

Material evidence: learning from archaeological practice

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MATERIAL EVIDENCE: LEARNING FROM ARCHAEOLOGICAL PRACTICE

Alison Wylie and Robert Chapman

A PASSION FOR THINGS

In recent decades, the arcana of archaeology have come sharply into focus as a subject and a resource that humanists and social scientists cannot afford to ignore, however resolutely text-based or wedded to face-to-face, “reactive” modes of inquiry they may be. The “in-depth study of things,” declare the editors of the *Object Reader* (Candlin and Guins 2009: 2), has taken shape in a sprawling diversity of research programs ranging from metaphysical interrogation of materiality as such, to probing analyses of the ways in which meaning and matter are entangled in specific objects and contexts of action. Object biographies have captured popular imagination, in the form of the wildly successful *History of the World in 100 Objects* (MacGregor 2010), and now figure as prominently in the history of science and technology as in art history and cultural studies (Daston 2008). This attention to objects – this appreciation of the dynamic, consequential social lives of things – has catalyzed the formation of a distinct interdisciplinary field of material culture studies (Myers 2001: 5),¹ one that now has a history of its own in which the insights that set the field in motion are themselves subject to critical scrutiny. To insist that things be seen as a medium through which the social is articulated and meaning communicated is now decried as a “colonization of the object by the subject and the social” (Candlin and Guins 2009: 4), charged with trading in the very Cartesian oppositions between mind and matter it was meant to displace (Henare et al. 2007: 1-3).² The turn to things – objects, the body, artifacts, traces – is thus reinforced by renewed insistence that objects must be engaged in material as well as symbolic and social terms.

In all these contexts object studies are compelling, not only because the stuff of lives lived is intrinsically interesting and is constitutive of these lives, but because it is

invaluable as evidence. Thinking with (or through, or about) things has opened up otherwise inaccessible areas of inquiry and it has reconfigured our understanding of a great many longstanding topics of social scientific interest, from the dynamics of popular culture to the form and logic of political regimes (Auschlander 1996), from the condensation of value and the nature of commodities to the ramifying construction of social difference and solidarity (Appadurai 1986; Myers 2001). The brief for assembling *A History of the World in 100 Objects* was to “tell a history of the world that [had] not been attempted before,” one that is “truer,” more comprehensive and, crucially, “more equitable than one based solely on texts” (MacGregor 2010: xv, xxv, xix). Objects and traces have the potential to “give voice” to those who left no texts, to contest history as written by elites and victors, to bear witness to dimensions of life no one thought to tell, or actively suppressed.

This commitment to explore the kinds of history that “only a thing can tell” (MacGregor 2010: xxii) is not new. “History from below” has been championed at least since the Marxist Historians Group took shape in the UK after WW2, and it has antecedents dating to the 1930s (Beard 1935, Becker 1931). Although E. P. Thompson’s *Making of the English Working Class* (1963) is the most widely cited example,³ Hilton is especially interesting because his research on medieval peasantry lead him to initiate, with archaeologist Phillip Rahtz, the excavation of a deserted medieval village at Upton (Goucestershire); evidence of people’s houses and everyday possessions had the potential, he thought, to enlarge the scope of inquiry beyond dependence on texts.⁴ Expanding on the tradition of class-based analysis, feminist, critical race, and postcolonial historians, to name a few, have demonstrated just how different history looks when centered on the lives of those who have largely been written out of account. But to tell these counter-histories – of the everyday, of habit and localized practice, of the marginal – requires considerable ingenuity, reading canonical texts against the grain, expanding the archive to include

what had been dismissed as ephemera and, crucially, drawing on the non-textual evidence afforded by physical traces and material things. Historical archaeologists have been especially forthright in insisting that rigorous scrutiny of material evidence is not just a supplement to text-based histories but often the only resource we have for exposing and correcting “superficial and elitist...myths[s] for the contemporary power structure” (Glassie 1977: 29): the systematic distortions that arise from ignoring “the inarticulate” (Ascher 1974: 11), the “endless silent majority who did not leave us written projections of their minds” (Glassie 1977: 29). These themes are taken up by contributors to a recent discussion of “Historians and the Study of Material Culture” in the *American Historical Review* (Auslander et al. 2009) which begins with the observation that, “while some might still associate [the study of material culture] with objects found in museums or things from the remote past, it is in fact a field that takes an interest in all conceivable objects and every historical period”; it is especially relevant to any historical subject that takes as its subject a “concern for everyday life and the material circumstances of ordinary people” (*AHR editor*, Auschlander 2009: 1355).

Parallel arguments for attending to material evidence also figure in sociology, although for more strictly methodological reasons. In a classic of the 1960s the proponents of *Unobtrusive Measures* (Webb et al. 1966) made the case that, given the inescapable limitations of “reactive” methods, it is folly to proceed “simply by asking,” whether this takes the form of participant observation or structured interviews, surveys or experimental interventions. They detail a range of interactive dynamics and interviewer or intervention effects that arise from the ways in which subjects manage their self-presentation in response to what they perceive as the expectations of a research setting, compromising the internal and external validity of standard research methods in the social sciences (Webb et al. 1966: Chapter 1). The only way forward is to engage the resources of multiple methods, including

underdeveloped strategies for using inadvertently produced physical traces as evidence of patterns of action, preference, and intention that survey respondents or interviewees might not themselves be aware of, or might be disinclined to disclose (Webb et al. 1966: 3, 34). In a vigorous renewal of the case for “revalorizing sources marginalized by dominant social science,” Lee emphasizes the value of “ephemeral traces” of movement and interaction in a social environment – physical erosion or accretion, the litter discarded, the “performative opportunities” afforded by objects – as “caches of data” that make possible strategies of triangulation (Webb et al. 1966: XX). The principle here is that evidence from very different sources, in this case archival and material, should be mobilized as an independent basis for assessing the results of reactive methods (Lee 2000: 1, 8, 14). The Tucson “Garbage Project,” initiated by archaeologist William Rathje in the early 1970s and later expanded internationally, embodies a similar rationale; the systematic analysis of what we throw away, recovered from curbside garbage collection and through the excavation of landfill sites, often reveals patterns of consumption that stand in stark contrast with the results of surveys that depend on self-reports (Rathje and Murphy 1992).

