declined greatly from its midcentury peak. Postmodern scholarship frowned on scientific approaches to explanation and on rigorous comparisons; in the words of geographer Jan Nijman (2007:1), “Comparative methodologies largely disappeared from view” (see also Ward 2009:6). For many, “comparative analysis” consisted of assembling a group of case studies by divergent authors within a single symposium or edited volume, often with little or no systematic evaluation of similarities and differences (for discussion of some of the problems with this procedure, see Kantor and Savitch 2005; M. E. Smith 2006). For others, comparison consisted of the haphazard use of comparative tidbits to illustrate an argument (e.g., Rykwert 1976), a trend that continues today. In discussing James Scott’s book, *The Art of Not Being Governed* (Scott 2009), Frederik Barth comments, “His conclusions seem weakened because of a failure of comparative method . . . I felt overwhelmed by a spate of brief comparisons and one-liners about much of the world . . . If we are to draw useful conclusions,

we need features to be systematized and the connections among them to be illuminated” (Barth 2010:175).

Nevertheless, a number of archaeologists have managed to maintain a systematic approach to comparison. In another paper I single out Bruce Trigger’s massive *Understanding Early Civilizations* (Trigger 2003) and Adam T. Smith’s *The Political Landscape* (A. T. Smith 2003) as contrasting examples of work in this area by a senior and a junior scholar (M. E. Smith 2006). Several of the authors represented in the current book – particularly Robert Drennan, Timothy Earle, Gary Feinman, and Roland Fletcher – have made significant contributions to the comparative analysis of archaeological data over the years.

The essays in the preceding chapters join a growing number of rigorous comparative studies of ancient complex societies by scholars in a number of disciplines, including archaeology (Blanton and Fargher 2008; Feinman and Garraty 2010; Peregrine et al. 2007), classics or ancient history (Bang 2008; Morris and Scheidel 2009), cultural anthropology (Ember and Ember 2001; Hunt 2007), and even biologists-turned-historians (Diamond and Robinson 2010; Turchin 2003, 2008). This body of work is now illuminating some of the most important historical transformations in human society – from the initial rise of social complexity to the changes brought about by imperialism or commercial exchange – using models based on actual archaeological and historical data in place of the speculative accounts of earlier scholars.

Acknowledgments

I thank Cynthia Heath-Smith, Monica Smith, and Barbara Stark for helpful comments on an earlier draft of this chapter.

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Note

- 1 Robert Merton’s concept of middle-range theory should not be confused with the unrelated archaeological concept that was labeled middle-range theory by Lewis Binford to refer to archaeological formation processes. See discussion in Raab and Goodyear (1984).