

FROM NARRATIVES TO ALGORITHMS: EXTENDING ARCHAEOLOGICAL EXPLANATION BEYOND ARCHAEOLOGY

C. Michael Barton

School of Human Evolution & Social Change
Center for Social Dynamics and Complexity
Arizona State University

DO NOT CITE IN ANY CONTEXT WITHOUT PERMISSION OF THE AUTHOR

Abstract

The static, fragmentary archaeological record requires us to construct models of the human past. Traditionally, these have been narratives that make compelling stories but are difficult to evaluate. Recent advances in geospatial and agent-based modeling technology offers the potential to create quantitative models of human systems, but also challenge us to conceive of human societies in ways that can be expressed in algorithmic form. Besides making our own explanations more robust, integrating such quantitative modeling into archaeological practice can produce more useful accounts of human systems and their long-term dynamics for other disciplines and policy makers.

Archaeology and Social Dynamics

While archaeology has long enjoyed a special place in western society, that of documenting the human past, archaeologists have begun to argue recently that it also has a potentially broader role. Because only archaeology documents human societies, their long-term dynamics, and their interactions with the world around them globally and beyond the few written records from the very few literate societies—we have made the point that archaeologists are well placed to offer unique insight into the underlying processes of social change and ecological interactions (Redman 2004; Sander van der Leeuw and Redman 2002; Fisher and Feinman 2005). At least some beyond the field have been receptive to this claim, and recent research to provide useful examples of the potential contribution of archaeology to a broader field of human social dynamics is the subject of this session.

It is important to realize, however, the claim that archaeological research and its results has relevance in the world of today's social and environmental issues carries with it an expectation for a level of validity in archaeological explanation that we have not previously faced. If we are simply recounting the past, the '*correctness*' of our interpretations—the stories we tell—is not really all that important beyond the intellectual satisfaction of approximating some kind of truth. We can even debate whether such a 'true' past even existed. In this context, we can afford to be ecumenical and tolerant of alternative accounts of the past. However, if our accounts of social organization and change have the potential to alter social policies, with consequences for the lives and well being of real people, then the correctness of our interpretations matters a great deal. That is, the claim of relevance beyond the discipline of archaeology means that we can be held accountable for our work by a wide constituency outside the field. In this latter context, it is increasingly important that we are able identify—and improve on—better and worse accounts of human society and its changes.

This is both challenging and somewhat frightening. Certainly, some would prefer to remain simply narrators of the past. But while there will continue to be a need for this role, it is not likely that the job market for keepers of a global human heritage will expand in the near future;

in fact many anthropology and humanities programs in universities are seeing budgets shrink alarmingly. On the other hand, the growing recognition that an understanding of human social dynamics—and even social and environmental planning—can benefit from scientific information about the human past, is an important opportunity for archaeology to become a much more valuable contributor to social science. At the same time, the fact that archaeology is the only discipline that can provide such information in a systematic and rigorous way over most of the human past, can serve to imbue our field with a new vitality and offer stronger justification for the continued support of archaeology within academic and research institutions, both public and private.

Archaeological Data and Research Protocols

If we embark upon the road of a social science that is more than just a study of past societies, we must also accept the accompanying greater responsibility for the usefulness of the results of archaeological research to others. This obliges us to re-evaluate the nature of archaeological data and our protocols for extracting knowledge from those data. Much archaeology is conducted in the same way it has been for more than a century—by hand excavation in small rectangular pits. The last half of the 20th Century saw this approach augmented by the introduction of extensive pedestrian survey, statistical sampling techniques, and most recently by geophysical survey and remote sensing. Even with new electronic methods, study of buried archaeological materials takes place over only a miniscule fraction of the preserved archaeological record in the best of cases. Systematic survey and large scale remote sensing cover extensive geographical areas, but only inform on materials visible at the surface (or substantial structures near the surface in the case of the most sensitive of remote sensing techniques). Most of the archaeological record is pragmatically inaccessible. In fact, the overwhelming majority is simply gone and unrecoverable—lost to decomposition, physical disintegration, moved far beyond its original context, or simply eroded into the oceans. We have continued to devise ever more clever ways to extract useful information from this fragmented, inaccessible, and largely missing record of past human

activities. Nevertheless, we will never be able to study more than a very tiny and probably biased fraction of originally available human behavioral residues.

This does not mean that we need to energetically begin a massive new program of intensive fieldwork to collect more data, nor is the state of the archaeological record necessarily cause to be despondent about the future of our field as a social science. Indeed, the very small portion of the archaeological record that we have recovered has filled our museums and repositories to capacity. However, we need to reconsider what we *do* with those broken fragments of material culture, chemical residues, and the like that comprise archaeological data.

Regardless of the technological sophistication with which we collect information from the archaeological record, most archaeologists follow similar protocols for ultimately making sense of these data. We *infer* past behaviors and organizations from multiple lines of mutually corroborating evidence, then we weave these inferences into a narrative that verbally *reconstructs* past human social systems and their dynamics. These narratives can bring the past alive for the general public. However, the data we use for our reconstructions are static, highly altered, secondary or tertiary residues of dynamic processes, and not the processes themselves. Through careful application of middle-range theory and uniformitarian principles, archaeologists do a truly amazing job of reconstructing past systems from these fragmentary and often ambiguous data. However, because we must fill-in enormous gaps in our information with prose, multiple—and commonly conflicting—reconstructions often can be derived from the same set of sparse data.

In other words, in spite of efforts to bring hypothesis testing and replicability to archaeological analysis, in the end our accounts of the past remain largely inductive stories. While this may be sufficient if our only goal is to produce accounts of the past, if we are social scientists (i.e., engaged in the scientific study of human society) who employ data from the human past rather than from interviews or participant observation, then such inductive accounts are much less satisfactory. There is nothing particularly magical about the Kuhnian hypothetico-deductive method, and indeed it probably has been over-ritualized in archaeological method and

theory classes. Nevertheless, the protocol of proposing explicit and falsifiable models to account for phenomena and then testing them against the empirical record has been shown repeatedly to be a pragmatically effective way of differentiating more and less robust explanations.

