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From Narratives to Algorithms: Extending Archaeological Explanation beyond Archaeology

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Abstract and Keywords

The static, fragmentary nature of the archaeological record requires us to construct models of past human dynamics. Traditionally, these have been in the form of narratives that can make compelling stories but are difficult to evaluate. Recent advances in numerical and computational modelling offer the potential to create quantitative representations of human systems and carry out experiments in social dynamics that would otherwise be impossible. These new approaches challenge us to learn to conceive of human societies in ways that can be expressed in algorithmic form. Besides making our own explanations more rigorous, integrating quantitative modelling into archaeological practice helps us produce more robust accounts of human systems and their long-term changes that can be more useful to other disciplines and policy-makers than compelling narratives.

Keywords: computational modelling, archaeology, archaeological practice, quantitative representation, human systems, social dynamics

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. Archaeology documents the long-term dynamics of human societies and their interactions with the world around them at global scale and beyond the few surviving written records from the very few literate societies that existed until quite recently. This makes archaeologists uniquely placed to offer insights into the diverse, underlying processes of long-term social change and ecological interactions (Fisher et al., 2009; Redman et al., 2004; van der Leeuw and Redman, 2002).

It is important to realize, however, that the claim that archaeological research and its results have 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 practitioners 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. From this perspective, we can even debate whether such a 'true' past even existed, and we can afford to be ecumenically tolerant of alternative reconstructions 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 to identify—and improve on—better and worse accounts of human societies and the ways they have changed.

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 programmes in universities are seeing budgets shrink alarmingly. On the other hand, the growing recognition that an understanding of human social dynamics—as well as 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 and society more broadly. 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.

Archaeological Data and Research Protocols

If we embark upon this course, we must also accept the accompanying greater responsibility for the usefulness of the results of archaeological research. 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 twentieth 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, however, the study of buried archaeological materials takes place over only a minuscule 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 built 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 biased fraction of originally available human behavioural residues.

This does not mean that we need to embark on a massive new programme of intensive fieldwork to collect more data, nor is the state of the archaeological record necessarily a 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 behaviours and organizations from multiple lines of mutually corroborating evidence (Wylie, 1992), 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 our audiences. However, the data we use for reconstructions are static, highly altered, secondary or tertiary residues of dynamic processes—not the processes themselves. Through careful application of middle-range theory (i.e. linking human behaviour to the archaeological record [but see Smith, 2011]) 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 imaginative 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. But, if we are social scientists 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, cannot be evaluated in this way. Because they are created from often lengthy chains of inferences, mixed with a considerable amount of intuition, drawn from sparse proxy data, they can easily be adjusted to fit the data. This makes them very difficult to falsify; and testing them against the data from which they ultimately are inferred is circular and hence of little help in this endeavour. Furthermore, the narrative character of reconstructions weaves together the complex interactions of many social, behavioural, 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 intuited from 'common sense' understandings of society and rarely examined in detail (Dunnell, 1982). Archaeological narratives that reconstruct the past can make for compelling reading and serve as useful cautionary tales, but we lack a robust way to evaluate the degree to which they accurately represent 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. But 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 detailed operations of past human societies and processes of social change through chains of inference. In fact, I propose that we should seriously reconsider the practices of attempting to reconstruct the past as the primary means of studying human society and its dynamics. Narrative reconstruction certainly has an important, educational role for describing humanity's 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 practising a 'science of artefacts' that has been made more versatile and powerful by recent advances in information technologies. We can create explicit models of individual behaviour and social change, express these models mathematically or computationally, and compare the results against the empirical archaeological record (Kohler and van der Leeuw, 2007a; Turchin, 2008; van der Leeuw, 2004). This approach in fact follows the more general hypotheses-testing protocol 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 and conveying our knowledge to others (i.e. most of us think in natural language, not mathematical terms). 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. The 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 behaviour 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 principally be about crafting accounts of the past, but should emphasize making and testing models about society and individual practice (see also Minnis, this volume). 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, following the inferential, narrative reconstructions of ancient lifeways. Moreover, rather than testing inferences about the past, we should at the outset be testing models of social dynamics against the archaeological record; the *implications* of such model testing can be insights about life in the past. Such a reorganization in the 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 behaviour, rather than reconstructing ancient life, then it is no longer a problem per se that 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, rather than serve as questionable ‘analogy’ to populate long-dead societies.
- Models are 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 south-west and Neolithic Greece, but it is problematic to test a reconstruction of the abandonment of ancient 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 also can 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 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 Epipalaeolithic foragers of the Near East or mid-Holocene pastoralists of the southern fringe of the Sahara.
- Finally, redirecting archaeological research towards 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 behavioural 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 modelling provide the 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 in the next section), recent developments in computational social simulation provide new opportunities to build spatially explicit models that combine rule-based individual action and larger scale social process in new and complex ways (Barton et al., 2010; Batty, 2005; Bonabeau, 2002; Gilbert and Troitzsch, 1999; Kohler and van der Leeuw, 2007b; Parker et al., 2003). These new simulation approaches and their results 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. In the next section, 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