In all these areas, then, an enthusiasm for the capacity of material things and physical traces to function as evidence reflects an appreciation of their stubborn concreteness, the “brute intransigence of matter,” as Daston puts it (2008 [2004]: 11), by virtue of which they are sometimes seen as impartial witnesses to the past, bearing marks of their makers, their various uses, and the shifting configurations of meaning and action in which they have been implicated in the course of their travels. However enigmatic they may be, this “bony materiality” sustains a certain epistemic optimism, even when the more naïve aspects of a “positivist historiography of facts” have been abandoned (Daston 2008: 15-16). But for all this, a recurrent theme in the literature valorizing objects as subject and source is that there has been too little attention to things themselves. This concern figures prominently in reflection on

formative examples of object studies dating to the 1990s. Gell rejected sociological and iconographic alternatives to the “aesthetic preoccupations” of then-contemporary anthropology of art on grounds that they effectively ignore “the art object itself”; they “look...only at the power [of the object] to mark distinctions” or treat it as a “species of writing” and consider only its symbolic meaning (1992: 43).⁵ Similarly, Corn took aim at “object myths” in the history of technology. Reviewing publications through the 1990s he found that, despite a rhetoric privileging the object, a substantial majority of historians of technical artifacts relied exclusively on textual or oral sources; fewer than a third had undertaken object-based analysis and typically only for periods or contexts for which textual sources are not available (1996: 37). Although this has certainly changed, even historians who are committed to the study of material culture lament the fact that “we observe the march of images into the historian’s study, but physical objects are mostly kept at arm’s length”; material culture may be a subject of study, but material things and traces are not still not “among the standard resources of academic research in the humanities and social sciences” (Sibum in Auschlander et al. 2009: 1384). This is evident in the *Object Reader* and *Things that Talk*; text-based analysis of the social and symbolic significance of objects continues to dominate the study of material culture, both as a subject in its own right and as a resource for social and historical inquiry of other sorts. As Simon Werrett argues in his contribution to this volume, even though historians of science have largely abandoned intellectual history and cultivated an interest in the material culture of science, “they have done so with a relatively limited disciplinary repertoire”; they tend to proceed by “reading about things rather than engaging with them directly” (Chapter 19, PG).

This disconnection between intent and practice should come as no surprise. The challenges of working with material evidence are legendary, and the prejudices of the literate continue to favor the seeming transparency of textual records and the direct

testimony of subjects.⁶ They are also reinforced by traditions of disciplinary specialization that set the human, social sciences apart from the biological and physical, reproducing a divide not unlike like that lamented by C. P. Snow in the late 1950s (1963 [1959]). To make effective use of physical traces and objects as evidence requires expertise in an enormous range of fields, most of them not ones in which humanists and social scientists have any training. Consider, for example, Marc Bloch's reflection on the demands of research in historical ecology:

Now, if almost any important human problem...demands the handling of diverse types of evidence....the types of evidence necessarily mark off the several branches of technical scholarship. The apprenticeship for each is long, but full mastery demands still longer and almost constant practice. For example, very few scholars can boast that they are equally well equipped to read critically a medieval charter, to explain correctly the etymology of place-names, to date unerringly the ruins of dwellings of the prehistoric, Celtic, or Gallo-Roman periods, and to analyze the plant life proper to a pasture, a field, or a moor. Without all these, however, how could one pretend to describe the history of land use? (Bloch 1953: 68).⁷

Add to this catalogue of necessary expertise the resources of material science and physical geology, art historical interpretation and ethnohistory, the sciences of cognition and of biomedicine, all fields required to understand the cultural context and human implications of land use patterns, and the complexity of the enterprise is apparent. The gulf between these various fields is not always characterized by the stark incomprehension that concerned Snow, but the range of expertise required to ground material culture studies in rigorous analysis of the material – to meet MacGregor's demand that objects be "interrogated and interpreted as deeply and rigorously as the written report (2010: xvi) – is daunting in its variety. As the editor

moderating the *American Historical Review* discussion observes, there are a great many questions that “strike at the heart of methodological concerns we don’t often discuss” (Auschlander et al. 2009: 1355); “to take cues from the objects themselves,” historians will have to “work hard to develop methods in order to make speak the silent representatives of the past” (Sibum in Auschlander et al. 2009: 1359).

