Just How Useful is Archaeology for Scientists and Scholars in Other Disciplines?

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Abstract:
I review the potential of archaeological data to make contributions to debates and research beyond archaeology and anthropology. Archaeology’s long temporal span and emphasis on material culture provide the foundation for its broader usefulness. Our greatest external successes lie in the realms of human-environmental interactions over time. In the broad realm of comparative and historical social research (e.g., urban studies, economics, political science) archaeology has great potential to make contributions, but so far little progress has been made. I discuss the reasons for this, both internal (factors within archaeological practice) and external (conditions within other social and historical sciences).

What is archaeological knowledge useful for? Beyond the domain of archaeology it is easy to think of a variety of disciplines and approaches for which archaeological data and findings might be interesting, relevant, and useful. But among these fields, there is great variation in the degree to which archaeology had made positive contributions. In this paper I discuss three broad research domains and their degree of “success” in generating useful and positive results from archaeological data. I call these domains human ecodynamics, modeling of complex adaptive systems, and comparative social science. Archaeology has clearly had a positive impact on research in the first two domains, whereas its role in the latter domain has been almost invisible.¹

The present symposium includes papers pertaining to these three domains. In this introductory paper I briefly describe each domain and the nature of archaeological contributions. I explore some of the reasons for the divergence in the receptivity to archaeology between the first two domains (which show greater receptivity) and the third. I devote more attention to the third domain, comparative social science scholarship, partly because this is the area I know best and partly because I feel this domain is in greater need of attention within the field of archaeology. In this discussion I tend to conflate two processes that could be separated for greater analytical precision: (1) processes of collaborative, transdisciplinary research among archaeologists and other scholars; and (2) the degree to which archaeological data and findings are taken seriously and used by scholars in other disciplines. I believe that these two processes are related, and this provides a justification for their joint consideration here.

Because the main audience of this paper consists of archaeologists, I refrain from proving a lengthy discussion of why archaeological data are particularly valuable for research themes in

¹ My own work falls within the third domain, and my knowledge of research and publishing in the first two domains is much more limited with greater chances of omission and error. Comments on those sections are particularly welcome.
other disciplines. The most commonly discussed reason is the broad coverage of archaeology, both temporally and spatially. We are the only discipline with social data from a truly long duration, and our fieldwork covers all humanly-occupied parts of the earth. For further discussion, see Kirch (2005) or Redman (2005). van der Leeuw and Redman (2002) provide additional arguments for the value of archaeology as a central discipline “at the center of socio-natural studies”: our history of environmental research and the extensive experience archaeologist have in transdisciplinary research projects.

**DOMAIN 1: HUMAN ECODYNAMICS**

Kirch (2005:411-414) provides a nice summary of the development of archaeological research in this area. After building a successful research program of environmental archaeology in the mid-twentieth century (Butzer 1982), archaeologists began to interact intensively with ecologists and related scholars in the 1990s. Kirch notes two strands of this work: historical ecology (Crumley 1994; McIntosh et al. 2000), which developed in close collaboration with environmental historians and ecologists, and human ecodynamics, a more explicitly archaeological approach (McGlade 1995; Redman 1999). I use the term “human ecodynamics” here to cover these and other related research that links environmental archaeology to the discipline of ecology and related fields. Within this domain, I discuss four themes of active current research in which archaeologists are interacting with other scholars and making contributions beyond the confines of archaeology: sustainability, resilience, long-term change, and modeling (I single out the latter theme for separate treatment as a second broad domain).

**Sustainability.** Many archaeologists are active participants in the explosion of research in the broad area of sustainability science (Kates and al. 2001). Participant Joseph Tainter has made numerous contributions to sustainability studies, including both empirical analyses of archaeological data (Tainter 2006a) and theoretical works (Allen et al. 2003; Tainter 2006b). Our presenter/discussant, ecological economist Robert Costanza will discuss the IHOPE project (Integrated History of People on Earth), a broad transdisciplinary investigation in long-term human-environmental dynamics. The fact that seven archaeologists are included in *Sustainability or Collapse?* the flagship publication of this project (Costanza et al. 2007) signals the potentially important role of archaeology in current sustainability research.