A quarter-century ago, Albert Ammerman and Luigi Cavalli-Sforza (1984) proposed a quantitative, dynamic model of how small-scale farming spreads across regions. They suggested that subsistence farming communities expand by demographic growth and the establishment of daughter communities (i.e. as residents of existing communities strike out to found new ones) in territory not previously occupied, a process they termed ‘demic diffusion’. They expressed this model in the form of spatially referenced reaction 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.

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. It revolutionized the archaeological perspective on the origins of farming in Europe and became a sort of ‘null’ model (i.e. requiring a minimum of a priori assumptions) 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 and quantitative, 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.

As is common with first models, aspects of it have been falsified by better data. However, its overall approach remains the basis of more elaborate models recently developed for the spread of agriculture and other human dispersals (Davison et al., 2006; Fort et al., 2012; Lemmen et al., 2011; Russell and Steele, 2009; Steele et al., 1998; Bernabeu Aubán et al., 2015). Ultimately, it will not be better data but only a better scientific and testable model that can replace it as robust framework for understanding space–time regional dispersals among small-scale agropastoral societies.

Human Behavioural Ecology

Human behavioural ecology (HBE) is an outgrowth of decades of animal behaviour studies. It takes a different approach from that of Ammerman and Cavalli-Sforza, employing a suite of mathematical optimization models of (primarily) individual behaviour in various ecological and social contexts. 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 Foley (1985), Robert Kelly (1991, 1995), Robert Bettinger (1991), and James Boone (1992), among others. 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; Shennan, 2008). There is little consensus, however, about the applicability of HBE models for social systems with more complex economic and social organizations than hunter-gatherers.

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 behaviour. Rather the models are often used in a more reconstructionist mode to provide an underlying explanation for inferred past behaviour. However, HBE models are also beginning to be used as the basis for individual decision rules in more complex computational modelling (e.g. Premo, 2005).

A pragmatic limitation of both HBE models and the diffusion model of Ammerman and Cavalli-Sforza is that neither deals explicitly with variable behaviour among multiple, interacting, individual members of human societies. HBE models focus on the actions of generic individuals under specified circumstances, but treat these as representative of social aggregates. 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 abstract pairs of individuals. 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 behaviour are aspatial, even if they refer to spatially variable behaviours like moving to new foraging patches. Sometimes this is irrelevant to the explanatory power of the model, but can misrepresent socioecological processes in other contexts. Diffusion models likewise 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 modelling are helping to resolve these issues by assigning rule sets—including rules derived from HBE—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 while maintaining a large degree of individual autonomy and diversity.

Simple Rules and Complex Systems

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Figure 1 Balinese irrigated rice fields (Wikipedia Creative Commons).

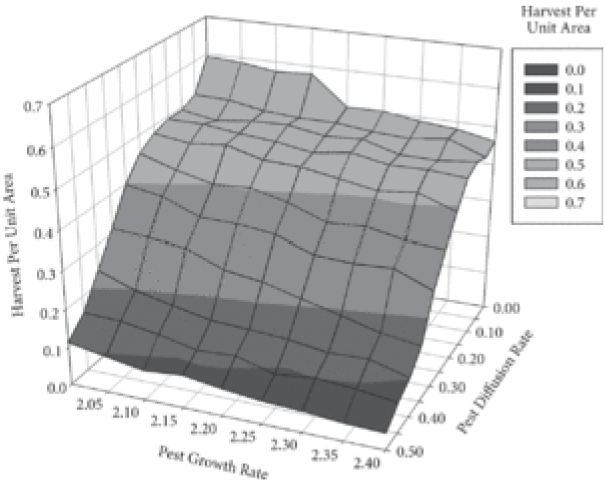

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Figure 2 Simulated subak output from Lansing simulation model (Lansing et al., 2009: figure 8;

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Following on more than a decade of ethnography in Bali, Steven Lansing turned to a new kind of computational modelling, agent-based modelling (ABM; see also Heckbert et al., this volume), 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 in 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 that cropping schedules were highly optimized by Balinese farmers without large-scale, centralized planning and management (Fig. 1).