THE WISDOM OF PRACTICE

It is primarily archaeologists who have taken on these challenges, and yet they figure very little in the object studies literature. Since the early nineteenth century they have built up a repertoire of research strategies specifically designed to mobilize the evidence of human lives and events that survives in an enormous range of material evidence, from garbage dumps to monuments, from the physical traces of single events to the palimpsest of evidence that bears witness to long-term, large scale cultural processes, from finely crafted artifacts rich with cultural significance to the inadvertent traces left by human activity on a continent-wide scale and in the minutiae of domestic consumption. In the process they have decisively enlarged, challenged, and reconfigured what we know about the past, putting material evidence to work in the investigation of a great many different aspects of the cultural past. The tempo and process of development varies by region, but from the early nineteenth to the mid-twentieth centuries archaeologists built relative and absolute chronologies and made use of material evidence as a proxy for cultural affinity, focusing on artefacts in settlement and burial contexts, and on monumental architecture. By the mid-twentieth century in many areas these local histories of material culture had been integrated into regional and inter-regional typologies and, with growing attention to botanical and faunal remains, they delineated distinct ecological and subsistence regimes and major cultural transitions. The ecological contexts of people’s everyday lives and subsistence practices became the subject of pioneering studies in Scandinavian and British archaeologies from the 1930s and were influential in the Americas in the 1950s, providing the basis for refined models

of regional settlement and subsistence patterns of the kind developed by Wiley in the Viru valley, Peru (1953) and by McNeish in the Sierra de Tamaulipas, Mexico (1958). The processual archaeologists of the 1960s and 1970s built on such studies by analyzing the underlying adaptive dynamics of cultural systems and constructing large scale, long-term systemic models of their social, political and economic structures (see examples in Binford and Binford 1968). Through the 1980s, in reaction against this system-level eco-materialism, postprocessual archaeologists developed a program of material culture studies focused on symbolic meaning and agency (e.g., Hodder 1982). Archaeologists had long been interested in social identities; for example, in the form of enclaves like the Oaxacan barrio identified by Millon in the city of Teotihuacan (1973) and, from the early 1990s, gender, race and ethnic identity (e.g., Gilchrist 1999; Battle-Batiste 2011; Leach et al. 2009). In developments in the last two decades that parallel the object studies literature, there has been much focus on the materiality of objects marked by debate about what distinguishes materiality from materials (e.g., Ingold 2007; Knappett 2012), and now on objects as 'things' and their entanglements with the social, the ideational, and diverse forms of agency (e.g., Olsen 2010; Hodder 2012).

The question that animates this collection of essays is, then: how do archaeologists do this? How do they make effective use of physical traces and objects as repositories of evidence? There is a great deal of wisdom embodied in the practices by which archaeologists assemble, analyze, integrate and adjudicate physical evidence but all too often this wisdom has been obscured by sharply polarized internal debate about the limitations of material evidence. There have always been epistemic pessimists who insist that, insofar as claims about the past overreach the available evidence and the evidence itself is an interpretive construct, the conclusions that archaeologists draw can be nothing more than speculation. In an especially strong statement of this position in the mid-1950s M. A. Smith insisted that

there is no “necessary link” between the surviving record and “the human activities we should like to know about”; the inference from one to the other is, therefore, “a hopeless task” (1955: 3-4). Similar concerns have surfaced repeatedly in Anglo-American archaeology since the early twentieth century (see Wylie 2002: Part I) but, just as regularly, they have been rebutted by epistemic optimists who insist that the limitations lie not in the material record itself but in the intellectual and material resources archaeologists bring to bear in recovering and interpreting it. The advocates of processual archaeology in the 1960s and 1970s were particularly adamant that, if archaeologists adopted a properly scientific hypothesis-testing methodology they could meet the gold standard of deductive certainty they associated with rigorous scientific inquiry (Binford 1962, 1972; Watson et al. 1971). Postprocessual critics were quick to object that archaeological evidence is as much a construct, therefore as insecure a ground for testing as for building hypotheses about the cultural past and some insisted, on these grounds, that relativist conclusions are inescapable: “there is literally nothing independent of theory or propositions to test against” (Shanks and Tilley 1987: 111); archaeologists should candidly acknowledge that they simply “create facts” (Hodder 1983: 6). The upshot, as Killick describes it, was that the “stories archaeologists tell are no more than politically charged fables imposed from the present onto the past” (5). Few held such extreme positions for long; although intransigent in their rejection of the deductivist ideals of the New Archaeology, postprocessual critics nonetheless acknowledged that archaeological data have an impressive capacity to “resist theoretical appropriation” (Shanks and Tilley 1989: 44), and New Archaeologists made it a priority to build a robust, empirically grounded body of “middle range theory” to underpin the interpretation of surviving material traces as evidence. Although the “theory wars” have largely died down in recent years (Johnson 2010: 220-3), the epistemic anxieties and ambitions that gave them impetus continue to structure archaeological thinking. For example, in his contribution to this volume, Gavin Lucas traces the contrasting legacies of

metaphors for the archaeological record that embody, on one hand, a “negative epistemology” – the archaeological record as “fragment,” incomplete and compromised – and, on the other, a more hopeful emphasis on survival and continuity into the present associated with the metaphor of “relic.”

For the most part debate about the epistemic status of archaeological evidence has been conducted at a level of abstraction that provides little useful guidance for practice. And although there are a great many handbooks on the practicalities of field work and techniques of analysis, archaeologists have published comparatively little that captures the underlying rationale for specific forms of practice, in the mid-space between “how to” guides and philosophical generalities. What is needed, we suggest, is resolutely case-based analysis of actual practice – key instances of exemplary practice, critical turning points, innovations, and instructive failures in the use of archaeological data as evidence – aimed at making explicit the norms of evidential reasoning that have taken shape in the context of evolving traditions of practical experience working with archaeological material. This is the goal we had in view when we took up the project of assembling this collection of essays; we draw inspiration from David Clarke’s brief for an internal philosophy of science that addresses the challenges faced by archaeologists, rather than imposing models developed to make sense of physics (1973), and we see it as an extension of Ian Hodder’s call for systematic reflection on the dynamics of “archaeological process” (1999).