Inductive narrative reconstructions of the past, on the other hand, are very difficult to evaluate in this way. Because they are created from often lengthy chains of inferences drawn from sparse proxy data, they can easily be adjusted to fit the data. This makes them very difficult to falsify; testing them against the data from which they ultimately are inferred is indeed circular. Furthermore, the narrative character of reconstructions weaves together the complex interactions of many social, behavioral, and ecological variables whose values are rarely specified; often many other variables are implied in our reconstructions and not explicitly specified. These many interactions are often assumed from 'common sense' understandings of society and rarely examined in detail.

In the end, archaeological narratives that reconstruct the past can make for compelling reading and serve as useful cautionary tales, but we lack a way to evaluate robustly the degree to which they accurately represent human social dynamics. In order to choose between competing stories to account for the same bits of pottery and stone, we are left with the authority of the writer or the quality of the prose. These criticisms have been raised before, and archaeologists have long struggled to craft better reconstructions of the past. Given this assessment, how *can* we differentiate between alternative reconstructions? I suggest that we cannot. The archaeological record is simply too fragmentary, altered, and ambiguous to reconstruct the past through chains of inference. In fact, I propose that we should stop attempting to reconstruct the past as a means of studying human society and its dynamics. Narrative reconstruction certainly has an important educational role for describing our past to non-specialists, and can continue to spark our own imagination and enthusiasm for archaeology. However, it is not a foundation on which to base a social *science* of archaeology.

On the other hand, eschewing speculative stories of a reconstructed past should not require us to confine archaeology to a study of the physical objects of the archaeological record—a sort of

quantitative antiquarianism. A field of inquiry that focuses exclusively on the measurement and analysis of preserved material culture is not really a *social* science either; it would be intellectually unsatisfying to most archaeologists and of even less value to a broader constituency than compelling, but speculative, reconstructions.

Models in Archaeology

Fortunately, there is an alternative to simply telling stories about the past or a science of artifacts. We can create explicit models of individual behavior and social change, express these models mathematically or simulate them computationally, and compare the results against the empirical archaeological record (Turchin 2008; Kohler and van der Leeuw 2007). This approach is in fact commonly called for in much archaeological programmatic literature, even though it is not often followed very well in practice. A model is any abstract or simplified representation of real-world phenomena, and can be expressed in a wide variety of forms. Scientific models often are also explanatory in that they account for complex phenomena in terms of simpler, usually more general rules or algorithms. Describing models in narrative form is important for conceptualizing (i.e., most of us think in natural language, not mathematical terms) and conveying what we do to others. However, expressing models in mathematical or algorithmic form for testing purposes allows a researcher and others to much better evaluate the details of a model, its assumptions, and its explanatory robustness. That is, formal expression of models makes for greater transparency and facilitates the kinds of cumulative improvements to understanding that are a hallmark of science. Indeed, archaeological reconstructions *are* models of human behavior and social change, though they are only sometimes explanatory. It is the fact that reconstructions are created inferentially from archaeological data and expressed in ambiguous prose that makes them problematic for a social science of archaeology, not the fact that they are models.

I suggest that the scientific goals of our discipline should no longer be about crafting accounts of the past, but should emphasize making and testing models about society and individual practice. While we increasingly discuss the implications of our research for

understanding society in general, these implications usually are found in the discussion and conclusion sections, after our inferential reconstructions. Rather than testing inferences about the past, we should at the outset be testing models of social dynamics against the archaeological record; the *implications* can be insights about life in the past. Such a shift in focus of archaeological research has a number of benefits for extending the intellectual scope of the field within and beyond anthropology.

If we are creating models of human society and behavior, then it is no longer a problem *per se* that recent and modern people do not act exactly like people in the past. Information from ethnography can parameterize models of social dynamics with realistic values that allow them to better represent processes of long-term social change, not to populate long-dead societies.

Models are much more readily falsifiable if we create them independent of the data used to test them. A model of regional abandonment by subsistence farmers can be tested against data from the American Southwest and Neolithic Greece, but it is problematic to test a reconstruction of the abandonment of Greek villages against the archaeological data used to craft the model. This helps bring out strong points and flaws in models and ultimately makes them more robust. Related to this, we can make a better case for the reliability and correctness of models about social process, since they can also be evaluated against modern data, than we can with reconstructions of past societies that can never be observed.

The case for broader relevance is also much easier to make if we are testing models of social dynamics against the archaeological record. The value of using the archaeological record to test models of human response to rapid and severe climate change can more readily be understood by scientists outside the field than can research seeking to reconstruct settlement change in Epipaleolithic foragers of the Near East or mid-Holocene pastoralists of the southern fringe of the Sahara.

Finally, redirecting archaeological research toward the creation and testing of models of social dynamics not only extends archaeology beyond anthropology, but also benefits archaeology as archaeology. More robust, explicit social and behavioral models, tested against

real-world case studies of the archaeological record can offer better accounts of the past (i.e., accounts that more closely approximate the vanished human societies that we can never observe) and provide a pragmatic, scientific avenue for cumulatively improving those accounts.

This approach is a conceptual shift in archaeological research protocols, independent of the tools and techniques to express and test models. However, advances in computational modeling provide an opportunity to undertake such a new direction more effectively than has been possible in the past. While linear equations and systems dynamics equations have been used to characterize individual and social actions (e.g., see discussion of human behavioral ecology below), recent developments in computational social simulation provide new opportunities to build spatially explicit models that combine individual action and larger scale social process (Kohler and van der Leeuw 2007b; Gilbert and Troitzsch 1999; Bonabeau 2002; Parker et al. 2003; Batty 2005; Mayer et al. 2006). These new simulation approaches better 'map on' to the structure of the archaeological record to make evaluation more straightforward for models of long-term change. They also offer a way to explicitly embed the richness and variety of individual practice into general models of group-level social process and still maintain the transparency and potential for evaluation that comes with expressing models in a formal, quantitative manner. Below, I offer a selection of case studies of model-centric approaches to research on social dynamics to illustrate the potential benefits of reorienting the discipline in this way.

Case Studies

The Spread of Farming

Almost 25 years ago, Albert Ammerman and Luigi Cavalli-Sforza proposed a quantitative, dynamic model of how small-scale farming spreads across regions (Ammerman and Cavalli-Sforza 1984). They suggested that subsistence farming communities expand by demographic growth and the establishment of daughter communities in territory not previously occupied, a process they termed "demic diffusion". They expressed this model in the form of spatial diffusion equations. Starting from the Near East, they simulated the spread of farming

communities across Europe and compared the timing of the arrival of the wave of advance created by demic diffusion at various points across Europe with the Radiocarbon dates for the earliest Neolithic settlements in these locales (Figure 1).