By creating an ABM simulation with farmer agents using simple behaviour rules, Lansing found that he could create a complex optimal cropping schedule needed to maximize production (Fig. 2). Farmers simply needed to observe the cropping schedule of their local neighbours and copy the schedule of the most successful neighbour in the subsequent year. The cropping schedule simulated with agents following these rules closely matched the system observed for real-world Balinese rice growers. Lansing was able to show in clear, quantitative terms how simple behavioural rules could produce complex social practices. His work also offered important insight into the management of irrigation systems, and showed why the 'bottom-up' actions of local farmers were successful in providing sufficient water for crops and reducing pest infestations over a large area, while a 'top-down' programme 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 contexts 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 (Epstein and Axtell, 1996) to study the abandonment of regional landscapes in the American south-west (Axtell et al., 2002; Dean et al., 2000). 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 ▯ 800 to peak around 1250; subsequently, the valley seems to have been entirely abandoned by 1300. While archaeological interest in this simulation was stimulated by the fact that some runs were able to closely match the empirical demographic changes in the valley over 500 years, a more interesting general result is that many model runs did *not* match the outcomes of the prehistoric case (see Janssen, 2009: fig. 3). 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 the question of why people abandon regions in times of stress when they can still make a living. Importantly, this initial modelling project stimulated a new model-centric research project in the northern south-west, directed by Kohler (Johnson et al., 2005; Kohler et al., 2005). The original Artificial Anasazi simulation recently has been repackaged for demonstrating and teaching ABM (ascape.sourceforge.net/). More importantly, as is the case with other explicit, quantitative/algorithmic models, Kohler and colleagues have been able to significantly enhance the capabilities of his initial modelling environment to ask new questions about the complex interactions between social and environmental change and the dynamics of ancient societies (Kohler and Varian, 2010; Kohler et al., 2012).

Modelling as Laboratory

Controlled experiments have been useful in many natural sciences for identifying critical parameters and underlying drivers of change. However, in social sciences in general and historical social sciences in particular, 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 behaviours or small groups. While such small-scale experiments are important for understanding the fundamental bases for human social behaviour (e.g. Janssen et al., 2010; Mesoudi and Whiten, 2008; Tomasello, 1999), 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 time 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, as noted earlier, the nature of the archaeological record means that it often serves better as a diverse and extensive test bed for models than a laboratory.

An outgrowth of the application of new forms of computational modelling 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 (see Heckbert et al., this volume). Notably, the goal of such modelling is not to simulate the past—i.e. reconstruct the ‘true’ past in a computer instead of in prose. The problematic nature of the archaeological record that 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—not the least of which is the complexity of real socioecological systems and uncertainty about how diverse phenomena interact in detail from microscopic to landscape scales, and about the range of reasonable values for these parameters (Bankes et al., 2002). A more useful approach in many settings is to use ABM and related computational model-based approaches as means to carry out replicable, controlled experiments in social dynamics, the results of which can be tested against empirical data (Bankes et al., 2002). For archaeology, this means we can create experimental designs to examine alternative hypotheses about social process, and evaluate them against the test bed 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 can actually provide a better data set for testing models of long-term 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 test bed in this way than as a basis for reconstructing particular past social systems.

For the past decade, the Mediterranean Landscape Dynamics Project (MedLand) has been developing and using a computational modelling laboratory for studying the social and ecological consequences of rural land-use practices at regional spatial scales and century temporal scales. The project is using the archaeological record of the Holocene Mediterranean as a test bed for this laboratory, though other regions of the world could serve equally well. The MedLand Modeling Laboratory (MML) couples ABM for human land-use decisions and practices, with surface process models combining differential equations with cellular automata (a computational model that represents a real-world system as a regular grid of equal sized cells, each assigned a set of rules in which its state can change in response to changes in the states of neighbouring cells; see also Heckbert et al., this volume) in a Geographic Information System (GIS) framework. The technical details of the modelling environment are described elsewhere (Barton et al., 2010, 2012; Mayer and Sarjoughian, 2007; Mitasova et al., 2013), but a brief review of some of the results provide an example of an experimental modelling approach.