To this end we asked each contributor to identify a particular aspect of evidential reasoning with which they grapple and to consider, with reference to concrete examples in their area of specialization, how they construct evidential claims, critically assess them, and bring them to bear on pivotal questions about the cultural past. We asked, more specifically, that they identify best practices and draw out

lessons learned, both positive and negative, as a basis for articulating constructive guidelines for practice. The structure of the volume reflects key junctures at which questions about evidential reasoning arise in archaeological practice, as well as highlighting cross-cutting issues with which practitioners grapple in a wide range of fields, wherever they confront the challenge of “building scientific knowledge in the absence of infallible foundations,” as Hasok Chang puts in a philosophical history of chemistry (2004: 234). We outline first the rationale for the overall structure of the volume, and then trace cross-cutting themes that knit it together.

WHAT FOLLOWS: OVERALL STRUCTURE

We begin with a set of essays on “Fieldwork and Recording Conventions” (Part 1). In the first of these, Richard Bradley makes explicit the insight that frames this section and much that follows: established norms of ‘good’ excavation and survey practice, and conventions of rigorous recording and documentation, were designed to answer specific questions which, in turn, presuppose a rich array of assumptions about the nature of the subject domain, about what is puzzling or interesting and, crucially, what can feasibly be asked about it. They are purpose-built creatures of context that have become standardized and persist in training and practice long after the original questions are forgotten. Critical histories of archaeological practice recover these starting points and reveal how deeply they configure subsequent research: Bradley traces the legacy of Pitt Rivers’ now canonical nineteenth century excavations at Cranborne Chase; Anna Booser scrutinizes the shaky foundations of a typology of domestic architecture, lost to collective consciousness, that underpins contemporary descriptions and comparisons of house forms in Roman Egyptian archaeology; Manning reflects on the legacy of a chronology flawed by “a poorly based best guess” in Late Bronze Age Aegean archaeology (p. 23); and Martin Bell warns against the risks of “xeroxing” in which new research simply reproduces established results. The central question here is, to what extent do original purposes constrain the

potential for addressing new questions and putting old evidence to work? Does the process of selection and of interpretation “at the trowel’s edge” (Hodder 1997) entail that archaeologists will recognize, collect, record, and categorize material in ways that answer to their expectations, as the most intransigent epistemic pessimists have suspected? In fact, while the cases considered in this section illustrate how deeply entrenched the conventions of past practice can be, the stubborn intransigence of material evidence is also clearly on view, as remarked upon by Shanks and Tilley (1989: 44), and they draw attention to a range of ways in which practitioners can, and do, effectively come to terms with these conventions. Bell details empirical experimental strategies for recognizing and counteracting “the influence of pre-understandings” (p. 13); Shahina Farid assesses the successes and challenges of the reflexive, interactive pluralism instituted by Hodder at Çatalhöyük; Bradley and Boozer both illustrate the value of critical histories designed to interrogate the limitations of past practice, the better to build on it; and, turning to the impact of computing technologies on field recording, Mike Rains reflects on the variable impact of new computer technologies on field recording and data management practices, a current and ongoing process that throws into relief the need to tune new technologies to the evolving goals of inquiry as well as to existing working practices.

The essays that comprise the next section, “Cross-field Trade” (Part 2), address the complex web of interdisciplinary engagements by which archaeologists meet the challenges outlined by Bloch. Archaeology is resolutely a ‘trading zone’; throughout its history its horizons as a discipline have been expanded dramatically and continuously by importing expertise and technologies developed elsewhere for application to archaeological problems of data recovery and analysis. At the same time, this import trade has been bedeviled by miscommunication and unrealistic expectations about what these external resources can deliver. The result is a recurrent boom and bust lifecycle of enthusiastic early adoption followed by

frustration and either premature abandonment or a long difficult process of “settling in”: retooling external resources for archaeological purposes. The contributors to this section offer jointly historical and pragmatic assessments of how this process has unfolded in a number of key areas of engagement. Three consider the ongoing trade with high profile physical sciences: Sturt Manning traces the fortunes of radiocarbon dating through three revolutions; David Killick juxtaposes with this the challenges of realizing comparable precision and accuracy in other physical dating methods, and refining stable isotope analysis as a tool for dietary reconstruction; and Mark Pollard and Peter Bray reassess archaeometry at a point where technical refinement has destabilized the original questions about provenance that set the field in motion in the nineteenth century. Marcos Llobera echoes many elements of their retrospective assessments of long-standing cross-field engagements, now with respect to newly emerging digital simulation techniques: it is crucial, he argues, to cultivate a sophisticated understanding of what these tools can (and cannot) do, to keep archaeological questions clearly in focus, and to purpose-build applications that address these questions rather than allowing tools designed for other purposes to dictate the archaeological agenda. In a telling counterpoint to these examples of the impact of new technoscience resources, Sara Perry considers the long history of visualization drawing attention to the pivotal but often discounted role played by professional illustrators in archaeology; their seemingly prosaic practice requires a high order of skill that embodies a misrecognized genre of “epistemic productivity” (p. 4). Taken together these essays bring into sharp focus a number of factors that determine the viability of cross-field or external expertise: institutional and funding structures, as well as internal hierarchies of expertise and labor, that constrain or enable effective transfers and collaborations; training that puts archaeologists in a position to adjudicate external expertise and successfully tune it to archaeological purposes; and a keen reflective appreciation of how the research agenda of the discipline has been both served and circumscribed by cross-field trade.