The model was able to account for the spatial and temporal distribution of the earliest Neolithic settlements across Europe with a high degree of correspondence between the predicted and empirical record of these initial farming communities. This model revolutionized the archaeological perspective on the origins of farming in Europe and became a sort of 'null' model for the spread of farming communities globally. Its assumptions and algorithms were explicitly presented, making it possible to evaluate its operation in detail and improve it. Its predictions were equally explicit allowing it to be continuously compared against the European archaeological record as new sites were found and new dates calculated. Currently, it no longer accounts for the European archaeological data as well as it did when first proposed, yet it remains a standard against which other models are assessed (Zilhão 2001; Richards 2003). As is common with first models, aspects of it have been falsified by better data. However, it is not better data but only a better model that can ultimately replace it as framework for understanding space-time regional dispersals among small-scale agropastoral societies.

Human Behavioral Ecology

Human behavioral ecology (HBE) is an outgrowth of decades of animal behavior studies. It comprises a suite of mathematical models of (primarily) individual behavior in various ecological and social contexts, with model development ongoing. HBE models began to appear within anthropology over 25 years ago (Winterhalder and Smith 1981; 2000), and were introduced into archaeology especially by the work of Robert Kelley, Robert Bettinger, and James Boone, among others (Bettinger 1991; Kelly 1991; Kelly 1995; Boone 1992). Combining Darwinian concepts with microeconomics and game theory (Shennan 2002), HBE models have been applied increasingly to research on prehistoric hunter-gatherers; more recently they have been applied to agricultural groups (Kennett and Winterhalder 2006). Perhaps because the models are generally assumed to be valid within their specified constraints, being borrowed from

biology, the archaeological record is not often used to evaluate the applicability of these models to human behavior. Rather the models are often used in a more reconstructionist mode to provide an underlying 'cause' for inferred past behavior. Perhaps for this reason, there is little consensus about the applicability of HBE models for social systems with more complex economic and social organizations than hunter-gatherers. However, HBE models are also beginning to be used as the basis for individual decision rules in more complex computational modeling.

A pragmatic limitation of both HBE models and the diffusion model of Ammerman and Cavalli-Sforza is that neither deals explicitly with variable behavior among multiple, interacting, individual members of human societies. HBE models focus on the actions of generic individuals under specified circumstances. Through game theory, HBE examines frequency-dependent effects—that is, when the actions of one individual are affected by the actions of another—but only at the level of generic dyads. In the way these models are normally expressed, there is no way to apply them simultaneously to the many people who make up a social group. Similarly, HBE models generally do not have spatially explicit consequences; their predictions about behavior are aspatial, even if they refer to spatially variable behaviors like moving to new foraging patches. Diffusion models also have difficulty dealing with individual variation and interactions, but for different reasons. As a simplifying assumption, all individuals or groups in the diffusion model behave exactly alike (i.e., have identical probabilities for the same range of actions) and do not interact with each other. New forms of computational modeling are helping to resolve these issues by assigning rule sets—like those of HBE models—to each agent of a large set of discrete agents. This allows each agent to respond differently to its environmental context, including the presence of other agents. Moreover, if the agents are also mobile, they can approximate the spatially explicit aspects of a diffusion model with maintaining a large degree of individual autonomy.

Simple Rules and Complex Systems

Following on more than a decade of ethnography in Bali, Steven Lansing turned to a new kind of computational modeling, agent-based modeling or ABM, to study the underlying

processes by which small-scale farmers could manage a large and complex irrigation system (Lansing 1991; Lansing and Kremer 1993). Farmers sought to minimize the stress to rice fields due to insufficient water and the effects of insect and rodent pests, by adjusting cropping schedules. Given that the amount of water a farmer receives depends on the cropping schedule of other farmers upstream on the irrigation network and the activity of rice pests is affected by the timing of flooding of adjacent fields, this could be a difficult optimization problem to solve. But Lansing's work shows clearly cropping schedules were highly optimized by Balinese farmers without large-scale, centralized planning and management (Figure 2).

By creating an ABM simulation with farmer agents using simple behavior rules, Lansing found that he could create a the complex optimal cropping schedule needed to maximize production and observed in real-world Balinese rice growers (Figure 3). Farmers simply needed to observe the cropping schedule of their local neighbors and copy the schedule of the most successful neighbor in the subsequent year. The cropping schedule simulated with agents following these rules closely matched the real-world system in Bali. Lansing was able to show in clear, quantitative terms how simple behavioral rules could produce complex social practices. His work also offers important insight into the management of irrigation systems, and showed why the 'bottom-up' actions of local farmers was successful in providing sufficient water for crops and reducing pest infestations over a large area, while a 'top-down' program to increase agricultural productivity in the same region was a failure (Lansing 2006). Because this model is a quantitatively expressed explanation of human action, it can be tested in other context and improved so that it can account for a wider range of social and environmental contexts (Janssen 2007).

Ecology and Regional Abandonment

Shortly after Lansing described his model of Balinese water management, George Gummerman, Jeffrey Dean, Timothy Kohler, and colleagues at the Santa Fe Institute adapted the concepts in Joshua Epstein and Robert Axtell's Sugarscape ABM (J. M Epstein and R. Axtell 1996) to study the abandonment of regional landscapes in the American Southwest (Dean et al.