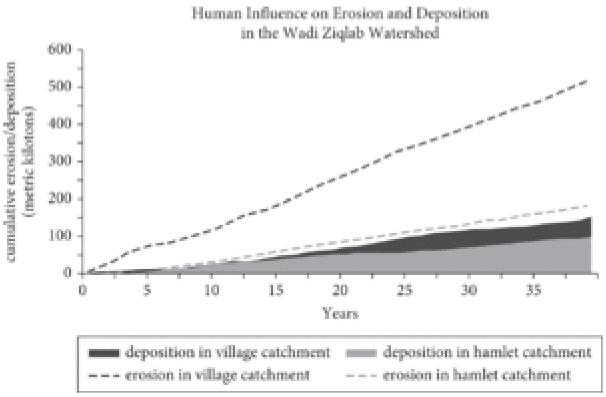

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Figure 3 Cumulative anthropogenic erosion and deposition in the Wadi Ziqlab watershed (northern Jordan) during 40 years of shifting cultivation and grazing by communities of different sizes. Grey line is erosion and grey area is deposition for tiny hamlet; black line is erosion and black area is deposition for small village.

In one of the initial experiments in the MML, we examined how varying land-use practices and village size affects erosion, deposition, and vegetation cover at different temporal and spatial scales (Fig. 3). 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 this suite of experiments involved identifying ‘tipping points’ or ‘thresholds’ in the landscape impacts of agropastoral land-use practices.

We contextualized these experiments in the prehistoric landscapes of northern Jordan. 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, grazed vegetation degrades slowly and is offset by regrowth. Erosion tends to most strongly affect upland areas that are not cultivated and, hence, has little direct economic impacts on farmers. Although there is more net erosion than deposition, the redeposition of eroded soil roughly keeps pace with the erosion rate and most accumulates in cultivated zones along wadi bottoms (i.e. intermittent watercourses in this semi-arid landscape)—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 and the ratio of redeposition to erosion is considerably lower than for tiny hamlets and continues to decline 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. Our modelling suggested that 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 Pre-pottery Neolithic. Experimental modelling of land-use/landscape interaction and testing this against the archaeological record also offers new insight into past societies.

Currently, we are beginning a new series of experiments to better understand the interaction of social and natural processes that created today’s Mediterranean landscape (Barton et al., 2012). These experiments are set in the Penaguila Valley of eastern Spain, a region characterized by a series of Pliocene and Pleistocene terraces into which deep *barrancos* (ravines) were incised sometime after the earliest Neolithic (Barton et al., 2002, 2015). The archaeological record of this region also indicates significant social changes between the early and late Neolithic (Neolithic I–IIa, *c*.5600–3700 cal BC, and Neolithic IIb–c, *c*.3700–2300 cal BC) in settlement location and size, domesticates emphasized, and social complexity and differentiation (Bernabeu Aubán et al., 2006; McClure et al., 2009; Miller et al., 2009).

Finally, palaeoclimatic models display shifts in the seasonal distribution of precipitation between the earlier and later Neolithic. Taken together, this suggests that some combination of social change, land-use change, and climate change initiated geomorphic processes that produced the modern landscape (McClure et al., 2009; Miller et al., 2009).

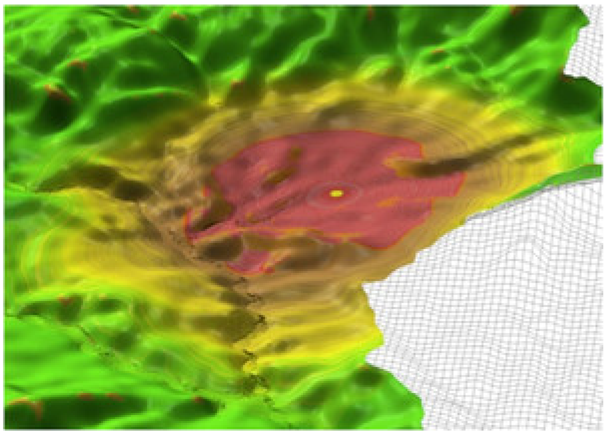

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Figure 4 PaleoDEM of the Penaguila Valley (northern Alicante Province, Spain) after 500 years of simulated agropastoral land-use. Small bright circle marks a Neolithic village, surrounded by zones of anthropogenic land-cover that range from cultivated (nearest the village) to grazed (farthest from the village). Incised *barrancos* can be seen in the lower left third of the image.