It is by now a truism that archaeologists almost never depend on a single line of evidential reasoning; as the histories outlined in Part 2 illustrate, they make use of a wide range of distinct lines of evidence to assess and to calibrate even the most robustly grounded scientific evidential claims. Crucially, as the advocates of “unobtrusive measures” in sociology appreciate, the value of mobilizing multiple lines of evidence lies in their capacity to be mutually constraining as well as mutually reinforcing; triangulation raises the epistemic credibility of the claims they support insofar as the independence between distinct lines of evidence counteracts the risk of error in any one of them. The contributors to Part 3, “Multiple Working Hypotheses,” illustrate various strategies by which archaeologists make an epistemic virtue out of what often seems a liability, bringing disparate fragmentary lines of evidence into play in the construction and adjudication of interpretive claims of various kinds and scales. Amy Bogaard takes up themes articulated by Bradley and Bell, showing how an open-ended, iterative process of experimental testing that operates on a number of fronts at once – archaeological, ethnobotanical, ecological – makes possible a fine-grained comparison of alternative models of early Neolithic farming practice. In a complement to Manning’s essay, Alex Bayliss and Alasdair Whittle illustrate how a Bayesian modeling approach enables them to systematically integrate multiple lines of evidence and rigorously assess margins of error in physical dating – the third radiocarbon revolution – in some cases providing temporal resolution that brings human-scale action and events into sharp focus. In two essays on archaeology and the law, an archaeologist Thomas, and legal scholars Anderson and Twining make a case for applying to archaeological problems a system for schematically representing and assessing “mixed masses of evidence” that was developed in the context of early twentieth century legal theory by John Henry Wigmore. Finally, expanding the frame of interpretation, Nicholas and Markey consider a diversity of ways in which lines of evidence originating in indigenous oral

traditions can be brought into productive engagement with archaeological research. It is the friction between diverse types of expertise and epistemic standpoints as well as between independent lines of evidence that that is productive in these cases.

In the final section, “Broader Perspectives: Material Culture as Object and Evidence (Part 4), we close with three essays that take up directly the questions that animate the volume as a whole: about the presuppositions that configure archaeological practice, and what this practice has to offer the range of other fields in which there is growing interest in making use of material evidence. Gavin Lucas scrutinizes the ontological assumptions that underpin opposing, dominant conceptions of the archaeological record; he asks what this “record” is evidence *of*, and urges us to “re-imagine” material traces as surviving elements of enduring assemblages that integrate material with practice, and social worlds with agency of diverse kinds. Extending this reflection on agency Andrew Jones articulates a parallel shift in the framework assumptions that set the terms of archaeological engagement, one that closely tracks the arguments now dominant in material culture studies for taking material things seriously in their own terms. Rather than assume that archaeological materials are ‘mute’ and ‘inert’ until brought to life by our intervention – animated or induced to “talk”, to use Daston’s metaphor – Jones explores the potential of an ‘intra-active’ approach, inspired by Karen Barad’s agential realism (2007), in the context of a study of late prehistoric rock art; his practice is predicated on a recognition of and engagement with the distinctive “agentive potentiality of objects, things or materials” (p. 14). In the final essay of the volume, Simon Werrett reverses the direction of inquiry and asks what historians of science might learn from archaeological practice. His account of how material evidence has figured in historical inquiry sharpens the ambivalence Daston notes, and his response to this ambivalence sets against “the fragility of material evidence” (p. 17) an appreciation of how archaeological analyses of, experimentation on, and modelling of material

objects can enlarge the “disciplinary repertoire” of fields like the history of science in which there is, increasingly, an acute sense of their relevance, as subjects, protagonists and crucial resources for inquiry.

CROSS-CUTTING THEMES: GETTING THINGS TO TALK IN ARCHAEOLOGY

The sectional divisions we have outlined are, in their nature, somewhat arbitrary; there are all kinds of overlaps in the types of cases considered, the focal issues addressed, and the lessons drawn. Reading across the grain all the contributors grapple, in one way or another, with what Boozer identifies as a paradox at the heart of archaeological practice (p. 2), and in this they take up the threads of the internal debate about the limitations of material evidence. She describes material evidence as tangible in terms that resonate with Daston’s appraisal of material evidence; it can surprise, challenge settled assumptions in ways that reinforce the sense, reported by Daston, that it stands as an impartial witness to what happened in its past, but at the same time it is radically ambiguous, its significance as evidence a function of sedimented layers of interpretation that can easily reproduce and amplify error. There is no escape from what Bell describes as “pre-understandings” (after Hodder 1999) and these configure not just the interpretive or explanatory claims that are the focus of inquiry, but what will count as evidence in building or adjudicating them. There is always the possibility that, as Thomas puts it, the assumptions that guide inquiry will “blind you to evidence that might lead in a different direction” (p. 5), and they can do this in insidious ways. They may take the form of “value judgments masquerading as factual propositions” operating well below the threshold of conscious awareness (Anderson and Twining p. 8); they are embedded in conventions of representation, as Perry, Llobera, and Rains illustrate in quite different ways (recording forms, database structure, conventions of visualization); they are internalized through long training and embodied in skilled empirical practice, in archaeology (Bell) as in the sciences generally (Werrett); and as Farid shows, taking up Hodder’s “trowels’ edge”

argument, they are often built into the fabric archaeological practice in the form of conventional divisions of labor and work discipline.