Nevertheless, one can ask just how seriously archaeology is being taken by sustainability scientists. My reading of *Sustainability or Collapse?* suggests that within the conference and volume, archaeology was valued more as a source of case studies than as a source of concepts. For example, the notion of time scales is recognized in the book as crucial (and serves to organize the chapters), yet none of the archaeological literature on temporal scaling is cited or used. Archaeologists have a substantial literature on time scaling (e.g., Bailey 2007; Holdaway and Wandsnider 2008; Knapp 1992) and one would think that our work in this area would be relevant and useful to a project such as IHOPE (Michelle Hegmon discusses temporal scaling in her paper). This is only one example of a general impression I had after reading the book that archaeology has much more to offer this project than a series of case studies.2

**Resilience.** The concept of resilience has recently come to the fore in both ecology and sustainability research. I single it out here because it provides a narrower domain where linkages between archaeology and ecology may be easier to see. Resilience (“the amount of change a

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2 In another paper I argue that aspects of the approach taken in *Sustainability or Collapse?* may be of limited value for understanding urban sustainability in ancient cities (M.E. Smith n.d.).
system can withstand while retaining certain functions and/or structures” Redman and Kinzig 2003:2) was defined initially by ecologist C.S. Holling (1973). Although Robert McC. Adams (1978) was the first archaeologist to use the concept, it was not taken up intensively by archaeologists until after 2000 (e.g., Redman 2005; Redman and Kinzig 2003; Scarborough 2000). Our presenter Peggy Nelson has been active in applying the resilience concept to archaeological data (Nelson et al. 2006). As in the case of sustainability science, however, it is unclear to what extent ecologists working on resilience see archaeology as a valuable partner. A quick search of the “Researchers Database” on the website of the Resilience Alliance (http://www.resalliance.org) turns up only six out of 240 scholars listing archaeology as a keyword. It is likely that as archaeologists conduct more research designed explicitly to investigate resilience in the past, our contributions to the broader arena will increase.

**Longterm change.** I use this category as a catch-all for archaeologists pursuing research on human ecodynamics that is not strongly wedded to the sustainability or resilience fields. For example, presenter Patrick Kirch (2007) employs the concepts of sustainability and resilience in his research, but takes a broader perspective on long-term change that is more firmly rooted in archaeology. I would also include archaeological research in the human ecology and landscape approaches in this category (e.g., Fisher and Feinman 2005; Hornborg and Crumley 2006).

Quantitative modeling is used in the above approaches in various ways, and a number of researchers in human eco-dynamics employ the technique of modeling (see presentation by Kirch). Nevertheless I single it out as a separate research domain because it forms a distinct intellectual and scientific approach of its own.

**DOMAIN 2: MODELING OF COMPLEX ADAPTIVE SYSTEMS**

The domain of modeling of complex systems is both broader and narrower than the first domain. It is broader in that the complex systems approach transcends archaeology and ecology to include virtually the entirety of the known universe, and it is narrower in that work in this domain is distinguished most strongly by its methods rather than its concepts. The field of complex adaptive systems is strongly associated with the work of the Santa Fe Institute. Although some authors suggest that this field differs greatly from the systems theory of the 1960s (Kohler and van der Leeuw 2007a:5), it sounds to me very much like the systems theory I read as a graduate student in the 1970s (e.g., Buckley 1967; Maruyama 1963; von Bertalanffy 1968), but with better concepts (scale-free networks, chaos theory) and significantly improved methods (agent-based modeling, network analysis); see Bentley and Maschner (2003; 2008).

Although archaeologists adopted some concepts from the systems theory of the 1960s (e.g., Butzer 1980; Flannery 1968; Watson et al. 1971), there is little indication that this work excited much interest among scholars in other disciplines. One possible exception is Tainter’s (1988) *The Collapse of Complex Societies*, a work employing systems concepts that has been influential both within and outside of archaeology. Today, on the other hand, archaeologists have become contributing members of broader communities working with complex adaptive systems. For example, a number of archaeologists have affiliations with the Santa Fe Institute (e.g., Robert McC. Adams, Sander van der Leeuw, Henry Wright). Agent-based modeling is a growing method within archaeology (e.g, Costopoulos 2008; Kohler and van der Leeuw 2007b; Wilkinson et al. 2007), and this work is being done within a wider scholarly community that seems to value the data and findings of archaeologists (Alessa et al. 2006). Archaeologists have been involved in the establishment and promotion of the Open Agent Based Modeling
Consortium (http://www.openabm.org). In our session today, Timothy Kohler and Michael Barton represent this domain.

**DOMAIN 3: COMPARATIVE SOCIAL SCIENCE SCHOLARSHIP**

In comparison with the first two domains, the role of archaeology in comparative social science scholarship has been far less productive. Although most anthropological archaeologists working on complex societies would probably claim to be interested in comparisons and interaction with disciplines like geography, economics, or social history, in fact the level of collaborative work is quite low. Moreover, the impact of archaeology on most social science disciplines is almost negligible. I first explore some of the reasons why this is so, and then I highlight promising work by participants in this session and other archaeologists. I organize my discussion of the first issue in terms the nature of obstacles to the extra-disciplinary acceptance of archaeological research: those external to archaeology, and those internal to our discipline.