2000; Axtell et al. 2002). This agent-based 'Artificial Anasazi' model combined information about the environment—especially water availability—with rules for human farming practices and household-level demography (including fertility, mortality, and food consumption). The model was tested against the archaeological record of the Long House Valley, Arizona, where populations grew rapidly after AD 800 to peak around 1250; subsequently, the valley seems to have been entirely abandoned by 1300m (Figure 4). While archaeological interest in this simulation was stimulated by the fact that a some runs were able to closely match the empirical demographic changes in the valley over 500 years, the more interesting general results is that many model runs did *not* match the outcomes of the prehistoric case. In fact, many suggested that sufficient arable land and water remained in the Long House Valley to sustain continued human occupation after 1300, albeit at a lower population level than that of the 1250 peak. This leads to questions of why do people abandon regions in times of stress when they can still make a living? Importantly, this initial modeling project stimulated an ongoing model-centric research project in the northern Southwest, directed by Kohler (Johnson, Kohler, and Cowan 2005; Kohler, Gummerman, and Reynolds 2005). The increasingly sophisticated modeling environment developed by Kohler and his research team is permitting them to ask questions about the complex interactions between social and environmental change. The original Artificial Anasazi simulation recently has been repackaged for demonstrating and teaching ABM <<http://ascape.sourceforge.net/>>

Modeling as Laboratory

Controlled experiments have been very useful in many natural sciences for identifying critical parameters and underlying drivers of change. However, in social sciences in general, and especially for historical cases, it is difficult to impossible to carry out experiments in long-term social dynamics. Ethical considerations and long human life spans preclude all but the simplest experimental designs, focusing on short term individual behaviors or small groups. While such small-scale experiments are important for understanding the fundamental bases for human social behavior (e.g., Tomasello 1999; Janssen and Ostrom 2008; Janssen et al. 2008), they

cannot address the kinds of social processes that characterize even small communities over a few generations, nor are they able to examine ecological consequences of human action over times frames of more than a few years. Because it represents such a wide range of human culture and its expression in such diverse social and ecological contexts globally over millennia, the archaeological record sometimes has been characterized as a natural laboratory for the study of long-term social change. However, the highly fragmentary nature of that record and the necessarily speculative character of reconstructions means that comparisons among case studies or between prehistoric and modern cases provide ambiguous results at best. Furthermore, as a laboratory, it lacks the possibility for control of relevant variables; we must take it as it comes. The archaeological record is better viewed as a diverse and extensive testbed for models that we construct.

An outgrowth of the application of new forms of computational modeling—especially ABM and cellular automata (including dynamic GIS modeling)—is the ability to create experimental laboratories for social and ecological dynamics in which we *can* control relevant variables and have access to *all* the results. Notably, the goal of such modeling is not to simulate the past—i.e., reconstruct the past in a computer instead of in prose. The problematic nature of the archaeological record precludes reliable narrative reconstruction equally prevents robust digital recreation of the past. Moreover, using simulation to recreate real-world systems in general, even modern ones, is fraught with other conceptual and practical difficulties (Banks, Lempert, and Popper 2002). A much more useful approach is to use ABM and related simulation approaches as means to carry out replicable, controlled experiments in social dynamics, the results of which can be tested against empirical data (Ibid.). For archaeology, this means we can create experimental designs to examine alternative hypotheses about social process, and evaluate them against the testbed of diverse social and ecological outcomes of human action represented by the archaeological record. Used in this way, the fragmentary nature of the record, scattered through space and time actually makes it a better dataset for testing models of social dynamics than the much less diverse set of societies found in the world today. Models that can account well for

archaeological residues of social phenomena in diverse contexts spread across centuries or millennia are likely to be comparatively robust. The incomplete nature of the record is less of a problem when used as a testbed in this way than as a basis for reconstructing particular past social systems

For the past several years, I have directed a research team that is creating a computational modeling laboratory for studying the social and ecological consequences of land-use practices at regional spatial scales and century temporal scales. We are using the archaeological record of the Holocene Mediterranean as a testbed for this laboratory, though other regions of the world would also serve well. The laboratory will couple ABM for human land-use decisions and practices, with surface process models (themselves combining differential equations with cellular automata) in a GIS framework. The technical details of the modeling environment are described elsewhere (Barton, Ullah, and Mitasova n.d.; Mayer et al. 2006), but some of the preliminary results provide an example of an experimental modeling approach.

In initial testing of the modeling laboratory, we have examined how varying land-use practices and village size affects erosion, deposition, and vegetation cover at different temporal and spatial scales (Figure 5). In this experimental context, we are able to do something that is impossible using the archaeological record: we can recreate the landscape and simulate surface processes and vegetation change in the absence of humans. Then we can add humans to the landscape and assess the differences. One of the more interesting results of our experiments involves identifying 'tipping points' or 'thresholds' in the effects of land-use practices.

In small hamlets of a few families, shifting cultivation and grazing in Mediterranean woodland alter vegetation cover and cause erosion in some places and deposition in others. However, vegetation degrades slowly and is offset by regrowth. Erosion tends to most strongly affect upland areas that are not cultivated and, hence, has little economic impacts. Although there is more net erosion than deposition, the redeposition of eroded soil roughly keeps pace with the erosion rate and most strongly affects areas in cultivation—renewing fertility and even extending

potentially farmable land. Increasing the extent of agropastoral land use, due to population growth for example, will tend to also increase the productivity of the system...up to a point.

At some point in the growth of settlements and accompanying agropastoral land-use, a tipping point is passed in which expansion no longer increases productivity. In the context of our experimental design, this tipping point had already been passed for villages of 50 to 100 occupants. In these slightly larger communities, erosion affects cultivated as well as uncultivated areas. Also, the ratio of redeposition:erosion is considerably lower than for tiny hamlets and continues to drop at an increasing rate the longer the area is farmed. In other words, the more people expanded their agropastoral practices after a certain settlement size has been reached, the more it degraded the productive potential of the landscape. The deleterious effects of passing such a tipping point seem to only become apparent after several generations, beyond the personal experience of farmers and their parents. Possible solutions to this dilemma include reducing the size of farming communities, increasing reliance on pastoralism (because it increases the deposition:erosion ratio for cultivated parts of the landscape), or intensification in the form of conservation measures such as terraces. All three solutions are seen archaeologically in the Near East after the initial expansion of farming villages in the Prepottery Neolithic. Experimental modeling of land-use/landscape interaction and testing this against the archaeological record also offers new insight into the past societies. We plan to continue this experimental program, examining the effects of other phenomena such as climate change, market economies, and anthropogenically triggered changes in biodiversity.