In the MML, we created and parameterized a small Neolithic settlement using information from archaeological research at the Cardial Neolithic site of Mas d'Is (Bernabeu Aubán et al., 2003, 2006) and a wide array of ethnographic information about subsistence farming. Because the modern landscape is a product of thousands of years of complex human–environment interaction, we created a simulated landscape that approximates early Holocene conditions by combining geoarchaeological fieldwork with GIS tools, in which we situated the modelled Neolithic community. This experimental design allows us to quantitatively measure the extent to which the modern landscape has diverged from early Holocene conditions and compare that divergence with the results of different modelling experiments.

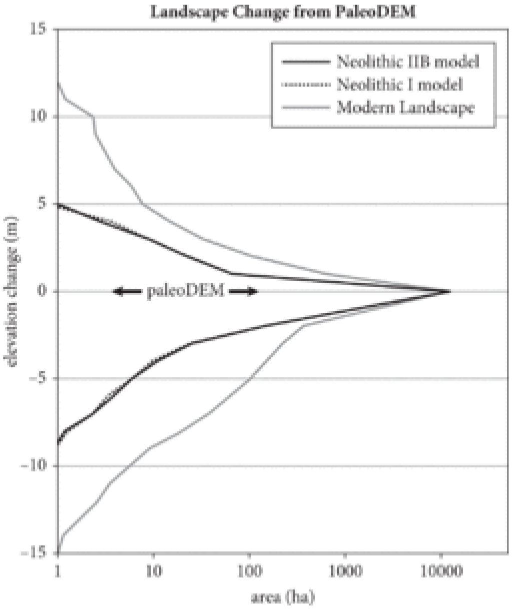

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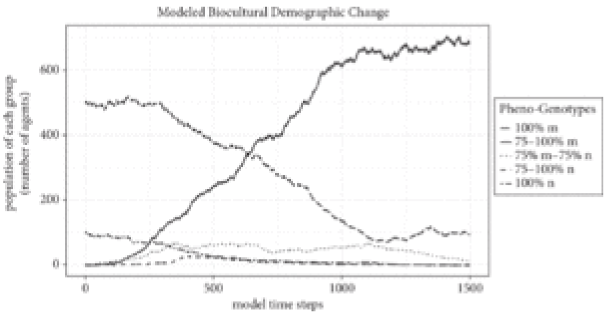
Figure 5 Graph shows the relationship between PaleoDEM, modern landscape, and modelled landscapes after 500 years of agropastoral land-use. Grey line indicates the amount that the modern landscape differs from the paleoDEM in terms of the areas that are lower than their corresponding locations in the paleoDEM (erosion) and higher than the corresponding locations of the palelDEM (deposition). The black solid and dashed lines similarly indicate divergence in the modelled landscape from corresponding locations in the paleoDEM.

So far, we have carried out initial experiments under two different climate regimes that may have prevailed during the earlier and later Neolithic (e.g. Miller et al., 2009). Fig. 4 shows that the modelled terrain after 500 years of agropastoral land-use has altered the paleoDEM (digital elevation model), with the shading indicating anthropogenic land-cover change. Incised *barrancos*, like those that characterize the modern landscape, can be seen clearly within the major and some minor drainages. Fig. 5 compares the result of land-use/landscape modelling with early and later Neolithic rainfall patterns to the modern landscape. The modern landscape and both Neolithic models are represented as divergence from the paleoDEM.

Both Neolithic I and IIb models show landscapes that are shifting towards the modern terrain pattern, although neither model has converged on the modern landscape after 500 years of land-use. Importantly, almost identical results are produced under both Neolithic I and Neolithic IIb climatic conditions. We emphasize that these are very preliminary results, and we have not yet completed the kind of multiple runs and sensitivity tests needed to confirm them. However, if they hold up in further experimental work, it appears that climate change of the magnitude estimated for the Neolithic is not of itself a primary driver of modern landscape formation in this region. In subsequent experiments we have examined the impacts of different land-use strategies on the evolution of different parts of the Mediterranean landscape. This work indicates that human-driven changes have very different signatures than landscape evolution caused by climate change (Barton et al., 2015).

Models beyond Farming

Some may note that the previous examples focus especially on interactions between societies and the environment, especially in the context of small-scale, agropastoral societies. This is not because such systems are more amenable to quantitative and computational modelling, but more a function of the fact that (1) human–environmental interaction is a topic of considerable interest to many archaeologists, and (2) quantitative modelling is more common in the natural sciences and archaeologists carrying out research in this domain today are more likely to be familiar with such models. However, quantitative and algorithmic modelling are beginning to be applied to an increasingly diverse array of processes operating within societies, between individuals, between culture and biology, and at multiple scales of social organization.