**External Obstacles**

Much of the responsibility for the lack of consideration of archaeology in the broader realm of social science research is a lack of interest in the distant past. Most social scientists who work on contemporary society don’t really care what happened thousands of years ago. This is to be expected, since it is hard to argue that a knowledge of, say, growing inequality in Formative Mesoamerica can contribute directly to a better understanding of social classes today. But this lack of interest also extends to scholars who claim to have an interest in past societies and their changes through time.

Geographer Rhys Jones (2004) identified a phenomenon in scholarship in historical geography called “recentism” (the term is from Sluyter 2005). Jones uses citation analysis to document a trend in major geography journals toward increasing concentration on more recent periods in comparison to earlier periods. The accompanying graph (data from Sluyter 2005:290) shows the increase in papers on the 19th and 20th centuries, which was accompanied by a decline in papers on Medieval and pre-Medieval periods. Although I have not done a comparable citation analysis, my impression is that similar publishing trends exist in other journals of historical social science (e.g., Social Science History, Journal of Urban History Journal of Family History, and various journals in economic history). This trend illustrates the point that over the past few decades, scholarship in the historical social sciences has paid less and less attention to the distant past while concentrating almost exclusively on the 19th and 20th centuries. Thus it is not surprising if scholars in these fields pay little attention to archaeology and the distant past.

Another obstacle to the penetration of archaeological knowledge into other disciplines is simple ignorance. I have found that many social scientists express surprise when they learn archaeologists do serious research on topics that occupy sociology, political science, and other
social sciences. Archaeology can study these things? Ethnic segregation in cities? Imperialism? The origins of money? Other scholars really don’t know much about archaeology as a social science. They think we spend all our time hacking through the jungle looking for lost cities or obsessing over arrowhead types. It would never occur to many of these people that we may actually have methods to measure changing standards of living or to analyze ancient communities and social interaction.

I realize that this is a rather broad generalization, and there are certainly exceptions. The fields of economics and economic history can illustrate the situation. A very small number of economic historians take archaeology seriously as a source of data on ancient economies, and apply various economic models and methods to our data (e.g., Steckel 2008; Temin 2003). Others take archaeology seriously and collaborate with archaeologists and ancient historians to analyze aspects of ancient economies (e.g., Hudson and Levine 1996; Hudson and Wunsch 2004). But the standard procedure for economists (and others) interested in understanding ancient societies is to apply models based on modern capitalist societies to an imaginary setting in the distant past, with no consideration that there might be any relevant archaeological data. Thus economist Yoran Barzel (2002) and political scientist Mancur Olson (2000) both believe they can explain the origin of the state without any data on early states; needless to say their models are off base and archaeologists do not find them convincing.3

In a recent example, a group of economists published a paper in the *Proceedings of the National Academy of Sciences* claiming that “Recordkeeping Alters Economic History” (Basu et al. 2009). They performed experiments on college students and found that recordkeeping improves performance in economic games. From this finding they confidently claim that they have now explained the origins of recordkeeping in the past, without pausing to consider two key questions: (1) the methodological role of modern data in inferences about the past; and (2) the existence of archaeological (and historical) data on the origin of accounting and recordkeeping (Hudson and Wunsch 2004; Nissen et al. 1993). Is there a way to combat such ignorance?

**Internal Obstacles**

In another paper (M.E. Smith n.d.) I consider the issue of whether archaeological research on ancient cities can contribute to an understanding of certain modern urban issues (the examples I use are sprawl, squatters, and sustainable cities). I argue that archaeological data can potentially inform a broader understanding of these and other modern urban topics, but archaeologists need to do some housekeeping first. We need to address the concepts used by scholars studying modern and historical cities, and we need to analyze and present our data in terms of those concepts. The presentation by Rebecca Storey illustrates these points for the topic of urban health. At present, non-archaeologists have a hard time accessing our data. They can

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3 A related complaint against these and other scholars is their ignorance of the findings of cultural anthropology. Many economists follow Thomas Hobbes in characterizing life in the state of nature (i.e., before states) as “solitary, poor, nasty, brutish and short” with a constant “war of all against all.” Thus for Douglas North and his colleagues, as for Barzel and Olson, the central problem of pre-state society was violence. Therefore states and elites arose in order to contain that violence, because someone was needed to enforce property rights and stimulate economic growth (North et al. 2009a, b). But if the discipline of anthropology has learned anything from a century of ethnography, it is that societies without the state can generally manage violence, and that people can do quite well, thank you, without having elites around. I do not mean to imply that the works of North and colleagues are not valuable; in fact their book is filled with useful insights and analyses. But it does seem to proceed from a flawed understanding of the way societies were and are organized in contexts away from states or the modern capitalist economy.
read the summaries and syntheses that we write, but these typically do not have the richness and detail of our actual data, which are presented in formats that generally exclude non-expert analysis. The translation of archaeological data into formats so that others can use our data—not just our end-product interpretations—should be an important goal (Drennan and Peterson 2006).