Challenges to an Archaeology of Social Dynamics

Initiating a program to redirect archaeological research towards the the creation and testing of explicit, quantitative models of social dynamics involves a number of significant challenges, even beyond convincing sufficient archaeologists that it is a desirable goal. Conceptually, one of the most difficult challenges will be for archaeologists to learn how to express social process and individual practice in algorithmic form. I am well aware of that many archaeologists may feel that human social processes simply cannot be adequately expressed as quantitative models. In

fact, many aspects of models of social dynamics can be expressed explicitly in the prose of natural language. Nevertheless, it usually will take many words to express a dynamic process with the same degree of unambiguous specificity of a formal model in mathematical or algorithmic form. Furthermore, except for simple Boolean statements (e.g., if x then y) prose cannot generate testable results with the same degree of specificity and replicability of quantitative models. Finally, natural language prose cannot be executed in computational simulations to carry out tedious experimental designs (e.g., when the same algorithm is repeated 100 times with incremental variation in a critical parameter to test model sensitivity). Pragmatically, in order to develop explicit and unambiguously testable models of social process, we will need express them quantitatively as formal models. Moreover, it is much better that we, as social scientists, learn how to express social dynamics in this way than to depend on others trained solely in the computer or mathematical sciences to do so.

Some may note that the examples I provided focus especially on human ecology and interactions between societies and the environment. This is not because such dynamic interactions are more amenable to quantitative and computational modeling than purely social practice and interactions among people. It is more a function of the fact that 1) human-environmental interaction is a topic of considerable interest to many archaeologists, and 2) quantitative modeling is more common in the natural sciences and archaeologists carrying out research in this domain are more likely to be familiar with such models. Nevertheless, processes operating within societies and between individuals warrant at least as much effort dedicated to development of quantitative and algorithmic models as do interactions between societies and their natural environments. Models of social practice are being explored currently (e.g., Janssen et al. 2008) but their scope needs to be expanded to address social phenomena like the relationships between structure and practice, the growth of political hierarchies, and the role of perception in decision making.

While archaeologists have been steadily gaining basic quantitative skills over the past several decades, the level of mathematical training still is not high overall. And many fewer have

experience with algorithmic expression needed for constructing computational models. These are generally looked on as expertise to be gained in a few specialized courses, over and above more 'fundamental' anthropological knowledge. To make such modeling central to archaeological practice means updating the curricula in undergraduate and graduate anthropology programs to require more advanced mathematics training and to require training in basic programming skills or even social simulation. Importantly, relevant concepts should be integrated into basic courses on archaeological principles, methods, and theory rather than left to extra electives. To jump-start the widespread acquisition of such expertise, special summer courses and workshops could be offered at the SAA meetings or other professional venues. Increasing the level of competence in formal modeling has additional beneficial side effects. Most students entering the job market in the coming years will benefit greatly by some degree of familiarity in applied computational methods, or informatics, regardless of their career track. Furthermore, more widespread understanding of formal and computational modeling will make it easier for those carrying out model-centric research to have their results funded and published so that others can learn from them and build on them. Currently, grant proposal reviews and journal reviews of research with significant computational modeling components suffer from a lack of qualified reviewers for such projects.

Finally, testing explicit, falsifiable models against the archaeological record is easier than testing models expressed as narrative prose, but testing computational models is still far from straightforward. Many of the expectations of HBE models cannot easily be tested with archaeological data because they specify individual behaviors and phenomena like caloric intake that are very difficult to identify archaeologically. It is important that models be designed so that they *can* be tested against the archaeological record that is comprised of bits and pieces of discarded trash. Spatially explicit models like ABM's inherently map on to archaeological materials somewhat better than aggregate or individual behavior equations. However, because of the newness of ABM, there is not yet a set of widely agreed on protocols to assess the strength of matches between ABM results and real-world phenomena. With experience in adapting statistical

techniques to the needs of archaeological data, archaeologists could make important contributions to methods for validating complex computational models.

In conclusion, I want to be clear that I do not advocate that we collectively abandon our interests in the past. Much of the appeal of archaeology to its practitioners—and to the general public—is its ability to imagine life in worlds far removed from our own in time and space. The excitement of discovery during fieldwork and the satisfaction of solving the complex puzzle of meaning embedded in a fragment of a human-made object are fundamental to our intellectual satisfaction in this often esoteric field. Even with a more model-centric approach to archaeology, we still carry out our research in the very wide and still mysterious world of the human past. Nevertheless, if we are to extend the application of archaeological knowledge beyond anthropology, we must center our discipline on being a social science that gathers its data from the long human past, rather than being a discipline of prehistorians. It is clear that people in a wide variety of other domains would benefit from a better understanding of the long-term consequences of human action, an understanding that only archaeology can provide. By enlarging our vision outward in this way, we make our unique field of scholarship more valuable to humanity and grow it in new and exciting directions.

Figure 1. First principle component of genetic markers for Neolithic wave of advance (after Cavalli-Sforza 1997, figure 2).