Click to view larger

Figure 6 Numbers of agents of differing phenotypes and genotypes over the course of 1,500 model cycles with increased radius of foraging. The model starts with a smaller number of agents in Europe (representing Neanderthals) with a genotype of 'NN' at all alleles and a larger number of agents with a genotype of 'MM' at all alleles in the rest of the virtual world (representing 'modern' hominins). There are no migration or fitness differences between the agents. At the end of the simulation, the total population is dominated by agents with mostly M alleles, but with a small amount of N allele introgression, a small number of MM homozygotes with no introgression (i.e. like the original 'purely' non-Neanderthal agents), and no agents with NN genomes (i.e. original 'purely' Neanderthal), mostly N genomes (mostly Neanderthal), or even MN genomes (hybrid).

For example, Stephen Shennan and colleagues have used computational models to study how demography can impact social learning and rates of cultural change over human evolutionary time (Powell et al., 2009; Shennan, 2000). Recently, colleagues and I used computational modelling experiments to examine the consequences of changing forager land-use strategies on human biocultural evolution in western Eurasia during the onset of the last glaciation (Barton et al., 2011; Barton and Riel-Salvatore, 2012). This work demonstrates how adaptive changes in foraging strategies alone in response to climatic changes could lead to the apparent disappearance of Neanderthals (Fig. 6). At the opposite end of the spectrum of social complexity, Graham (2009) is using modelling to explore the dimensions of social networks and violence in Imperial Rome, while Cioffi-Revilla and colleagues (2008) are using computational modelling to examine the interaction of social and natural factors and their roles in the rise and fall of empires in central Asia.

These new approaches to computational modelling are still rare in archaeological research, but there appears to be a growing realization of their potential to provide new knowledge about long-term social dynamics that cannot be generated in other, more traditional ways (Wurzer et al., 2015). While advances in computing technology—both hardware and software—have made advanced modelling realistic and accessible for archaeologists, most of this work to date is very limited in scope (e.g. only a few hundred agents in virtual worlds comprising a few thousand landscape cells) and of necessity are relatively abstract. This is not necessarily a problem, since there is a considerable amount of hypotheses-testing that can be carried out with small-scale, abstract models, as the examples discussed here show.

On the other hand, increasingly powerful computers and more sophisticated software that can take advantage of new processors—or clusters of computers in a high-performance computing environment—to run multiple algorithms simultaneously in parallel offer the potential to create experimental modelling environments that more closely approximate real-world conditions and, hence, can be better evaluated against the empirical archaeological record. Kohler's new Village Ecodynamics Project and the MedLand Project, for example, have created highly realistic simulation environments in which to carry out their experiments. Models can run for days on high-end desktop computers, making the logical next step to move models like these to 'supercomputing' environments. But this will require resources and expertise different in scope and scale from those generally found in archaeological research, presenting challenges as well as opportunities.

Challenges to an Archaeology of Social Dynamics

Initiating a programme to redirect archaeological research towards the creation and testing of explicit, quantitative models of social dynamics involves significant challenges, even beyond convincing archaeologists that it is a desirable goal. Conceptually, one of the most difficult challenges will be for archaeologists to learn how to express social processes and individual practices in algorithmic form. While some archaeologists may feel that human social processes simply cannot be adequately expressed as quantitative models, it usually takes many words of natural language 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 as 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 to 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.

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 expressions 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 modelling as central an archaeological practice as excavation methods means updating the curricula in undergraduate and graduate anthropology programmes to require more advanced mathematics training and to require training in basic programming skills or even social simulation. Relevant concepts need to 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 national or international professional meetings. Increasing the level of competence in formal modelling has additional positive consequences for students. 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 modelling 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, an increasing proportion of funding programmes in social sciences include some expectation of modelling, while grant proposal reviews and journal reviews of research with significant computational modelling 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 behaviours 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 can map onto archaeological materials better than aggregate or individual behaviour equations. However, because of the newness of ABM, there is not yet a set of widely agreed protocols to assess the strength of correspondence 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, the ability of archaeology to make significant contributions to human knowledge—and its long-term viability as a field of study—depend on our discipline being a social science that gathers its data from the long human past, rather than only being a discipline of prehistorians. It is clear that people in a wide variety of other domains can 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.

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