The upshot, as Bradley encapsulates this line of thinking, is that the kind of “complete objectivity” – in the sense of freedom from interpretive presuppositions – that he finds associated with the technical virtuosity of General Pitt Rivers and endorsed by Philip Barker (Bradley p. 4) is, in principle, unrealizable. It stands in stark contrast to the messy realities of the bootstrapping and scaffolding practices by which archaeologists necessarily rely on shaky background assumptions to get inquiry off the ground, and then wrestle with the constraints that these tentative foundations impose on subsequent research; reliance on pre-understandings is an inescapable “condition of possibility” for archaeology (Jones p. 3). But rather than take these pre-understandings to be all of a kind and equally insecure, as Smith had done and as suggested (briefly) by some of the more intemperate post-processual critiques, a great many different kinds of presupposition figure in examples that follow and it’s clear that they vary widely in the degree to which they are credible, in themselves and in archaeological application. As Anderson and Twining describe the background assumptions that underpin evidence-based inferences in legal arguments, they can include everything from “well grounded science [and] evolving science [to] dubious, contested or junk science”; “practical expertise” and “everyday experience”; robust generalizations and one-off anecdotes; common sense, hearsay, “speculation ... faith ... prejudice” (p. 8). There are archaeological counterparts to all of these identified in the case studies presented here: the robust background knowledge drawn from the physical sciences discussed by Pollard and Bray, Manning, Bayliss and Whittle, and Killick that, nonetheless, requires a great deal of scaffolding to be useful in archaeological contexts; the emerging information technologies appraised by Rains and by Llobera; the embodied expertise of professional excavators and illustrators discussed by Farid and by Perry; the rich but

often discounted oral traditions of indigenous descendant communities highlighted by Nicholas and Markey and the contrasting culture-wide conventions of Western thought, like the “distinction between nature and culture” discussed by Bradley and by Werrett; and the empirical, archaeology-specific pre-understandings drawn from type sites and regional sequences that are the anchors for regional typologies and chronologies of the kinds discussed by Boozer, Bell, and Manning.

As the details of the case studies make clear, however, archaeologists do routinely call out presuppositions of these various kinds when they have become ingrained in practice and hold them accountable, conceptually and empirically. In fact, despite bringing the uncertainties of evidential reasoning into sharp focus, there are a number of striking narratives of success outlined here: chronologies calibrated, typologies reconfigured, focal questions reframed, specification of the range of application of “middle range theory,” effective tuning of new technologies to archaeological problems, and nuanced adjudication of the claims for and against seemingly undecidable alternative working hypotheses. It is also clear that, to do this – to build, and continuously rebuild, credible background knowledge in the range of areas required to sustain archaeological reasoning from material evidence – requires a repertoire of critical and constructive methods that are as diverse as the presuppositions on which archaeologists rely. Here is a short list of strategies by which this is accomplished that stand out for us as editors in the accounts that follow.

First, and most obviously, there are all the strategies by which archaeologists generate new data: as Bell puts this point, it ought not be so difficult to “escape the tyranny of background knowledge and pre-understandings....the discipline is, after all, refreshed by a constant stream of new discoveries” (p. 18). Of course, while some empirical discoveries arise unbidden and unanticipated, even they are consequential only given a prepared mind, and just as often striking new empirical

discoveries depend on deliberate innovation – of questions, of search space, of technical tools that bring new empirical data on stream. The long process of reconciling discordant chronologies detailed by Manning, and the dramatic reframing of settled assumptions about cultural sequence and associations that results, as illustrated by Bayliss and Whittle as well as Manning, reinforces the point that transformative empirical discoveries depend heavily on conceptual scaffolding. So too the process of coming to terms with the compositional complexity of metal objects outlined by Pollard and Bray where the weight of hard-won evidence unsettles the assumptions that inform a “narrowly-defined scientific approach”; they describe a growing recognition that repeated melting, mixing and repurposing is not the exception but the norm and that the chemical components of these objects are themselves dynamic which, in turn, prompts a fundamental rethinking of what it is that archaeometallurgists study: “not merely a set of material processes but also the human decisions and structures that surround them” (p. 17). The data force the point, but they come into view and are salient only given the evolving scaffolding of jointly physical and social scientific background knowledge that Pollard and Bray describe.

This means that the action, where evidential reasoning is concerned, is largely off-stage: it is not so much a matter of discovering new data, as important as this can be, but of painstakingly building and continuously reworking the necessary background knowledge in an ongoing iterative process, much like what Chang describes for chemistry where, he insists, there are no fixed, self-warranting foundations (2004: 228-231). The most direct ways of doing this involve experimental and “practice-based” research (Bell p. 19) designed to test assumptions about practices and processes that produce, or that could have produced specific types of archaeological trace. Long-term studies of the erosion of earthworks (Bell), “practical experimentation” with quartz assemblages (Jones, p. 10), the chemical analysis of metal decay and transformation just described (Pollard and Bray), sharply focused

testing of ethnographic analogies (Killick), and experimentation with crop yield and weed profiles under different farming scenarios (Bogaard) are all examples presented here of approaches that are now a staple of archaeological practice. Crucially, as Bell argues, this is not typically a process of establishing uniformitarian principles that can secure deductive certainty; it is, rather, a matter of specifying with as much precision as possible the conditions under which linking principles are likely to hold in specific contexts of application. The aim, Manning argues with reference to the third radiocarbon revolution, is to replace the “web of a priori assumptions” that underpins conventional chronologies, not with the certainty of an imagined silver bullet, but with systematic modelling of error (p. 23), a practice that Bayliss and Whittle illustrate in a series of cases ranging from highly local human-scale occupations to large scale landscapes and millennial timescapes. In addition, several contributors make the case for recognizing what might be described as indirect evidence generated by experimenting with models themselves: for example, the recognition of gestural knowledge as constitutive of experimental practice that arises from the reenactments by historians of science that Werrett describes (p. 7); the sensitivity analyses Bayliss and Whittle use to assess alternative chronological models (p. 20); the distinctive interpretive insights that arise from visualizations described by Perry (p. 16) and by Farid (p. 16); and the methods of simulation that, Llobera argues, make it possible to explore “basic properties of the study area,” extending and contextualizing field observation (p. 18).