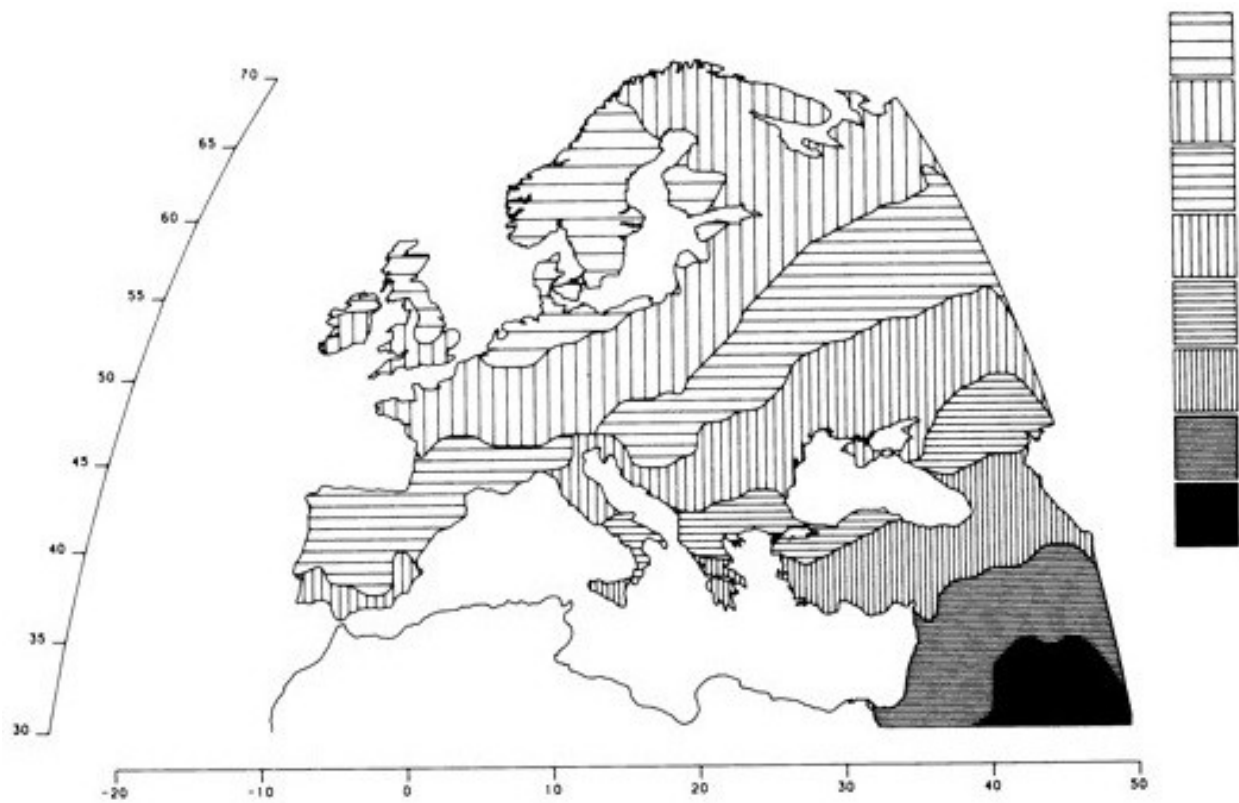


Figure 2. Balinese irrigated rice fields (Wikipedia Creative Commons).



Figure 3. Simulated subak output from Lansing simulation model (Lansing et al. 2009, figure 8).

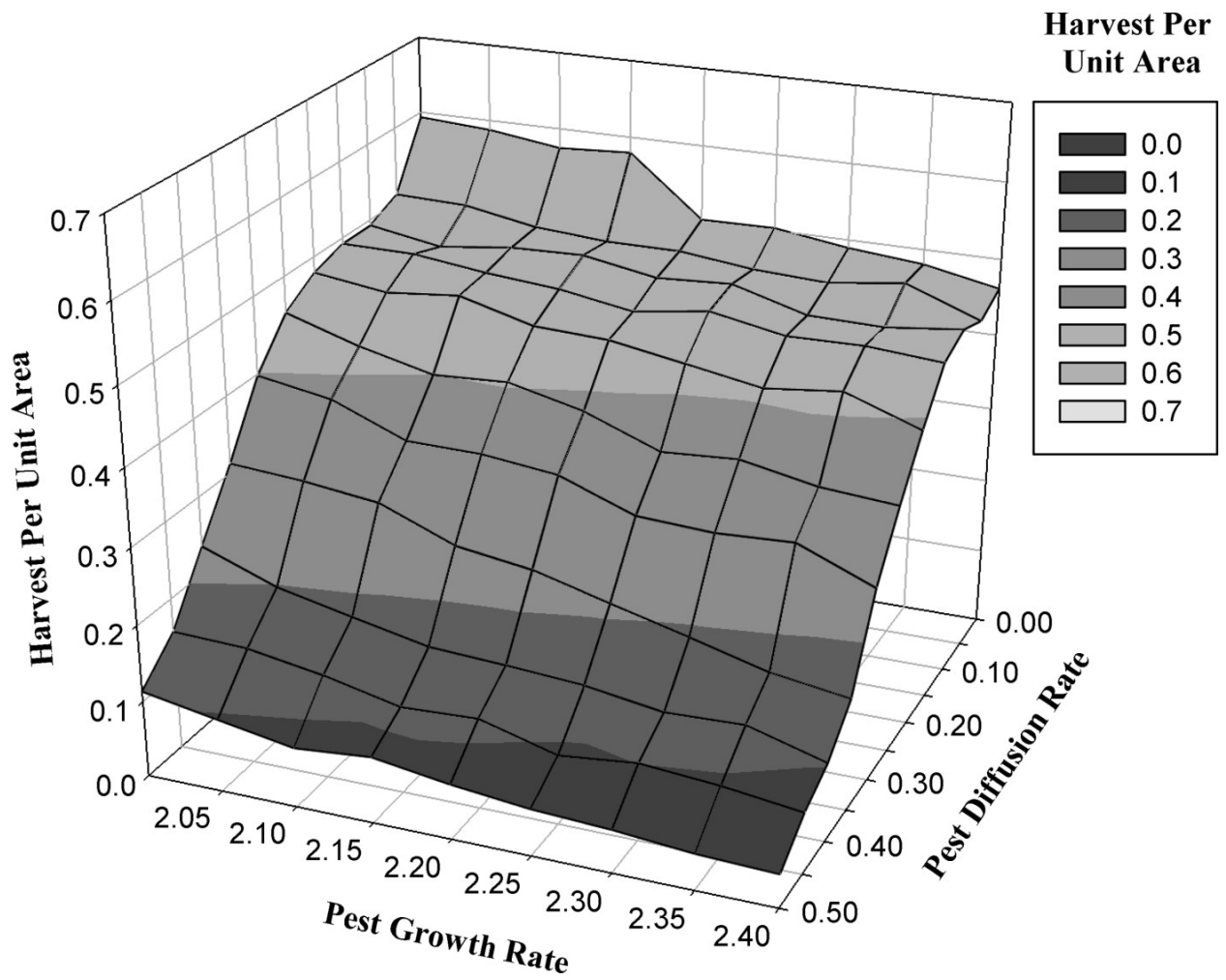


Figure 4. Ancient Anasazi simulation results and comparisons with empirical archaeological data (Kohler, Gummerman, and Reynolds 2005).