In summary, I suggest that if we want other scholars to use our data and findings to illuminate phenomena beyond the archaeological record, two things are required. First, we need to explore other disciplines to discover concepts that can serve as bridges between those fields and archaeology. We will need to develop material indices and measures for the concepts and phenomena of interest. Second, we need to analyze and present our data in ways that can be compared to the findings of other disciplines. These are not easy tasks, and I am not suggesting that archaeologists need become experts in a large number of new disciplines. But those of us working on cities, for example, should be reading the urban literature beyond archaeology; those working on polities could benefit from the literature in political science; and so on.

Furthermore, transdisciplinary research can help break down the artificial barriers created by current disciplinary structures (Wallerstein 2003). Transdisciplinary work is important because “many, if not all, of the traditional approaches, as well as many heterodox tactics, fail to answer the most pressing issues plaguing the world” (Polimeni 2006:2). Van der Leeuw and Redman (2002) argue that archaeology should take the lead in such transdisciplinary research on social and environmental issues.

Some Promising Directions

The papers in this session that pertain to the third domain illustrate some positive steps forward in exploring the potential contributions of archaeological data to broader realms of comparative social science. The participants join others (e.g., Fletcher 1995; Morris 2004; M.E. Smith n.d.; Tainter 2008) in arguing that archaeology can contribute both empirical data and theoretical insights of value to other social science disciplines. I am including the discipline of history under the social sciences, and Carla Sinopoli’s paper explores some of the relationships between archaeology and history in the study of the Indian past. Although archaeology has long been viewed as a source of facts for broader understandings of history, Sinopoli joins Moreland (2001) and others in arguing that archaeology has more to offer historians than simple facts about individual places or events.

Michelle Hegmon goes even farther than Sinopoli in conceptualizing archaeology’s contributions to knowledge as constituting a realm much broader than simple facts about the past. While at a basic level archaeology is like history in telling us about “one damn thing after another,” archaeologists have developed a considerable body of theoretical knowledge of potential use in other disciplines. Hegmon singles out aspects of temporal and spatial scaling as crucial components of distinctively archaeological knowledge with broader implications. In contrast, Rebecca Storey’s presentation focuses on a single empirical domain—urban health—in which archaeological data have especially great potential for illuminating broader realms of scholarship.

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4 I am currently engaged in a transdisciplinary research project funded by Arizona State University called “Urban Organization Through the Ages: Neighborhoods, Open Spaces, and Urban Life” (see http://latelessons.asu.edu/urban). This multi-year project is in its initial stages, and it is too early to determine what the outcomes will be (York et al. 2009).
SOME COMPARISONS OF THE THREE DOMAINS

I’m sure all of our contributors can easily point out problems and limitations of transdisciplinary work, or list difficulties in the acceptance of that work by practitioners in other fields. Nevertheless, in comparison with the third domain, archaeology has been wildly successful in promoting its mission and data in the areas of human ecodynamics and modeling of complexity. What factors may account for this differential success? I single out four contributing factors: greater convenience in working with natural scientists than social scientists; the use of common concepts and methods that bridge disciplines; the simplification required for these concepts and methods; and the existence of higher levels of institutional support.

1. Greater convenience in working with natural scientists than social scientists. Natural scientists seem more inclined to participate in transdisciplinary research focused on specific problems or questions than do social scientists and historians. Issues of “turf” that inhibit such collaborative research are far more pronounced in the social sciences. The postmodern paradigm—present to some degree in most of the social sciences and humanities—inhibits transdisciplinary research, and its emphasis on “deconstruction” and “problematization” slows the establishment of bodies of empirical findings required for scientific advances.