When material evidence is put to work building, assessing, testing and refining the models and hypotheses that are the focus of inquiry, as indicated earlier it is almost invariably in the context of a complex web of evidential reasoning. The contributors identify several different ways in which and purposes for which multiple lines of evidence are mobilized. For example: Bogaard makes the case for developing proxies for the farming practices she studies that are based on completely different

datasets (p. 12); the impact of bringing new archaeological comparanda to bear figures in Bradley's and Lucas' discussions of the circumstances that made it possible to recognize timber structures in the archaeological record, and is the motivation for Boozer's insistence on the need to reassess architectural typologies in Roman Egyptian archaeology; Killick emphasizes the capacity for purpose-built lines of material science-based evidence to provide an independent test of interpretive assumptions that derive from sources that have dominated interpretation, like ethnography or common sense (e.g., about dependence on maize or the labor requirements of earthwork construction); and Bayliss and Whittle advocate a "pragmatic" Bayesian approach precisely because it is "a formal and explicit methodology for weaving together different strands of evidence" (p. 8). As different as these cases are, they share a common rationale for expanding the range of evidence: to exploit the potential for independent lines of evidence to constrain one another, counteracting the risks of "xeroxing" (Bell), and identifying error in individual lines of evidence that might not otherwise be recognized, as well as for enriching interpretation, bringing previously inscrutable aspects of the past into view.

A recurrent theme emphasized by a number of contributors is the need for rigorous transparency about the constituents of these webs of inference and the "analytical scaffolding" (Boozer p. 18) that underpins them. This is a particular virtue of Bayesian modeling – "it force[s] archaeologists to be explicit about the strands of their reasoning" (Bayliss and Whittle, p. 8) – and it is the reason why Thomas, and Anderson and Twining, advocate the application to archaeological problems of a modified Wigmore method for schematizing the empirical and inferential components of evidential reasoning. It is only possible to build on and refine tentative foundations in an ongoing, iterative process if the bases for earlier inferences are kept clearly in view. Citing Gero (2007), Boozer urges a commitment to "honor ambiguity" rather than smoothing, cleaning or otherwise suppressing the messiness and uncertainty of

the descriptive and interpretive claims that become the basis for subsequent reasoning with archaeological data. By extension, it is crucial to resist the kind of canalization of thinking that comes from investment in a seemingly a “best” hypothesis. In this spirit, Bayliss and Whittle warn against the potential of provisional models to “fossilise” and “become received wisdom” (p. 18), Nicholas and Markey note the risks of “intellectual complacency” that come with considering only the range of evidence and hypotheses that have become familiar staples of disciplinary inquiry, and Killick warns against a “‘good enough’ attitude towards scientific evidence” (p. 18). They join Bogaard, among others, in stressing the importance of keeping multiple working hypotheses in play even while working to reduce the range of viable options by elimination testing. Model and hypothesis evaluation is typically comparative; the outcome can only be a (tentative) judgment about the relative strengths of the alternatives considered, which carries with it the risk that an “inference to the best explanation” will endorse the best of a set of limited set of possibilities. It is crucial, Bogaard argues, to guard against “propensity to infer that the ‘best’ explanation at any given moment is true”; it may only be “best” given a failure to conceive of relevant other possibilities (16).

One way to realize transparency about underpinnings and alternatives is to put critical historiography to work as a way of taking distance from entrenched conventions and mobilizing the wisdom of hindsight. Bradley urges “a kind of historiography” (p. 4) in the context of reengaging legacy data that involves, not just returning to the data and records generated by past excavations, but situating them in a broader motivational, social and political context. This holds the potential, he argues, for realizing a degree of “repeatability”: rerunning the process of interpretation in light of new archaeological comparanda and with an awareness of how broad cultural sensibilities have shifted. It puts him in a position to recognize the cultural significance of geological features that had been dismissed as natural. In this

spirit a number of other contributors develop genealogies of key concepts or modes of practice that configure contemporary archaeology: Lucas scrutinizes the legacy, in our understanding of “the archaeological record,” of metaphors of survival (relic) and fragmentation (ruin) rooted in nineteenth century traditions of Romanticism and antiquarianism; Perry considers the institutional and disciplinary history of archaeological illustration; those who discuss archaeological science trace the tangled histories of “cross-field trade.”