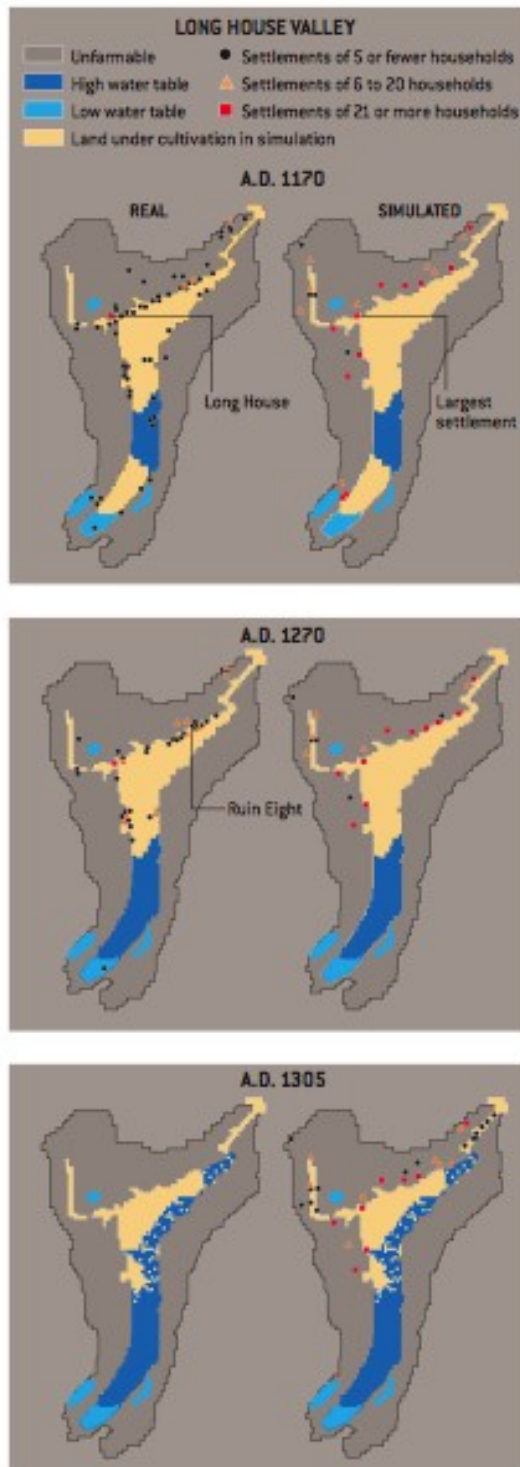
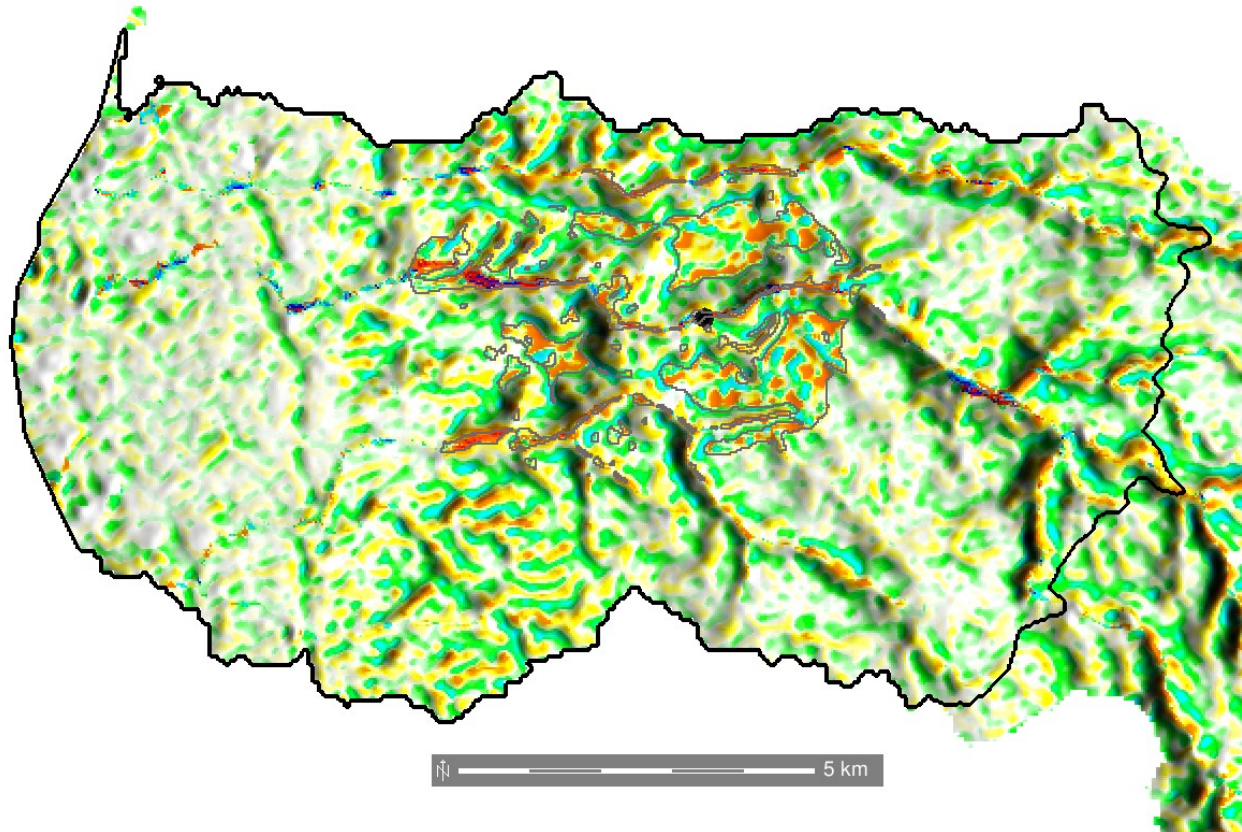


Figure 5. Net erosion and deposition in the Wadi Ziqlab watershed after 40 years of shifting cultivation and grazing. Yellow-red is increasing erosion, green through blue is increasing deposition. Agricultural catchment outlined in fine line, grazing catchment outlined in bold line (Barton et al, in review).