2. The use of common concepts and methods that bridge disciplines. Concepts from ecology (e.g., ecosystems, resilience) and complexity theory (e.g., dynamic systems, feedback) serve to link archaeological research to work in other disciplines. There is a conscious attempt by archaeologists working in the first two domains to use these concepts in ways compatible with other fields. The adoption of methods such as agent-based modeling and GIS spatial analysis by archaeologists also helps bridge differences between disciplines. This contrasts greatly with the social sciences, where each discipline has its own theoretical framework and cross-disciplinary work is not common. Many concepts employed by a number of disciplines (e.g., social capital, culture, social class, institution, political economy) are defined and used differently in each. There are few fora or organizations in the social sciences equivalent to the Resilience Alliance or the Consortium for Open Agent Based Modeling, where efforts to standardize concepts and compare diverse fields are encouraged.

3. The simplification required for common concepts and methods. Modeling and comparison are methods that require significant levels of abstraction and simplification. This tends to be appreciated to a greater extent in the natural sciences in comparison with the social sciences and humanities. Archaeologists working in the first two domains employ simplification in order to foster interaction with scholars in other fields. One negative consequence of simplification, however, may be the lower attention to research on complex societies (states, empires, chiefdoms) within domains 1 and 2 compared to research on non-hierarchical societies. If all societies are seen as complex, then all societies have equal value for modeling, and hunter-gather settlement is much easier to model than ancient urban systems.

4. Higher levels of institutional support. The level of institutional support for research is much higher in domains 1 and 2 than in the social sciences. To start with, the level of grant funding is much higher. Furthermore, such support includes special programs within the National Science Foundation—e.g., Biocomplexity in the Environment and Coupled Natural and Human Systems—that have supported much of the research by archaeologists in domains 1 and 2. The seminars and publications of the Santa Fe Institute also support this research, and there are a number of journals that actively publish work in this area (e.g., Ambio, Ecology and Society). Although there are journals that publish comparative social science research
(e.g., *Social Science History, Comparative Studies in Society and History, Urban History*), their count of archaeological papers with a broader audience is quite low. And while there are programs that specifically support research in domain 3 (e.g., several programs of the MacArthur Foundation), they are far more limited in their funding, visibility, and broader impact on research.

**SUGGESTIONS FOR ARCHAEOLOGICAL PUBLISHING**

One way to begin the processes of interacting with scholars in other disciplines and of bringing our work to their attention is to publish beyond archaeology. One strategy is to show students and the public the broader value of archaeology (Sabloff 2008). More to the point of this paper is the strategy of publishing in journals and collections that are likely to be seen by scholars in other disciplines. Within the domains of human ecodynamics and modeling, a growing number of archaeological publications are written, at least in part, with an intent to attract interest from scholars outside of archaeology (Kirch 2005; Kohler and van der Leeuw 2007b; Peeples et al. 2006; Redman et al. 2004).

There has been some publishing of this sort in the domain of comparative social science research. Early examples from the field of urban studies include Richard Blanton (1982) and Stephen Kowalewski (1982); more recent studies in diverse domains include Ian Morris (2004) in economics, Monica Smith (2005) in geography, and a paper of mine on urban planning (M.E. Smith 2007). If we think our scholarship is of interest to others, then we need to present it in venues where it will be seen and where it can become part of the wider realm of scholarship on human issues, from health to inequality to urbanism.

Our purpose in this session today is to further the discussion of these issues and promote both transdisciplinary scholarship and the acceptance of our data and findings by other scholars. This is what we mean by “archaeology beyond archaeology.”

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5 In my blog, Publishing Archaeology ([http://publishingarchaeology.blogspot.com](http://publishingarchaeology.blogspot.com)), I devote several posts to the need for archaeologists to publish outside of archaeology. One example from urban studies concerns the book *The Economy of Cities* by Jane Jacobs (1969), an influential urban scholar ([http://publishingarchaeology.blogspot.com/2008/11/why-archaeologists-need-to-publish.html](http://publishingarchaeology.blogspot.com/2008/11/why-archaeologists-need-to-publish.html)). Jacobs spends a chapter arguing that in the distant past, cities developed prior to agriculture. Although any archaeologist knows that this is ridiculously incorrect, I have found that many urban scholars in other disciplines accept Jacobs’s ideas because of her stature within the field and their lack of information about cultural evolution and early cities. Also, the idea of iconoclastic interpretations seems attractive to many scholars. Their defense of Jacobs’s erroneous notions can be quite spirited, if lacking in empirical content (judged by some comments on my blog entry about this). Indeed, after I improved and cleaned up the discussion of the origins of urbanism in the Wikipedia entry “Cities” a few months ago, someone inserted a long passage touting Jacob’s idea that cities preceded agriculture. If archaeologists published outside of archaeology more often, we could perhaps correct this and other erroneous notions about the past that are all too common among scholars of other disciplines.
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