Another way to promote transparency is to cultivate pluralism of a number of different kinds. The commitment to make use of the full “toolkit of available approaches” (Bell, p. 1) is not just a matter of making full use of available resources; extending this maxim, Farid highlights how the friction between specialisms, various forms of expertise, and standpoints can be enormously productive in mobilizing critical and constructive insights no one brought to the table in the first instance. She and Perry argue for recognizing diverse forms of “epistemic productivity” (Perry p. 10), including forms of expertise that are marginalized within archaeology, while Nicholas and Markey illustrate the value of taking seriously external resources that have selectively been read out of consideration: “even contradictory lines of evidence have value,” throwing into relief what has come to be “common sense” for archaeologists and opening up interpretive and explanatory options. A key advantage of Wigmore schemas as described by Anderson and Twining is that they require clarification of the standpoint from which an evidential argument has been developed, so that it can be evaluated from multiple perspectives. A pluralism of standpoints is a key resource for generating a repertoire of multiple working hypotheses that have the potential to counter the worries Bogaard raises about unconceived alternatives. This is reflexivity made concrete. It is not primarily a matter of introspection; we are often the last to be aware of the taken-for-granted that inform our own practice. Rather, it is a form of (collective) self-appraisal that depends on putting second-order empirical methods of

analysis to work – historical and genealogical, comparative and contextualizing – to bring into focus pre-understandings embodied in specific questions and contexts of inquiry now forgotten or internalized as encompassing disciplinary or cultural assumptions.

The reflexive appraisals of evidential reasoning presented here have a number of implications for orienting ideals and goals, and for the pragmatics of practice. They suggest, for example, the need to rethink the epistemic ideals of that inform programmatic debate and practice. Rather than take “objectivity” to be self-evidently a matter of transcending context or insulating against the influence of pre-understandings, the argument emerging here suggests the need to unpack the specific epistemic virtues gathered together (uneasily) under the aegis of this term, and consider in concrete terms how they are best realized in particular contexts of practice, for particular purposes. A resolutely pragmatic, proceduralist conception of objectivity is taking shape in these contributions that calls for a more nuanced and self-consciously situated account of epistemic success than the deductivism of a vernacular positivism that has set the terms of the sharply polarized debate between processual and post-processual archaeologists.

This reassessment extends as well to the practicalities of archaeological inquiry: how archaeologists are trained and how research is organized and funded. Killick, and Pollard and Bray particularly emphasize the need for training designed to cultivate the kind of scientific literacy necessary for archaeologists to make effective, well-informed use of scientific analyses of archaeological material. The perils of treating scientific analysis of archaeological material as a “self-service buffet” (Pollard and Bray, p. 2), appropriating the results of poorly understood specialist techniques without a clear sense of archaeological purpose, is reiterated with reference to digital technologies by Llobera and by Rains. The central message here is that the silos of

specialism that set disciplinary archaeology apart from the myriad other fields on which it depends constitute a structural constraint that archaeologists must continuously struggle to overcome. Perry and Farid make a similar point with respect to hierarchies of expertise within archaeology. The success of evidential reasoning depends on establishing institutional and funding structures, work processes and channels of communication that can sustain “productive and trustworthy forms of cooperation” (Killick p. 3) between archaeologists and those with whom they trade, internally and externally.

This is to reaffirm the point made by Bloch in the 1950s, articulated here with attention to practicalities which make it clear that effective engagement with things will not be accomplished by tooling up new expertise within discrete fields. The successes working with material evidence that have been realized by archaeologists depend in large measure on creating archaeology as a dynamic trading zone (Galison 2010), building an expansive network of technical, empirical, theoretical exchange underpinned by the kind of “diplomatic tradition” Nicholas and Markey describe with reference to the Haudenosaunee. It is to be hoped that the passion for things now manifest in a growing range of neighbouring fields may be an opening for extending this network.

ENDNOTES

¹ While Myers (2001) identifies material culture studies as taking shape since the 1990s, Miller describes entering a field already formed in the early 1980s (2005: 4). The impetus was, in any case, a critique of the presumption that objects are static media in which social meaning is reflected, “inert and mute, set in motion and animated, indeed knowable, only by persons and their words...divorced from the capacity of persons to act and the power of words to communicate” (Appadurai 1986: 4).

² At an extreme of animating objects and attending to “things themselves,” the arguments for embracing distributed, hybrid forms of agency – the “vital materialism” of Bennett’s *Vibrant Matter* (2010; Haraway’s cyborgs and Latour’s hybrids – resonate with the case made by recent advocates of object ethnography for refusing Cartesian dichotomies altogether; mind and matter, meaning and material are inseparable, the locus of proliferating ontologies (Henare et al. 2007: 3).

³ In addition to E. P. Thompson, this group included Christopher Hill, Rodney Hilton, and Eric Hobsbawm, among others.

⁴ This collaboration was not a happy or successful experience. Although the excavations at Upton made a contribution to archaeological research on deserted medieval villages, Hilton found the fieldwork “rather tedious” (Rahtz 2001: 91). There was no integration of historical texts and archaeological evidence in the publication (Hilton and Rahtz 1966).

⁵ Jones extends this objection in his contribution to this volume, finding in Gell an example of the limiting conception of ‘material agency’ that he means to push beyond. Although Gell advocates closer attention to material objects, on Jones’ account he nonetheless treats them as “a derived form of agency,” reproducing Cartesian dichotomies that reduce their “agentive potentialities”; even though art objects exist in a nexus of relations, in the end they “stand in for” human relations and intentions (INTERNAL REF).

⁶ Sibum goes on to observe that there continues to be a systematic bias in favor of text-based scholarship; the study of literary sources is recognized as historical “scholarship” while close study of material objects is demeaned as a form of

connoisseurship (Auschlander et al. 2009: 1384).

⁷ We were alerted to this passage of Bloch's *The Historian's Craft* by Carole Crumley who quotes it, in her own translation, in the Forward to *Advances in Historical Ecology* (1998: ix).

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