References Cited

- Ammerman, A. J, and L. L Cavalli-Sforza
1984 *The Neolithic transition and the genetics of populations in Europe*. Princeton University Press, Princeton.
- Axtell, Robert L, Joshua M Epstein, Jeffrey S Dean, et al.
2002 Population growth and collapse in a multiagent model of the Kayenta Anasazi in Long House Valley. *PNAS* 99(90003): 7275-7279.
- Banks, S., R. Lempert, and S. Popper
2002 Making Computational Social Science Effective: Epistemology, Methodology, and Technology. *Social Science Computer Review* 20(4): 377-388.
- Barton, C. Michael, Isaac Ullah, and Helena Mitsova
nd Computational Modeling and Socioecological Dynamics in the Neolithic of the Near East. *American Antiquity* (in review) (nd).
- Batty, Michael
2005 *Cities and complexity: understanding cities with cellular automata, agent-based models, and fractals*. MIT Press, Cambridge, Mass.
- Bettinger, Robert L
1991 *Hunter-gatherers: archaeological and evolutionary theory*. Plenum Press, New York.
- Bonabeau, Eric
2002 Agent-based modeling: Methods and techniques for simulating human systems. *PNAS* 99(90003): 7280-7287.
- Boone, J. L
1992 Competition conflict development of social, hierarchies. In *Evolutionary Ecology and Human Behavior*, edited by E. A Smith and B. Winterhalder. Aldine de Gruyter, New York.
- Cavalli-Sforza, L. Luca
July 22 1997 Genes, peoples, and languages. *Proceedings of the National Academy of Sciences of the United States of America* 94(15) (July 22): 7719-7724. doi:VL - 94.
- Dean, Jeffrey S, George J Gumerman, Joshua M Epstein, et al.
2000 Understanding Anasazi culture change through agent-based modeling. In *Dynamics in Human and Primate Societies: Agent-Based Modeling of Social and Spatial Processes*, edited by Timothy A Kohler and George J Gumerman. Oxford University Press, Oxford.
- Epstein, J. M, and R. Axtell
1996 *Growing Artificial Societies: Social Science from the Bottom Up*. Brookings Institution Press, Washington, D.C.
- Fisher, Christopher T, and Gary M Feinman
2005 Introduction to "Landscapes over Time". *American Anthropologist* 107(1): 62-69.
- Gilbert, Niles, and K G Troitzsch
1999 *Simulation for the Social Scientist*. Open University Press, Buckingham.

Janssen, Marco A

2007 Coordination in irrigation systems: An analysis of the Lansing-Kremer model of Bali. *Agricultural Systems* 93: 170-190.

Janssen, Marco A, R. L Goldstone, Filippo Menczer, and Elinor Ostrom

2008 Effect of rule choice in dynamic interactive spatial commons. *International Journal of the Commons*, 2(2): 288-312.

Janssen, Marco A, and Elinor Ostrom

2008 TURFs in the lab: Institutional Innovation in dynamic interactive spatial commons. *Rationality and Society* 20: 371-397.

Johnson, C. David, Timothy A Kohler, and Jason Cowan

2005 Modeling Historical Ecology, Thinking about Contemporary Systems. *American Anthropologist* 107(1): 96-107.

Kelly, Robert L

1991 Sedentism, sociopolitical inequality, and resource fluctuations. In *Between Bands and States*, edited by S. Gregg, 135-158. Occasional Paper No. 9. Center for Archaeological Investigations, Southern Illinois University, Carbondale, IL.

1995 *The Foraging Spectrum: Diversity in Hunter-Gatherer Lifeways*. Smithsonian Institution Press, Washington, DC.

Kennett, D. J., and B. Winterhalder

2006 *Behavioral ecology and the transition to agriculture*. University of California Press.

Kohler, T. A., and S. E. van der Leeuw

2007a Historical Socionatural Systems and Models. In *The model-based archaeology of socionatural systems*, 1-12. School for Advanced Research Press, Santa Fe, NM.

2007b *The model-based archaeology of socionatural systems*. School for Advanced Research Press.

Kohler, Timothy A, George J Gummerman, and Robert G Reynolds

2005 Simulating ancient societies. *Scientific American* 293(1): 76-84.

Lansing, J. Stephen

1991 *Priests and Programmers: Technologies of Power in the Engineered Landscape of Bali*. Princeton University Press, Princeton, N.J.

2006 *Perfect Order: Recognizing Complexity in Bali*. Princeton studies in complexity. Princeton University Press, Princeton, N.J.

Lansing, J. Stephen, Murray P. Cox, Sean S. Downey, Marco A. Janssen, and John W. Schoenfelder

2009 A robust budding model of Balinese water temple networks. *World Archaeology* 41(1): 112. doi:10.1080/00438240802668198.

Lansing, J. Steven, and James N Kremer

1993 Emergent properties of Balinese water temple networks: coadaptation on a rugged fitness landscape. *American Anthropologist* 95(1): 97-114.

van der Leeuw, Sander, and Charles L Redman

2002 Placing archaeology at the center of socio-natural studies. *American Antiquity* 67(4): 597-605.

Mayer, Gary R, Hessam S Sarjoughian, Eowyn K Allen, Steven E Falconer, and C. Michael Barton

2006 Simulation modeling for human community and agricultural landuse. In *Agent-Directed Simulation, Proceedings of the Agent-Directed Simulation Multi-Conference, Huntsville, Alabama, 65-72*. Huntsville, AL.

Parker, Dawn C, Steven M Manson, Marco A Janssen, Matthew J Hoffmann, and Peter J Deadman

2003 Multi-agent systems for the simulation of land-use and land-cover change: a review. *Annals of the Association of American Geographers* 93(2): 314-337.

Redman, Charles L

2004 *The archaeology of global change : the impact of humans on their environment*. Smithsonian Books, Washington.

Richards, Martin

2003 The Neolithic transition in Europe: archaeological models and genetic evidence. *Documenta Praehistorica* 30: 159-167.

Shennan, Stephan

2002 *Genes, Memes, and Human History: Darwinian Archaeology and Human Evolution*. Thames and Hudson, London.

Tomasello, Michael

1999 The Human Adaptation for Culture. *Annual Review of Anthropology* 28: 509-529.

Turchin, Peter

2008 Arise 'cliodynamics'. *Nature* 454: 34-35.

Winterhalder, Bruce, and Eric Alden Smith

1981 *Hunter-Gatherer Foraging Strategies: Ethnographic and Archeological Analyses*. University of Chicago Press, Chicago.

2000 Analyzing adaptive strategies: human behavioral ecology at twenty-five. *Evolutionary Anthropology* 9(2): 51-72.

Zilhão, João

2001 Radiocarbon evidence for maritime pioneer colonization at the origins of farming in west Mediterranean Europe. *Proceedings of the National Academy of Sciences* 98(24): 14180-14185.